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OF THE

American Philosophical Society

HELD AT PHILADELPHIA

FOR

PROMOTING USEFUL KNOWLEDGE

VOLUME LIII

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OF THE

AMERICAN PHILOSOPHICAL SOCIETY

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VOL. LIII | JANUARY–MAY, 1914 | No. 213

ON "PSYCHOLOGY AS THE BEHAVIORIST VIEWS IT."

BY E. B. TITCHENER.

(*Read April 3, 1914.*)

When we speak of a science, we have in mind a logically organized body of knowledge that has resulted from certain methods of attacking the problems presented by a particular subject-matter. The methods of science are all, in the last resort, observational; the problems of science are all, in the last resort, analytical. The subject-matter of a given science may be indicated in two different ways: by a simple enumeration of objects, or by a characterization of the point of view from which the science in question regards the common subject-matter of all science, namely, human experience. Thus we may say that our psychology will deal with such things as perceptions, feelings, thoughts, or we may say that psychology, dealing "in some sort with the whole of experience," is to be distinguished as "individualistic" from other sciences which are "universalistic." It is clear that a characterization of this kind, though it necessarily transcends the limits of the science in order to show how those limits are drawn, is far more satisfactory than a mere list of objects; and psychology, these many years past, has therefore had recourse to it.[1]

[1] J. Ward, "Psychology," Encyc. Brit., XX., 1886, 38 (and later); R. Avenarius, "Bemerkungen zum Begriff des Gegenstandes der Psychologie," *Vjs. f. wiss. Phil.*, XVIII., 1894, 418; H. Ebbinghaus, "Grundzüge der Psych.," I., 1897, 8 (and later editions). On the general subject, cf. E. B. Titchener, "Psychology: Science or Technology?", in *Pop. Sci. Mo.*, LXXXIV., 1914, 39 ff.

Instead, however, of calling psychology with Ward the "science of experience regarded objectively from the individualistic standpoint," or with Avenarius the "science of experience in general, so far as experience depends upon System C," or with Külpe the "science of the facts of experience in their dependency upon experiencing individuals," or something of that sort, we are accustomed to speak of it as the "science of mind." No harm would be done, if we and our readers always remembered what "mind," as used in a scientific context, must mean. Harm begins at once when we forget that scientific meaning, and start out from the commonsense or traditional significance of the word; when we equate "mind" with "consciousness," which we take as the equivalent of "awareness," and when we set off a group of "conscious phenomena" as the peculiar subject-matter of psychology. I do not think that modern psychologists can fairly be charged with neglect of their duty to correct these errors; it seems to me, on the contrary, that our leaders are painfully careful to set their house in logical order. But habits of speech are inveterate, and common sense is extraordinarily tenacious of life: small wonder, then, that misunderstandings should arise. It is, for example, a misunderstanding that has prompted the polemical paragraphs of Watson's recent articles on what, I suppose, we must be content to call Behaviorism.[2]

This doctrine, as set forth by Watson, has two sides, positive and negative. On the positive side, psychology is required to exchange its individualistic standpoint for the universalistic; it is to be "a purely objective experimental branch of natural science" in the sense in which physics and chemistry are natural sciences.[3] It is to concern itself solely with the changes set up, by way of receiving organ and nervous system, in muscle and gland.[4] It is differentiated from its sister sciences of life partly by its special point of view, partly by the goal which it strives to attain. The changes which it

[2] J. B. Watson, "Psychology as the Behaviorist Views It," *Psych. Rev.*, XX., 1913, 158 ff. (to be referred to in the future as *A*); "Image and Affection in Behavior," *Jour. Phil. Psych. Sci. Meth.*, X., 1913, 421 ff. (to be referred to in the future as *B*).

[3] *A*, 158, 176 f.

[4] *B*, 427 f.

studies are to be approached from the point of view of adjustment to environment; its categories are stimulus and response, heredity and habit.[5] Differentiation, however, is not to be understood as separation; there is now no barrier between psychology and the other "natural" sciences; in the long run behavior will appear as a matter of physical and chemical causation,[6] while nevertheless, as behavior, it is the subject-matter of the special science of psychology, to be interpreted and arranged under the rubrics just mentioned. The erection of this special science is both justified and made possible by the practical goal of behaviorism, which is the working out of general and special methods for the control of behavior, the regulation and control of evolution as a whole.[7]

On the negative side, again, psychology is enjoined by the behaviorist to ignore, even if it does not deny, those modes of human experience with which ordinary psychology is concerned, and in particular to reject the psychological method of introspection. "Consciousness in a psychological sense" may be dispensed with;[8] consciousness, in the sense of a tool or instrument with which all men of science work, may be utilized by the new psychology without scruple and without examination.[9] Imagery, the "inner stronghold of a psychology based on introspection," is denied outright; one of Watson's "principal contentions" is "that there are no centrally initiated processes."[10] And if consciousness may be dispensed with, self-observation and the introspective reports that result from it are to be treated in even more summary fashion; they are to be "eliminated."[11] There will be no real loss; for most of the essential problems with which psychology as an introspective science now concerns itself are open to behaviorist treatment, and the residue may "in all probability be phrased in such a way that refined methods in behavior (which certainly must come) will lead to their solution."[12]

[5] *A*, 167, 177.

[6] *A*, 173, 177.

[7] *A*, 158, 162, 168, 177.

[8] *A*. 161, 163, 175, 176 f.

[9] *A*, 175, 176.

[10] *B*, 423. The statement is qualified in a footnote; I return to the point later.

[11] *B*, 428; *A*, 158, 163, 166, 170, 175.

[12] *A*, 177; *B*, 428.

Such, in outline, is "psychology as the behaviorist views it." Watson, of course, goes into some amount of detail, offering illustration and personal explanation, as well as attacking the method and problems of current psychology. But before I follow him on these various paths, I should like to record two general impressions that the reading of his articles has made upon me. The first impression is that of their unhistorical character; and the second is that of their logical irrelevance to psychology as psychology is ordinarily understood.

I call the articles unhistorical because they give no hint that any similar revolt against an established psychology had taken place earlier in psychological history. Yet one need go no farther back than Comte to find a parallel. Comte's rejection of introspection has often been referred to: let me now quote another passage in which he sums up his attack upon ideology.

"It is evident, first, that no function can be studied but with relation to the organ that fulfils it or to the phenomena of its fulfilment; and, in the second place, that the affective functions, and yet more the intellectual, exhibit in respect of their fulfilment the peculiar characteristic that they cannot be directly observed during the actual course of this fulfilment, but only in its more or less immediate and more or less permanent results. There are then only two different ways of studying scientifically such an order of functions: we must either determine, with all attainable precision, the various organic conditions on which they depend,—and this is the chief object of phrenological physiology; or we must observe the consequence for conduct of intellectual and moral acts,—and this belongs rather to natural history . . .; these two inseparable aspects of one and the same subject being, of course, always so conceived that each may throw light on the other. Thus regarded, this great study is seen to be inseparably connected on the one hand with the whole . . . of natural philosophy, and especially with the fundamental doctrines of biology; and, on the other hand, with the whole of scientific history, of the animals as well as of man, and even of humanity. But when, by the pretended method of psychology, we discard absolutely from our subject-matter the consideration both of the agent and of the act [that is, of the organ of function and of the result of its exercise], what more is there left to occupy the mind than an unintelligible logomachy, in which merely nominal entities are everywhere substituted for scientific phenomena . . .? The most difficult study of all is thus placed at once in a state of complete isolation, without any possible point of support in the simpler and more perfect sciences, over which it is proposed, on the contrary, to give it sovereign rule.

On these two points, all psychologists, however extreme their differences in other regards, are found to agree."[13]

Not Watson himself could be more outspoken or more severe! But we need not go back to Comte and the thirties; we need go only to Cournot and the year 1851. After a sharp criticism of introspection, Cournot writes:

"So we see that the most useful observations on the intellectual and moral nature of man, observations gathered not by philosophers disposed to theories and systems, but by men gifted with the true spirit of observation and prepared to grasp the practical side of things,—by moralists, historians, men of affairs, legislators, instructors of youth,—have not as a rule been the fruit of a solitary contemplation and an internal study of the facts of consciousness, but far rather the result of an attentive study of the behavior (*conduite*) of men placed in various situations, subjected to passions and influences of all sorts."[14]

Here we are hardly without the circle of those "fifty-odd years" which Watson believes—how mistakenly!—have been "devoted to the study of states of consciousness."[15] It would not be difficult to cross that line;[16] but it is unnecessary. My point is that Watson's behaviorism is neither so revolutionary nor so modern as a reader unversed in history might be led to imagine; and that as psychology has weathered similar proposals in the past,—and, I hope and think, has benefited by the storm,—so also it may weather and be benefited by this latest trial of its staunchness.[17]

[13] A. Comte, "Cours de philosophie positive," III., 1838, 774 ff.; the translation of H. Martineau ("The Positive Philosophy of Auguste Comte," 1856, 383 f.) is here inadequate. The polemic against introspection will be found in "Cours," I., 1830, 34 ff.

[14] A. A. Cournot, "Essai sur les fondements de nos connaissances," etc., II., 1851, 319.

[15] *A*, 174. I have shown in my "Experimental Psychology" that the experimental period falls into fairly well-marked sub-periods.

[16] I have especially in mind Lange's chapter on "Scientific Psychology" (1866) and Maudsley's on the "Method of the Study of Mind" (1867 and later).

[17] "Should human psychologists fail to look with favor upon our overtures and refuse to modify their position," Watson writes, "the behaviorists will be driven to using human beings as subjects and to employ methods of investigation which are exactly comparable to those now employed in the animal work" (*A*, 159). The "overtures" seem to consist in the familiar "Ducky, ducky, come and be killed!" But, that apart, why should anything

The second general impression that I record is that of the logical irrelevance of Watson's programme to what is currently called psychology. For suppose that that programme were carried out to its last detail: how would introspective psychology be affected? Why, those who were interested in the method and results of introspection would simply start out where Watson had left off; the universalistic psychology being completed, it would be in order for the individualistic to be begun. A shift of standpoint over against the world of experience means the appearance of a new subject-matter, or (more strictly) of a new aspect of the common subject-matter; and any one aspect has the same claim to scientific consideration as any other; nor is there in science a Congregation of the Index to allow this and to forbid that. The behaviorist may, if he will, ignore "consciousness in a psychological sense"; he may use consciousness as a tool without making it "a special object of observation"; there is none to say him nay; but why should not some one who is not a behaviorist scrutinize what he has ignored, and try to find out empirically of what materials this particular tool is made? Logically, so far as I can see, behaviorism is irrelevant to introspective psychology. Materially, I believe that psychology will be furthered by it, since increased knowledge of the bodily mechanisms, of anything that pertains to Avenarius' System C, means greater stability of certain parts of the system of psychology. Neither logically nor materially can behaviorism "replace" psychology.

Impressions, however, must give way to closer argument: we must view Watson's articles at shorter range. And we shall, perhaps, make most progress if we begin with his pronouncements regarding the failure of experimental psychology.

Psychology, we are told, has failed signally, during the fifty-odd years of its existence, to make good its claim as a natural science. Its present condition is chaotic. The chances are that such ques-

that the "human psychologist" does or fails to do "drive" the behaviorist to do anything? I hope that Watson will find the opportunity to employ human subjects; I hope that he will find them (he will pardon the word) intelligent; I shall be honestly interested in his results.

tions as those of the extensive attribute of auditory and the intensive attribute of visual sensations, or the differences obtaining between sensation and image, will be debated two hundred years hence as inconclusively as they are debated today. Psychological method is esoteric. It has proved unable to grapple with such matters as imagination, judgment, reasoning, conception; these topics have simply become threadbare with much handling. Functional psychology is at fault no less than systematic and structural psychology. Only those "branches of psychology which have already partially withdrawn from the parent," and which are consequently less dependent upon introspection,—experimental pedagogy, the psychology of drugs, the psychology of advertising, legal psychology, the psychology of tests, and psychopathology,—are vigorous growths. The complete elimination of introspection from these disciplines will make their results still more valuable, and will keep them—as psychology itself emphatically is not—in touch with "problems which vitally concern human interest."[18]

That, I believe, is a fair statement of Watson's position; it is given largely in his own words. I have to reply, first, that fifty-odd years is not necessarily a long period in the history of an experimental science. It is not long, of course, regarded as mere duration: for it is in the sixteenth century that "the physicist abandons scholastic speculation and begins to study nature in the language of experiment,"[19] while it is only in the middle of the nineteenth that psychology becomes experimental. It might be long, in a transferred sense, if it were crowded with workers: but the number of productive students in "systematic, structural and functional" psychology does not compare with the number in physics or chemistry.[20] Has Watson, I wonder, ever counted the number of experimental papers that deal with imagination, judgment, reasoning and conception? It is notoriously difficult to trace beginnings; but we shall not

[18] *A*, 163, 176; 165; 164; 163; 173 ff.; 165; 169 f.; 170, 176.

[19] F. Cajori, "A History of Physics," 1899, 27.

[20] Mr. H. G. Bishop has kindly listed for me the experimental papers in psychology, physics and chemistry recorded in the last five volumes of Fock's *Bibliographischer Monatsbericht*. The ratio is approximately 1:9.5:44. Account is here taken of the psychological studies to be found under "Medizin," as well as of those under "Philosophie und Psychologie."

have gone far wrong if we date the first overt attempts to bring these complexes under experimental control from 1902, 1901, 1908 and 1903 respectively,—if we say, at any rate, that their experimental study belongs to the present century. And we have already worn such topics threadbare? I should rather judge that we have hardly touched their fringe. How many decades or centuries they will engage the attention of psychologists, I do not know; the important thing is that we should do thoroughly such work upon them as can be compassed in a generation. Our descendants may ask so much of us; but we owe them nothing more; and though I also hope that two hundred years hence other questions may have replaced those of visual attributes and imaginal characters, of orientation in the rat and of the homing sense of terns, I am far more deeply concerned to sift the materials of discussion than to hurry debate to a conclusion.[21]

There remain the seceding branches, experimental pedagogy and the rest. In their regard, I think, the unhistorical nature of Watson's paper renders his exposition seriously misleading; it is psychology, and not behaviorism, that has shaped their course; and it is psychology, and not behaviorism, that they still look to for guidance. Meumann's *Lectures,* for example, are offered as an introduction to experimental pedagogy and its psychological foundations; the work is penetrated with psychology; the pedagogical experiment is said to be " for the most part the psychological experiment applied to the developing and working school-child."[22] But it is largely owing to Meumann that experimental pedagogy flourishes. Rivers chose the subject of his Croonian Lectures with the desire to show that experimental psychology may be of service to medicine.[23] Stern, who

[21] It is, perhaps, beyond my province to defend functional psychology; but I should not like to have written this sentence: "It is rather interesting that no functional psychologist has carefully distinguished between 'perception' (and this is true of the other psychological terms as well) as employed by the systematist, and 'perceptual process' as used in functional psychology" (*A,* 165). What, then, of Brentano, and of the many psychologists who have been inspired by him?

[22] E. Meumann, "Vorlesungen zur Einführung in die experimentelle Pädagogik und ihre psychologischen Grundlagen," I., 1911, 27.

[23] W. H. R. Rivers, "The Influence of Alcohol and Other Drugs on Fatigue," 1908, 1, 121.

stands to the psychology of testimony in somewhat the same relation that Meumann bears to experimental pedagogy, is also through and through psychological. Binet, whose name is inseparably connected with the psychology of tests, might fairly be called an extremist in his devotion to introspection. Pick demands "eine psychologische Vertiefung der Aphasielehre," and makes constant use of laboratory material: "es ist höchste Zeit dass die Pathologie endlich von diesen Dingen Kenntnis nehme."[24] It is worth noting that Meumann, Stern and Binet—the men to whom we are chiefly indebted for experimental pedagogy, the psychology of testimony, and mental tests—would all have been brushed aside by Watson, a few years ago, as typically introspective psychologists; and it is worth noting also that they themselves look upon this later work, not as the negation of their psychological training, but as its direct extension and practical fulfilment. It is worth noting, again, that a man of Pick's authority ascribes the unprogressive state of psychopathology in large measure to an ignorance of current introspective psychology, and himself makes definite use of the "imageless thought, attitudes, and *Bewusstseinslage*, etc.," which Watson contemns.[25] I am not here depreciating behaviorism; but I think there is no justification for behaviorism's depreciation of psychology.[26]

[24] A. Pick, "Die agrammatischen Sprachstörungen: Studien zur psychologischen Grundlegung der Aphasielehre," 1913, I., 11, 58, etc.

[25] *A*, 163. The psychology of advertising, so far as it has gone, bears out my argument. Cf. D. Starch, "Principles of Advertising," 1910; W. D. Scott, "The Psychology of Advertising," 1912; W. A. Shryer, "Analytical Advertising," 1912; H. L. Hollingworth, "Advertising and Selling; Principles of Appeal and Response," 1913. The psychology of these works is not always of the severest type; but the attitude of the writers is unmistakably psychological.

[26] I have said nothing of the "esoteric" nature of introspection, because I have dealt with that charge in recent articles (*American Jour. Psych.*, XXIII., 1912, 427 ff., 485 ff.). In referring to my own work, Watson falls into the common mistake of confusing observation with theory. If he were to serve as observer in one of our studies on attention, he would have no difficulty, after a little practice, in passing the sensory judgments that we required of him. That is a matter of observation and report. Whether he would, after such participation in the actual work, accept our setting and interpretation of the results is another and a different question.

In his second article Watson discusses two topics "which may seem to many to be stumbling-blocks in the way of a free passage from structuralism to behaviorism." These topics, one sees with some surprise, are Image and Affection: with surprise, I say, because we had already been prepared to ignore consciousness and to eliminate introspection. It turns out, however, that the difficulty is methodological. For if the physiological counterpart of the image is cortical, then that mode of behavior which is to replace the introspective psychology of thought lies inaccessible within the skull. If "affection is a mental process distinct from cognition (*sic*)," then affection cannot be an "organic sensory response." So image and affection have to be dealt with; and Watson deals with them faithfully; the existence of the image is denied outright, and affection is carried willy-nilly to the periphery.

Watson offers three bits of evidence for his contention that "there are no centrally initiated processes." In the first place there are experimentalists who maintain that thought-processes may go on independently of imagery. In the second place there is no objective experimental evidence of the presence of different types of imagery. In the third place even the structuralists seek to reduce higher thought-processes to groups of obscure organic processes. I think that these arguments can be met in terms almost as brief as their statement. In the first place, the view that thought is independent of imagery hardly constitutes a presumption that there are no central processes of any kind. In the second place Fernald does not deny type, but asserts that "an individual's type can be adequately indicated only by an extended statement";[27] and that is the opinion now generally held by psychologists. But let us suppose that types cannot be indicated at all: by what logical inference may we pass from this negative finding to the denial of imagery? In the third place the reduction of thought to organic processes always implies in the background a cortical set corresponding to the *Aufgabe*. Watson, nevertheless, denies that there are centrally initiated processes, and proposes to find the behaviorist equivalent of thought in movements,

[27] M. R. Fernald, "The Diagnosis of Mental Imagery," *Psych. Monogr.*, XIV., 1, 1912, 128 ff.

chiefly, of the larynx. In the same way he finds the behaviorist parallel of affective process in tumescence and shrinkage of the organs of sex. These views are put forward as matters of hypothesis and of personal conviction, though they are also put forward with some confidence. Time and trial will prove their value.

Meanwhile, it would seem that Watson has in both cases, in the case of image as in that of affective process, overshot the logic of his position. The negative argument as regards imagery can never be proved in formal logic, to say nothing of the fact that it conflicts with a very large body of positive observation.[28] Logical confusion is shown plainly enough in the following remark: "I may have to grant a few sporadic cases of imagery to him who will not be otherwise convinced, but I insist that the images of such an one are sporadic, and as unnecessary to his well-being and *well-thinking* as a few hairs more or less on his head." If there are any images at all, then there are (on Watson's own showing) centrally initiated processes, and behaviorism is bound to take account of them; and his personal assurance that they are unnecessary to thought is offset at once by the assurance of Watt and others that thought does in fact go on in imaginal terms.[29] Science is concerned with empirical facts; and for the individual man of science to "insist" that certain facts of observation may be cancelled without loss to the science to whose subject-matter they belong is to incur, at the very least, the charge of a certain rashness of behavior.

Another logical objection seems to me to lie against Watson's procedure in this second article. All science works upon assumptions, psychology no less than the other sciences. Münsterberg, for instance, is wholly within his logical rights when he assumes that all conscious contents, without exception, may be transformed into sensations:[30] given his premises, they must be so transformed. Be-

[28] I quote a recent statement: "From an actual count of factors present in the recall of ten of our problems, we estimate that our investigation embraces approximately 200,000 images. . . . Of all our introspective data, about ninety per cent. are visual images" (E. O. Finkenbinder, *Amer. Journ. of Psych.*, XXV., 1914, 81).

[29] H. J. Watt, "Experimentelle Beiträge zu einer Theorie des Denkens," *Arch. f. d. ges. Psych.*, IV., 1905, 312; cf. my "Thought-processes," 1909, Lect. I.

[30] H. Münsterberg, "Grundzüge der Psychologie," I., 1900, 331.

haviorism would be equally within its logical rights in assuming that all central processes may be transformed into peripheral: given Watson's premises, they must be so transformed. But you cannot eat your cake and have it too. You may bring up facts in support of your choice of assumptions; and you may show the scientific results to which those assumptions lead; you may not, surely, offer these results, even hypothetically, as facts in proof of your assumptions. If we take up Münsterberg's position, we find nothing but sensations to work upon; but that is not evidence that Münsterberg's position is well-chosen. If we take up Watson's position, we find, perhaps, laryngeal movements and changes in the state of the sex-organs; but that discovery gives no logical support to the principles of his behaviorism.[81] It is, indeed, obvious that, if the larynx and the sex-organs prove refractory, the behavioristic equivalents of image and affection must just be put—hypothetically, again—somewhere else; and so on, and so forth; for it is a logical consequence of the position that somewhere on the periphery the required movements and changes are to be discovered; and the periphery is complex enough to suggest any number of localizations.[82]

[81] I do not deny that the empirical consequences of a particular theoretical attitude may serve *materially* to justify that attitude for its special day and generation; men have often worked successfully for a time though the logical foundations of their work were insecure. But the permanence of the structure depends on the solidity of the foundations, and to shirk their inspection is only to make "more haste" for the sake of "less speed."

[82] The reduction of pleasantness-unpleasantness at large to sheer sex-feeling is to me nothing else than nonsensical. But, like Watson, "I shall not attempt to develop the point further at the present time." It is, however, necessary to point out that the method of expression is not so ill bestead as Watson declares it to be. In his latest tabulation (*Arch. f. d. ges. Psych.*, XXXI., 1914, 27 ff.), E. Leschke finds 90 per cent. of substantial agreement in the investigations which he considers. The two principal sources of error are a disregard of neurasthenia and of vasomotor anomalies and—an inadequate psychological training of experimenter and observer!

I may, perhaps, be expected to say a word on Watson's criticism of my own doctrine of affection. The doctrine itself, I regret to say, he has not understood. But he has also mistaken the motives which led me to adopt it. My view that affection lacks the attribute of clearness is, he says, an assumption "arrived at largely in the interest of obtaining a structural differentiation between sensation and affection" (*B*, 426). As if a structural system would not be greatly simplified and, as system, improved by the reduc-

But the argument does not end here. I have formulated my criticism as if Watson's views were rigorously worked out, and as if his centrally initiated processes were conceived rigorously as physiological. That is, evidently, not the case; these processes are, in Watson's thought, both mental and physical; not only are brain-changes to be transformed into their equivalent peripheral changes, but the facts of psychology (as psychology is currently taken) are also to be carried, by way of behavioristic substitution, to the bodily periphery. The "required" peripheral changes are required—by the thoughts and emotions of an introspective psychology! And with that, by definition, behaviorism has nothing to do. The confusion here is plain, and the critical point need not be further labored. I must add, however, in the same connection, that I do not understand Watson's attitude to sensation. He admits that there are special cutaneous nerves "which mediate pain." He thinks that imagery is the key of the introspective stronghold: "all the outer defences might be given over to the enemy." These utterances seem to imply that sensation, if not part of the subject-matter of behaviorism, is at least neutral ground between that and introspective psychology; whereas, in the earlier article, sensation was definitely assigned to psychology.[33] Logically, I do not see how a behaviorist, in Watson's sense, can know anything of pain. I regard sensations as introspective material on precisely the same level with images; and I should challenge the behaviorist to replace or duplicate, in his universalistic terms, the various observations recorded, for example, in Stumpf's "Tonpsychologie," or in Hering's new "Lichtsinn."[34]

tion of affection to organic sensation! I only wish that I could see my way clear to it. J. R. Angell recognized the temptation in *Philos. Rev.*, XIX., 1910, 322; Watson's comment puts the cart before the horse.

[33] *A*, 164.

[34] C. Stumpf, "Tonpsychologie," I., 1883, Vorwort; E. Hering, "Zur Lehre vom Lichtsinne" [1874], 1878, 72, 106. "Der . . . Weg, welcher von den Aetherschwingungen ausgeht, hat bis jetzt, so weit es sich nicht blos um die Schicksale der Lichtstrahlen in den optischen Medien, also lediglich um eine Application der physikalischen Optik auf's Auge handelte, noch zu keinem Ergebnisse geführt"; "Ich war immer der Ansicht, dass die grossen Aufgaben, welche der Physiologie und insbesondere der Nervenphysiologie gestellt sind, am zweckmässigsten, ähnlich einer Tunnelbohrung, von zwei Seiten zugleich in Angriff genommen werden, nämlich nicht nur von der physikalisch-chemischen Seite, sondern auch von der psychischen."

All in all, this paper on Image and Affection, while it is written with a truly scientific candor, shows, I think, that the author has imperfectly grasped the logic of the situation which he has himself created.

In trying, now, to appraise Watson's proposals as a whole, we must begin by clearing them of their personal and accidental accompaniments. Watson demands a psychology "which concerns itself with human life" and whose "problems vitally concern human interest." He ascribes to such a psychology the practical goal of the control of behavior, the regulation and control of evolution in general; that is to say, he connects it with euthenics and eugenics. These expressions give his proposed psychology the stamp of a technology: for science goes its way without regard to human interests and without aiming at any practical goal; science is a transcription of the world of experience from a particular standpoint, deliberately adopted at the outset and deliberately maintained; the pursuit of a practical end is the earmark of a technology. And how does that matter in the present context? It matters very greatly. Watson is asking us, in effect, to exchange a science for a technology; and that exchange is impossible; for a technology draws not upon one but upon many sciences, and draws upon many other sources than science; and so the striking of a balance-sheet between a given science and a given technology is out of the question. I said above that behaviorism can never replace psychology because the scientific standpoints of the two disciplines are different; we now see that Watson's behaviorism can never replace psychology because the one is technological, the other scientific. This technological coloring, while it strengthens the emotional appeal of Watson's plea, is nevertheless not of the essence of behaviorism. The behaviorist's position, as we shall see, may be outlined in the plain black and white of science.

The two articles are characterized, again, by the recurring note of hurry, of impatience. Fifty-odd years gone, and we have accomplished so little: two hundred years, and shall we have accomplished much more? Surely it would be well to sweep the field clear, to forget the past, and to start the race anew! But all reformers, I

suppose, are likely to be impatient; and their impatience does not affect the value of their proposed reforms. We need not regard this hurry, either, as of the essence of behaviorism. Watson himself, in less fervid mood, might not grudge us a little time for the study of his plans,—would even recognize, I believe, that our hasty acceptance of them, without due consideration, must be more dangerous than a reasonable delay.

So we come at last to behaviorism itself; and what I take that to be I can best indicate by a parallel. In the disciplines which we call physiological psychology and psychophysiology we are interested, with slight difference of emphasis, in the two aspects of certain phenomena of the living organism; we seek to couple physiological with psychological, psychological with physiological, and so to get a complete description of the psychophysical. We may, now, in just the same way, speak of biological psychology and of psychobiology; indeed, those terms are already in use, and their general significance is plain. But here is the context to which behaviorism, if I understand it aright, must of necessity belong; it is the biological side of a biological psychology or of a psychobiology; I cannot make it more, and I do not think that its practitioners can make it less. The argument is as follows:

The behaviorist, as Watson describes him, also studies certain phenomena of the living organism. In theory, he may study these phenomena in either of two different ways. He may regard them as phenomena simply, as last facts, as things given, as phenomena to be taken at their face value and described and explained in their own right: then, he is working in what we are accustomed to call biology; he has adopted no new standpoint and needs no new name. Or again he may regard them as symptomatic; as reporting, expressing, indicating, leading up to something beyond themselves; as claiming detailed study, not only in their own right as data of biology, but also because of this further and specific character of report or expression. Here is ground for a discipline other than biology; a novel point of view has been attained. At once, however, the question arises: What, then, is it that the phenomena report or express? Of what are they symptomatic? The answer seems obvious: they are symptomatic of

behavior. And the answer seems satisfactory—until we remember that the phenomena, by hypothesis, *are* behavior, "behavior material," "behavior data," and that a phenomenon cannot both "be" and "be a symptom of" the same thing. I see no way out of this dilemma. Either the behaviorist is just biologist; and in that case he has no nearer relation to psychology than have his coworkers who are content to call themselves biologists: or the behaviorist sees expression where the biologist sees ultimate fact; and in that case he may equally well be called psychobiologist, seeing that the phenomena expressed or reported by the organic changes which he studies cannot be anything else than psychical.[85]

But if this conclusion is sound, it means two things. It means that behaviorism is correlated with a psychology, with some sort of psychology in the usual sense; and it means that behaviorism must take account of all kinds of organic changes, and not merely of those occurring at the periphery. I believe that both of these consequences must be accepted. Consider again, for example, Watson's reduction of thought to delicate movements of the larynx: those movements are movements of incipient or vestigial articulation. But words, as Watson seems to have forgotten, are also meanings; and meanings take us either to the nervous center—or to psychology; they take us, in fact, to both. Moreover, the very problem of these laryngeal movements is given to the behaviorist by psychology: how would he have lighted on the idea of transforming thought into movement unless psychology had made him acquainted with thought? I do not say that the incentive will come always or must necessarily come from the psychological side; there will be give and take; but it is none the less clear that behaviorism and psychology are, in this context, correlative; and that though an individual student may wisely and successfully confine himself to the study of behavior,—yes, and may all his life maintain a polemical attitude to psychology proper,—it is yet impossible to have a science of behaviorism independent of all psychology. It is equally impossible, of course, within the same context of psychobiology, to have an independent science of psychology; the two halves are essential to the single whole; and the psychology of

[85] Cf. with this paragraph *A*, 158 ff.

the behaviorist will, in matters of selection, emphasis, arrangement, terminology, perspective, differ from general psychology just as behaviorism itself differs from general biology.[36]

We thus conclude that to say, as was said above, "psychology would begin where a completed behaviorism left off," is really to say too little. The psychology which is correlated with behaviorism begins when behaviorism begins, and the fortunes of the two are bound up in the same bundle. Psychobiology will run the same course as psychophysiology and psychophysics. It is now, I suppose, in its first phase, when pioneer work brings in gross and tangible returns. Next will come the period of revision, of elaboration of details,—a period of discouragement, perhaps, as the former was a period of elation. And then will follow the period of slow and steady progress, varied by a certain amount of wholesome interruption. Meanwhile introspective psychology, which is now entering upon this third stage of its scientific career, will go quietly about its task, wishing the new movement all success, but declining—with the mild persistence natural to matters of fact—either to be eliminated or to be ignored.

[36] At this point we become involved in the controversy regarding the possibility of an "animal psychology." I have no wish to avoid that issue, though I must postpone its full discussion for another time. I believe that an animal psychology is definitely possible; I think that with the law of continuity as basal presupposition, and with the argument from analogy for use in the concrete case, the science may be established. Meantime I have elsewhere expressed my agreement with Watson that there can, in strictness, be no objective criterion of the psychical (*A*, 161).

THE VALENCE OF NITROGEN IN AMMONIUM SALTS.

By WILLIAM A. NOYES and RALPH S. POTTER.

(Read April 24, 1914.)

During the early years of the development of the theory of valence many chemists held the view that each element has an unvarying valence. The apparent change of valence in nitrogen from ammonia to ammonium salts and in phosphorus from phosphorus trichloride to phosphorus pentachloride was explained by calling the ammonium salts and the pentachloride molecular compounds, as distinguished from ammonia and the trichloride, in which the true valence of the elements was supposed to be shown. This view received support from the dissociation of ammonium salts and of phosphorus pentachloride in the gaseous state. Gradually, with the demonstration that phosphorus pentachloride volatilizes in part unchanged, that phosphorus pentafluoride, PF_5, has a vapor density corresponding to its formula and, in general, that dissociation in the gaseous state does not correspond to any rational distinction between unitary and molecular compounds the view that elements may show a varying valence in their compounds and that nitrogen and phosphorus are sometimes trivalent and sometimes quinquivalent, came to be generally accepted.

More recently Werner[1] has proposed a modified molecular formula for ammonium chloride, $H_3N..HCl$. By this formula he intends to indicate that in the ammonium salts the nitrogen atom retains a normal valence of three but that the nitrogen atom of the ammonia and the hydrogen atom of the hydrochloric acid are held together by secondary ("Neben") valences, the hydrogen and chlorine of the acid retaining essentially the same relation to each other as in the free acid.

[1] See "Neuere Anschauungen auf dem Gebiet der anorganischen Chemie," p. 96 (1905).

An amino acid may, theoretically, assume in the aqueous solution the following forms: (*a*) the free acid, $R\left\langle\begin{array}{l}CO_2H\\NH_2\end{array}\right.$; (*b*) a cyclic salt, $R\left\langle\begin{array}{l}CO\\NH_3\end{array}\right\rangle O$, or according to Werner, $R\left\langle\begin{array}{l}CO - O\\ \quad\ \ \vdots\\NH_2 - H\end{array}\right.$; (*c*) a bimolecular or polymolecular salt formed by the union of two or more molecules, $R\left\langle\begin{array}{l}CO_2 - H_3N\\NH_3 - O_2C\end{array}\right\rangle R$; (*d*) the ions of the acid group, $R\left\langle\begin{array}{l}CO_2^-\\NH_2\end{array}\right.$ and H^+; (*e*) the ions of the base, $R\left\langle\begin{array}{l}CO_2H\\NH_3^+\end{array}\right.$ and OH^-; (*f*) the double, amphoteric ion, $R\left\langle\begin{array}{l}CO_2^-\\NH_3^+\end{array}\right.$.[2]

The "inner salt" structure was first proposed by Erlenmeyer and Siegel[3] in 1875. Ten years later Ostwald[4] noticed that solutions of glycocoll, $CH_2NH_2CO_2H$, have a very low molecular conductivity and that this is only slightly increased by dilution. He states that in its behavior it is more like a neutral salt than an acid. In 1891 Marckwald[5] called attention to the fact that amino acids of the aliphatic series react only slowly with the mustard oils, while other primary amines react quite readily. Since the amino acids react easily in alkaline solutions, he held that the acids are, in reality, inner salts. Sakurai[6] attempted to substantiate the "inner salt" structure on the preparation from halogen derivatives of the acids and on the resistance which amino acids offer to the formation of acid chlorides. Walker[7] points out that conductivity determinations tell us very little about the structure of glycocoll but that since the conductivity of phenylglycocoll, $C_6H_5NHCH_2CO_2H$, is greater than that of acetic acid it must contain a carboxyl group which ionizes. Tilden and Forster[8] showed that the amino group of amino acids

[2] "Zwitterion."
[3] *Ann.*, *176*, 349 (1875).
[4] *J. prakt. Chem.*, *32*, 369 (1885).
[5] *Ber.*, *24*, 3278 (1891).
[6] *Proc. Chem. Soc.*, *10*, No. 138 (1894).
[7] *Proc. Chem. Soc.*, *10*, No. 139 (1895).
[8] *Chem. News*, *71*, 239 (1895).

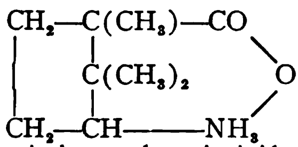
may be replaced by chlorine by the action of nitrosyl chloride and considered this an argument against the inner salt formation. Somewhat later Carrara and Rossi[9] based an argument for the inner salt structure on the conductivity of betaine hydrochloride, $(CH_3)_3NClCH_2CO_2H$. From the values found they considered that the salt was almost completely hydrolyzed to hydrochloric acid and betaine, $(CH_3)_3N\underbrace{CH_2CO}_{O}$. Winkelblech[10] points out, however, that if betaine hydrochloride is in reality hydrolyzed the conductivity of the solution should be the same as that of the equivalent amount of hydrochloric acid while both Bredig's measurements and those of Carrara and Rossi gave a conductivity scarcely more than one half as great There can be no doubt, of course, that the anhydride of betaine, $(CH_3)_3NCH_2CO_2$, has the structure of a salt, but no one seems to have determined whether this is monomolecular or dimolecular. Our results given below indicate that a solution of an amino acid which gives no inner salt may still contain the acid mostly in the monomolecular form.

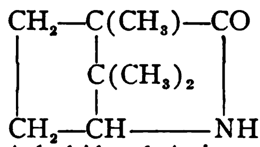
Winkleblech[11] discusses the hydrolysis of an amino acid on the basis of conductivity data for weak acids, weak bases and water. It does not seem possible from conductivity data, however, to determine whether the acid is in the form of an inner salt, $\left[R\begin{matrix}CO_2\\NH_3\end{matrix}\right]$, in the unionized state, $R\begin{matrix}CO_2H\\NH_2\end{matrix}$ or $R\begin{matrix}CO_2H\\NH_3OH\end{matrix}$, in the form of the double, amphoteric ion $R\begin{matrix}CO_2^-\\NH_3^+\end{matrix}$ or in the form of a bimolecular salt, $R\begin{matrix}CO_2—NH_3\\NH_3—CO_2\end{matrix}R$. The hydrogen and hydroxyl ions of the amphoteric form would, of course, combine to form water and if the acid and basic functions were of equal "strength" the solution would react neutral. None of these forms would show any conductivity and while the bimolecular form could be distin-

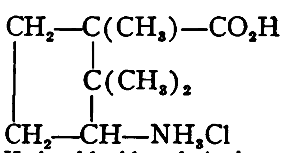
[9] *Atti R. Accad. Lincei* (5), *6*, 208 (1897).
[10] *Z. physik. Chem.*, *36*, 590 (1901).
[11] *Loc. cit.*

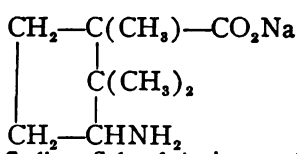
guished from the others by a determination of the molecular weight it is not clear how any of the ordinary physical methods could be used to distinguish between the three forms, $R\left\langle\begin{matrix}CO_2\\NH_3\end{matrix}\right.$, $R\left\langle\begin{matrix}CO_2H\\NH_3OH\end{matrix}\right.$ and $R\left\langle\begin{matrix}CO_2^-\\NH_3^+\end{matrix}\right.$. The form $R\left\langle\begin{matrix}CO_2H\\NH_2\end{matrix}\right.$ would have a lower molecular weight and might, possibly, be distinguished from the other three by that means. It does not seem to us that the ordinary equations for hydrolysis, which Winkelblech attempts to apply, could be used in a complex case of this sort.

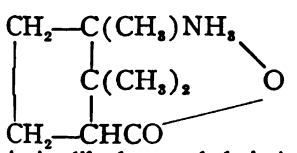
From the above summary it would seem that the evidence with regard to inner salt formation is not altogether satisfactory and light upon the question from an entirely different point of view is welcome. We think that we have secured this from a study of the specific rotations of a series of amino acids derived from camphor. The formulas and names of the compounds are given below. To bring out the relationships more clearly the specific rotations given for the salts are calculated to the basis of one gram of the free acid in 1 c.c. of the solution instead of for one gram of the salt.

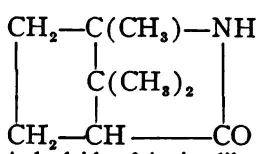

Aminocamphonanic Acid. $(\alpha)_D = -29.2°$.

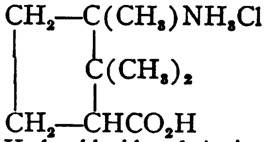

Anhydride of Aminocamphonanic Acid. $(\alpha)_D = -60.5°$.


Hydrochloride of Aminocamphonanic Acid. $(\alpha)_D = 25.0°$.

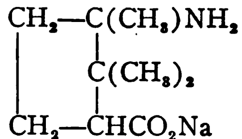

Sodium Salt of Aminocamphonanic Acid. $(\alpha)_D = 52.8°$.


Aminodihydrocampholytic Acid. $(\alpha)_D = 53.7°$.

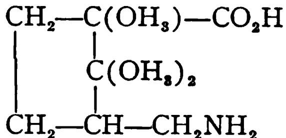

Anhydride of Aminodihydrocampholytic Acid. $(\alpha)_D = 72.8°$.



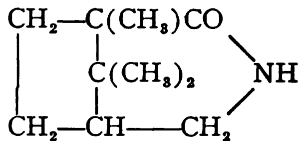
Hydrochloride of Aminodihydrocampholytic Acid. $(\alpha)_D = 41.3°$.



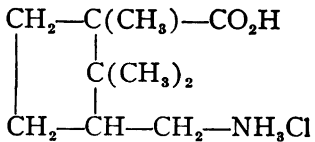
Sodium Salt of Aminodihydrocampholytic Acid. $(\alpha)_D = 18.3°$.



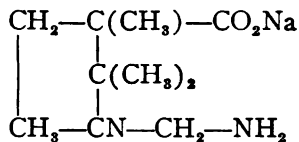
α-Aminocampholic Acid. $(\alpha)_D = 67°$.



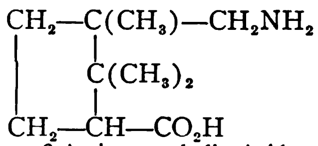
Anhydride of α-Aminocampholic Acid. $(\alpha)_D = -33.9°$.



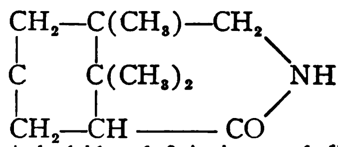
Hydrochloride of α-Aminocampholic Acid. $(\alpha)_D = 44.7°$.



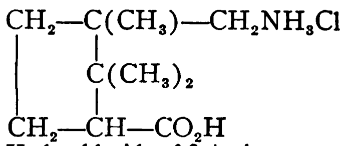
Sodium Salt of α-Aminocampholic Acid. $(\alpha)_D = 62.4°$.



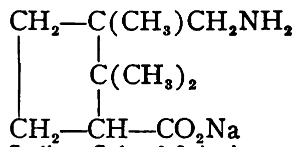
β-Aminocampholic Acid. $(\alpha)_D = 16.4°$.



Anhydride of β-Aminocampholic Acid. $(\alpha)_D = 66.5°$.



Hydrochloride of β-Aminocampholic Acid. $(\alpha)_D = 41.3°$.



Sodium Salt of β-Aminocampholic Acid. $(\alpha)_D = 14.3°$.

It will be noticed that the aminocamphonanic acid and aminodihydrocampholytic acid are represented as having a cyclic or inner salt structure, while the aminocampholic acids are both represented as having an open structure. The evidence for these structures is based on the specific rotations of the compounds. The rotation of the sodium salt and hydrochloride of aminocamphonanic acid are to the right while that of the anhydride, which is undoubtedly cyclic in

structure, is to the left. The free acid is also left handed, indicating a cyclic structure similar to that of the anhydride. The sodium salt and hydrochloride of aminodihydrocampholytic acid are right handed. The free acid and anhydride are also right handed, but with a considerably increased rotation. The sodium salt and free α-aminocampholic acid are both right handed with rotations closely alike, indicating that each has an open structure, but the anhydride, which certainly has a cyclic structure, is left handed and has a rotation very closely like that of the aminocamphonanic acid, indicating again very clearly that the latter has a cyclic structure and that each *compound contains a cycle of six atoms.* The sodium salt of β-aminocampholic acid and the free acid also correspond closely in rotation, indicating an open structure for both, while the hydrochloride and anhydride have a considerably greater rotation, as is the case with both the free aminodihydrocampholytic acid and its anhydride.

All of these observations are consistent with the hypothesis that aminodihydrocampholytic and aminocamphonanic acid form cyclic salts containing cycles of six atoms, while the aminocampholic acids do not form such salts because, if formed, they would contain cycles of seven atoms. It seems difficult to find any other simple explanation for the observations.

The results also point very strongly to the formula for ammonium salts which represents them as containing quinquivalent nitrogen and against Werner's formula. According to Werner's formula the free aminocamphonanic and aminodihydrocampholytic acids would contain cycles of seven atoms,

$$\begin{array}{llll}
CH_2 & \!\!—C(CH_3) & \!\!—CO— & O \\
| & \;\;| & & | \\
| & \;\;C(CH_3)_2 & & H \\
| & \;\;| & & \vdots \\
CH_2 & \!\!—CH & \text{————————} & NH_2
\end{array}$$

Such a formula is quite inconsistent with all that we know about the ease with which rings of five and six atoms are formed and the comparative rarity of seven-atom rings. It is also inconsistent with the close agreement between the rotation of the aminocamphonanic

acid and that of the anhydride of α-aminocampholic acid. We know that the latter compound contains a six-atom ring.

Determinations of the molecular weights in aqueous solutions by the freezing point method have shown that all four of the amino acids are monomolecular in such solutions.

The experimental details of the investigation will be published in the *Journal of the American Chemical Society.*

SOME FURTHER CONSIDERATIONS IN THE DEVELOPMENT OF THE ELECTRON CONCEPTION OF VALENCE.

By K. GEORGE FALK.

(Read April 24, 1914.)

The electron conception of valence is based upon the view that when two atoms combine, one becomes charged positively and the other negatively. According to J. J. Thomson,[1] the union of two atoms is brought about by the transfer of a negatively charged corpuscle from one atom to the other; the atom losing the corpuscle becoming charged positively, the one gaining the corpuscle, charged negatively. In order to represent graphically the linkings between atoms the lines or dots which represent the bonds ordinarily are replaced by arrows in the electronic considerations, the head of the arrow indicating the direction in which the corpuscle is assumed to be transferred in the production of the chemical bond.

In every discussion of valence, it is necessary to consider the limitations of the problem. Valence is a number. The valence of an atom shows the number of corpuscles or negative electrons gained or lost by that atom in forming chemical bonds. In slightly different terms, the valence of an atom shows the number of atoms (or groups of atoms) held in combination by that atom when the hydrogen atom as it exists in most of its compounds is taken as the positive unit. Valence may be likened to the capacity factor in energy considerations.[2] Like the capacity factor, it is denoted by a definite number, and while this number may vary under different conditions, a quantity or number of atoms (or combining weights) held by an atom (or combining weight) of the element in question is always meant.

[1] "The Corpuscular Theory of Matter," pp. 138–9 (1907).

[2] Cf. S. L. Bigelow, "Theoretical and Physical Chemistry," p. 80 (1913).

In the same way, the chemical affinity between two combining atoms would correspond to the intensity factor in chemical energy. This chemical affinity can be measured quantitatively only by the change in free energy of the reaction in question. Stability relationships, or chemical affinity discussions, do not enter directly into valence questions, although the existence of substances is controlled entirely by these. The separation of these two problems, valence and chemical affinity, makes it clear that while a great number of substances may be predicted from a consideration of valence structures alone, questions of chemical affinity, or relative stability, limits the number of these substances which are actually known or may be prepared.

As a result of the comparative study of large numbers of compounds, it has become possible to say which would probably exist under ordinary conditions and whether some would react more rapidly than others. These qualitative factors do not give any information concerning the real quantitative measures of relative stability.

Valence is therefore essentially a classifying principle. While it is based directly upon and derived from the atomic theory, it may also be used without considering atoms. The conception of atoms and molecules is based upon the experimental laws of definite and multiple proportions, and if, instead of atoms and molecules, combining and formula weights are used, the same relations will be found, although perhaps not pictured as readily.

The most important feature of the present development is that in speaking of the valence of an element, it is not sufficient to give a number. It is just as important to state whether this number is positive or negative, as the valence or the number of unit atoms or groups held in combination involves also the question whether these are electropositive or electronegative. This is brought out clearly in the Periodic System of Mendeléeff, especially for Groups 4 to 7, where the types of combination with hydrogen and with oxygen represent the maximum negative and positive valences of the elements of these groups.

Since the valence of an atom may be positive or negative depend-

ing upon the loss or gain of corpuscles, the knowledge of the electrical state of an atom in a compound is of importance. Ionization in solution is an invaluable aid in determining the distribution of these charges. Dissolving a substance does not produce electric charges on atoms but only makes these charges manifest to certain experimental methods. These are, in fact, the only direct experimental methods for determining valence. For substances which ordinarily do not ionize, a knowledge of the distribution of the charges is also important. This question was taken up for organic compounds and reactions by Professor J. M. Nelson, of Columbia University and the speaker in a number of papers.[3] It was shown that satisfactory classifications could be developed with the electron conception of valence alone, but that the use of both polar and non-polar valences leads to contradictions in reactions which are fundamentally similar.

With compounds which do not ionize, the Periodic System serves in a general way as a guide for developing valence structures in which relative positive and negative properties of the different atoms are involved. When two atoms are united by a single bond (one unit of valence), where one corpuscle is transferred in the production of the bond, there is ordinarily no question of the direction of transference of this corpuscle, that is to say, which element is positive and which is negative. If isomers exist, the difference in the relative affinity of the atoms for the negative corpuscle may be small (as in iodine monochloride) and the less stable modification may possess the structure in which the corpuscle is transferred in the opposite direction from that of the stable modification.

Some interesting questions are raised when the double bond is considered from the electronic point of view. As used in the past, the justification for the double bond lies in the desire to maintain consistently, constant values for the valence of certain atoms. Practical work during the past fifty years has borne out within certain limits the usefulness of this conception. Before discussing the significance of the double bond with the newer ideas of valence, some general

[3] *Jour. Amer. Chem. Soc.*, *32*, 1167 (1910); *33*, 440 (1911); *35*, 1810 (1913); *36*, 209 (1914).

facts must be mentioned. A single bond between two atoms gives no information as to the stability of the union between these atoms. A double bond between two atoms cannot give any more information with regard to the stability of the linking. Qualitatively it has been found that the rate of reaction for compounds containing double bonds is greater in some ways than the rate for compounds containing single bonds, and that with certain reagents decomposition at the double bond occurs more rapidly than at other parts of the molecule, but this is manifestly different from a discussion of true stabilities of compounds. Reaction velocities bear no simple relation to stabilities of substances and "reactivity" as used in organic chemistry very often refers only to these reaction velocities. The double bond in the ordinary language signifies two units of valences just as the single bond denotes one unit of valence, and in this sense, the only permissible one, the representation of a double bond by two lines is a correct picture of the linking when one line is used for the single bond.

When two atoms of elements which differ very markedly in electrochemical properties are combined by a double bond, one of these atoms may be considered to be electropositive and the other electronegative. In valence terms, an atom of one of these elements has given up two negative electrons to the atom of the other to form the double bond, the former becoming positive, the latter negative. These cases are as simple as those in which only single bonds are involved. Two units of valence are used in each linking with the result that the valence of one of the atoms is $+2$ and of the other -2 due to the double bond.

If two atoms of elements which do not differ much in electrochemical properties are combined by a double bond, the possible relations from the electronic point of view are somewhat more complex. On the other hand, explanations of reactions and classifications of isomers are afforded which are not possible with the view of the double bond in which electrons are not considered.

For the present purpose, it will be sufficient to outline some of the relations. The substances to be considered include mainly the compounds of carbon with double bonds between two carbon atoms

or between one carbon atom and an atom of some other element such as oxygen or nitrogen. In order to illustrate the present discussion with a definite case, a compound containing a double bond between two carbon atoms in which the other bonds are combined with similar groups, may be chosen. Two possible arrangements suggest themselves with regard to the directions in which the corpuscles may be transferred to form the double bond. One carbon atom may lose two corpuscles and the other may gain two in the formation of the double bond. In this case, the valence of the first carbon atom due to the double bond is $+2$, of the second -2. The other possibility involves the gain and loss of one corpuscle by each of the carbon atoms in forming the double bond. In this case, the valence of each carbon atom due to the double bond will be $-1+1$. Since the oxidation of an atom is defined as a decrease of the negative charge or number of corpuscles, and reduction as a decrease of the positive charge, these atoms united by the double bond would be present in different states of oxidation in the different isomeric substances. It is evident therefore, that, with the electron conception, the double bond may show different reactions with various reagents depending upon the directions of the valences of the double bond or the state of oxidation of the atoms united by the double bond. Isomeric substances might exist in which the isomerism would be due to the different directions of the valences of the double bond. This subject has been discussed at some length in previous papers.[4]

Similar relations should be expected to hold with compounds containing a triple bond. While not as much work has been recorded in the literature for substances of this nature, it has been possible, with the electron conception of valence, to explain some reactions of compounds containing triple bonds much more satisfactorily than with the older valence view.

The general view of valence is that of a classification of chemical compounds and reactions. Since the introduction of the electronic nature of valence into all branches of chemistry widens and

[4] Cf. *S. of M. Quarterly*, *30*, 179 (1909); *Jour. Amer. Chem. Soc.*, *32*, 1167 (1910).

extends the classification, and since much of the classification depends upon a number of correlated facts and relations and not upon single crucial and well-defined experiments, it may be expected that some of the formulas advanced and explanations of reactions offered at the present time will be subject to change. Caution must continually be exercised against reading into valence structures ideas which are foreign to valence. A limitation of the questions discussed to the phenomena which may rightly be included would obviate much confusion and bring valence relations into clearer light.

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HEWETTITE, METAHEWETTITE AND PASCOITE, HYDROUS CALCIUM VANADATES.

By W. F. HILLEBRAND, H. E. MERWIN and FRED E. WRIGHT.

Introduction.

(Read April 25, 1914.)

Some years ago, Mr. D. Foster Hewett, in a paper[1] on the remarkable vanadium locality of Minasragra, Peru, described briefly certain oxidation products of the vanadium sulphide ore, patronite, which he was inclined to regard as vanadic acids, although the opinion of one of us (H.), based on preliminary analyses, was that two of the minerals were calcium vanadates.

Several years later the chief constituent of a certain red ore of vanadium from Paradox Valley, Montrose County, Colorado, was identified (H.) as a calcium vanadate, seemingly identical with one of those from Peru. Since then this red ore has been found over a wide area, extending into Utah. A good deal of additional chemical work has been done intermittently during the past three years upon material of both occurrences and it has been studied microscopically. It has developed that, although the minerals are deceptively alike in appearance and general behavior and have the same empirical formula, they seem to be specifically distinct, are probably in fact isomers. The Peruvian mineral, the first known and studied, we are pleased to name *hewettite,* after Mr. D. Foster Hewett, now of the U. S. Geological Survey, who has done so much to make the Minasragra occurrence known. Its isomer may appropriately be called *metahewettite.* It is probable that hewettite occurs also in Paradox Valley (see under metahewettite, pp. 37–38).

The detailed results of the several authors' work are submitted in the following pages, a preliminary announcement having appeared in the *Jour. Washington Acad. of Sci., 3,* 157, 1913.

[1] *Trans. Am. Inst. Min. Eng., 40,* 291, 1909.

Hewettite.

Hewettite was rather abundant at the time of Mr. Hewett's visit to Minasragra. It is wholly of superficial occurrence, derived by oxidation from the vanadium sulphide, patronite. The principal specimen examined by us was a lump about the size of a small apple.

In the lumpy aggregates of pure mineral hewettite is deep red (mahogany red)[2] with a somewhat silky luster. Under the microscope the needles measure usually less than 0.01 mm. in width and 0.2 mm. in length. The extinction is parallel. The refractive indices measured by immersion methods could be only approximately found because of the extreme thinness of single blades and the lack of entire parallelism of the blades in a group. Also, β and γ were so high that slight heating was necessary to embed the mineral in the standard refractive media, thus causing expulsion of an unknown amount of water. For Li-light $\alpha = 1.77$, $\beta = 2.18$, $\gamma =$ about 2.35 to 2.4. Elongation is parallel to γ. Pleochroism is strong; γ dark red, α and β very light orange-yellow. The mineral is probably orthorhombic. A determination of density, made by Mr. E. S. Larsen, on air-dried material gave a value 2.618. A subsequent determination by one of the authors (M.), using material containing 9 molecules of water, gave 2.554. The apparent discrepancy between these two determinations is explained by the fact that the material used by Mr. Larsen was partially dehydrated.

The mineral when heated passes through various color changes (see p. 46) and melts readily, forming a dark red liquid; it is slightly soluble in water.

The composition of an almost pure specimen, on which the foregoing optical examination was made, is given under analysis I (p. 40). Analysis I*a*, in the footnote on pp. 40–41 (quoted by Mr. Hewett, loc. cit., p. 311), represents a lump of ore showing little evidences of crystallization but otherwise resembling closely the better specimen, although the microscope shows it to be far from homogeneous. In spite of the similarity in appearance of the two specimens, the

[2] The specific color terms used in these descriptions are based on comparisons with Ridgway's standards. See "Color Standards and Color Nomenclature," Robert Ridgway, Washington, D. C., 1912.

analyses show quite different percentages of lime. This is not surprising if we conceive that the vanadium of the patronite has become oxidized to a polybasic acid of quinquivalent vandium, which then was gradually neutralized by calcium. It seems reasonable to expect that ores of all gradations occur from the sulphide patronite to the half neutralized salt hewettite and finally to the fully neutralized salt represented by pascoite (p. 49). Indeed, such intermediate stages are probably represented by a number of the specimens brought from Peru by Mr. Hewett and of which a few analyses are given in his paper. These ores are, for the most part, microcrystalline, though some show distinct evidences of crystallization, as Mr. Hewett pointed out. They are of varying colors, from red through greenish to the black of the original patronite. Some of the specimens are not fully oxidized[3] but contain vanadium in a lower state of oxidation than corresponds to quinquivalency, and even free sulphur. Some of them are characterized by high iron content and relative freedom from lime, as shown by the following analysis,[4] which seems to represent essentially a ferric vanadate.

V_2O_5	57.3
V_2O_4	4.8
MoO_3	3.3
Fe_2O_3	19.6
TiO_2	.1
SiO_2	.6
CaO	.7
H_2O	13.9
	100.3

Molybdenum is naturally a characteristic component of the oxidation products of patronite since it occurs in the patronite ore.

Metahewettite.

Unlike the Peruvian mineral, the North American vanadate is an impregnation in sandstone, generally coating the sandstone grains, sometimes filling cavities and crevices. The specimens are almost

[3] Analysis I indicates that oxidation is not yet quite complete in our best specimen.

[4] Made in 1907 in the laboratory of the U. S. Geological Survey and quoted by Mr. Hewett, *loc. cit.*, p. 311.

always friable, some falling to powder. Gypsum frequently accompanies the metahewettite and often encloses it, producing then the appearance of a distinctly crystallized red mineral. When pure the powder is dark red; gypsum present lightens the color. Even when free from gypsum the red vanadate is almost always associated with other minerals, partly residuals from the impregnated sandstone, although occasionally almost pure material is found, like that represented by analysis II (p. 40). The impurities interfered greatly at first with the precise determination of the composition of the vanadate, particularly as to its water content. For this reason no quantitative analysis of the mineral from Paradox Valley is given, although an abundance of the ore was at our disposal and one or more analyses of it were made before purer material, from Thompson's, in eastern Utah, was obtained. These analyses made evident, however, the chemical identity of the Paradox and Thompson's minerals.

The ore at our disposal from Thompson's differs somewhat from that of Paradox Valley by a greater variation in its shades of red, some of these being very bright in contrast with the usually duller shades of the ore from Paradox.[5] There are also associated with it at least two interesting minerals, both of which were also noticed later in ore from Paradox and the Henry Mountains.

One of these, gray in color, is a hydrous silicate of aluminum, trivalent vanadium and potassium. It is no doubt the same silicate that was first noted by one of us (H.) in carnotite ores and seems to be a constant associate of all the uranium and vanadium ores of western Colorado and eastern Utah, in some places constituting the chief vanadiferous component of the ore. In the ore from Thompson's it forms soft patches throughout the red mass, some of which are of sufficient size to permit of separation in a fairly pure state.[6] Per-

[5] Ores of deep color have been found recently by Mr. Frank L. Hess, of the U. S. Geological Survey, in the Henry Mountains, Utah.

[6] The gray mineral accompanying metahewettite occurs in firm granules consisting of aggregates of very minute doubly refracting particles which cannot be isolated for microscopic study. The refractive indices of aggregates from various portions of the ore varied between 1.59 and 1.64, apparently indicating differences in composition. Some larger lath-shaped particles

haps the greenish tints of some of the Paradox ore specimens are caused by this or another related silicate, like the roscoelite from Placerville, Colorado.[7]

The second characteristic mineral is selenium, in amount up to one per cent. of the ore from Thompson's. It seems to be included as specks in the gray silicate. It is entirely absent from the specimens of Paradox ore first obtained, but is present in ore from the Henry Mountains, Utah, and in some small specimens seen recently that were said to come from Paradox Valley. The presence of selenium can be detected by heating the ore in a glass tube closed at one end, when a red sublimate (sometimes accompanied by a white one of selenium dioxide) appears when most of the water has escaped. The fact that the free element appears as a sublimate does not by itself prove the existence of the selenium in the free state in the ore, for there was enough organic matter present, in a state invisible to the eye, to reduce an oxygenated compound of selenium if present. But the weight of evidence points to its presence in the elemental state and not as a selenide or oxygenated compound.[8] No connection was observed between the presence of the selenium and the bright red color of some specimens of the ore. The differences in shades of red are attributed to differences in physical condition of the metahewettite and to the effect of associated minerals.

were observed, possibly pseudomorphs, containing abundant dark inclusions of more or less prismatic shape arranged parallel to the laths. These laths are aggregates, but portions of them seem to have a definite orientation with respect to the outlines, extinguishing parallel and having γ parallel to their lengths. The inclusions were selenium and bituminous matter. This gray material is probably not roscoelite. For purpose of comparison a study was made of the properties of the roscoelite from Placerville, California, probably identical with that previously analyzed by Hillebrand (*Am. J. Sci.*, *7*, 351, 1899; Bull. U. S. Geol. Survey, No. 167, p. 70, 1900).

Optical properties of roscoelite: Color, deep green with almost metallic luster. 2E variable between 60° and 75° or more. Optical character—. $\gamma = 1.680–1.685$, $\beta = 1.675–1.680$.

[7] Hillebrand and Ransome, *Am. J. Sci.*, *10*, 120, 1900, Bull. U. S. Geol. Survey, No. 262, p. 18, 1905.

[8] The mineral presents deep red transparent prisms, up to 0.05 mm. long, showing parallel extinction. This characterization fits one of the known forms of selenium. Sublimation tests on a few specks indicated free selenium. It seems to be insoluble in carbon bisulphide. So far as known this is the first established occurrence of elemental selenium in nature.

Nearly all of our ore specimens from Paradox Valley, Thompson's and the Henry Mountains were free from uranium minerals, but the complexity of the metahewettite ores under special conditions is well illustrated by a very small specimen from Paradox. In addition to constituents indistinguishable to the eye, this showed in juxtaposition and much commingled, metahewettite, carnotite, a brownish material rich in uranium and resembling some forms of ferric phosphate, and jet black, lustrous bituminous or coaly matter. This last, if uraniferous, is perhaps the unnamed mixture of which a preliminary notice by Karl Kithil appeared in *Science, 38*, 625, 1913.

Fortunately several small lumps of very pure material from Thompson's were found. This material gave only a very faint reaction for selenium and was otherwise almost free from contamination. Upon it analysis II. is based.

Metahewettite crystallizing with 9 molecules of water occurs in two typical habits with intermediate forms. The purest material from Thompson's is a feebly lustrous, loose, earthy powder; that from Paradox Valley appears chiefly in compact aggregates of separable, shining blades, though the earthy variety also is found here. Both are deep red, but on account of the larger size of its bright reflecting surfaces the bladed variety appears lighter colored. When powdered the bladed variety is claret brown, the earthy variety is dark maroon. The color of the ores containing the mineral varies greatly because of admixed minerals; furthermore, variation of the water content of the mineral produces changes in color (see p. 46).

Microscopically the earthy variety consists of minute sharply bounded tables about .04 mm. long, piled in subparallel groups. The outlines and optical properties indicate orthorhombic symmetry. The compact variety consists of plates like those in the earthy variety, closely joined in parallel or radiating, more fibrous aggregates. The optical properties are more easily determined on these large aggregates. Pleochroism is strong in groups seen edgewise, but is scarcely noticeable in the plane of the tables. α is light orange-yellow, β deep red, γ deeper red. Two optic axes barely come into the field of a No. 12 objective over a condenser immersed in oil. 2E thus measured is about 135°. The plane of the optic axes is parallel to the

elongation. The refractive indices α and β were determined with difficulty. α was obtained from groups of crystals seen edgewise; β is so high that it could barely be matched without heating by immersion in a mixture of tin iodide, methylene iodide and the compound of arsenic sulphide and methylene iodide. γ could not be obtained except after expelling water from the mineral by heat.

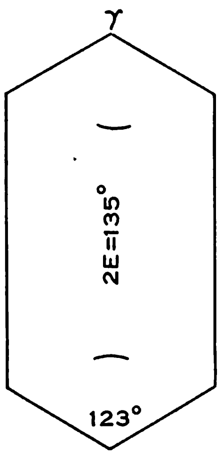


FIG. 1. Optical orientation of metahewettite.

For Li-light $\alpha = 1.70$, $\beta = 2.10$ and 2V (calculated) $= 52°$. α is the acute bisectrix. No measurable differences between the optical properties of specimens from different localities were found. Fig. 1 shows the optical and crystallographic relations. The axial ratio $a:b = .54:1$.

The density of the mineral containing 9 molecules of water—determined in xylol—is 2.511; after the loss of 6 molecules of water it is 2.942. The loss of this water is not accompanied by any perceptible breaking down of crystal structure. The same fact was observed with hewettite. Like hewettite, metahewettite melts readily and is slightly soluble in water. The solubility, slight as it is, affords a means, by the use of much boiling water, of separating from metahewettite the associated minerals mentioned on pages 34 and 35.

Differing strikingly in external aspect from the ores described

above is a single specimen from Paradox Valley, shown us by Mr. Frank L. Hess, of the U. S. Geological Survey. This consists of a single bundle of interwoven fibers implanted on a layer of crystallized gypsum. This specimen, on account of its markedly fibrous structure, bears a closer resemblance to the best of the hewettite from Peru than does the mineral described above. Indeed, optical evidence shows that it is hewettite.

Analyses of Hewettite and Metahewettite, and Their Discussion.

Long after the first analysis of hewettite was made, it was found that both it and metahewettite are extremely sensitive to atmospheric changes in humidity, especially within a certain narrow range. It was therefore essential that they be brought always to the same definite state of saturation with respect to water.[9] It was found by experiment that this condition could be satisfactorily attained by exposing the mineral powder at a definite temperature over sulphuric acid of vapor tension near that of pure water until equilibrium was established. The strength of acid over which the mineral was placed

[9] Failure to observe this precaution may lead to serious error in establishing a probable formula for minerals with variable water content. For instance, if, as with hewettite and metahewettite, the mineral is analyzed in air-dry condition when the air humidity is high, a very different result will be obtained than when the air is dry. The variations in moisture content of these minerals when left exposed to the air may vary 8 or 9 per cent. between September and December in Washington. This is not efflorescence as usually understood, for although the reaction reverses itself with return of humidity the loss of water is not accompanied by breaking down of the crystal structure.

Calcio-carnotite from Colorado (the tyuyamayunite (?) of Nenadkevich) and probably also the original carnotite (essentially the potassium salt) show similar wide differences in water content at different seasons of the year. This may be and probably is true also of other minerals. If so, an explanation is afforded of some of the conflicting statements of different analysts in regard to the water content of certain minerals.

It is, further, important to make a series of exposures over sulphuric acid of increasing concentrations, up to the maximum, at a fixed temperature, and then to carry out tests at rapidly increasing temperatures, in order to detect, by the losses at each step, the number of hydrates that may exist, and also, if possible, what proportion of the water may be differentiated from water of crystallization or of absorption. The curves resulting from such tests with these minerals are shown in Fig. 2.

preparatory to analysis was chosen after a preliminary trial had shown about the range of highest vapor pressure over which little change in water content took place. Sulphuric acid of sp. g. about 1.105 (15°) was found to give the desired vapor pressure, about nine tenths that of saturation.[10] Two different temperatures were employed for establishing equilibrium over this acid in a thermostat, namely, 25° in winter and 35° in summer (temperatures most easily maintained).

At 25° the water-vapor tension of this acid is about 21.8 mm. or 2 mm. less than that of pure water at the same temperature, and 38.8 mm. at 35°, or about 3.5 mm. less than that of pure water. Upon material thus brought to a definite water content the analyses were made. One portion was used for studying the course of dehydration, first by exposing the mineral at 25° (or 35°) until equilibrium was reached, successively over sulphuric acid of concentrations that corresponded to lowering of the vapor tension by tenths approximately and finally over phosphorus pentoxide.[11] With the

[10] The tables of Domke and Bein were used in this connection (*Z. anorg. Chem.*, *43*, 176, 1905).

[11] The value of conclusions deducible from such a series of fractionations on minerals of the kind in question depends on careful observance of certain precautions. In the first place, the mistake must not be made, as in the present case, of using different temperatures for the initial saturation. It was not expected that our tests would extend from winter into summer, as they did at intervals, thus necessitating the use of two temperatures. Of course, the vapor tension of an acid of given strength is markedly greater at 35° than at 25°. The initial water content of the mineral may therefore differ and the results of tests started at the two temperatures not be strictly comparable. For, although the mineral is under a greater water vapor tension of the acid at 35° than at 25° and might therefore perhaps take up more water than at the lower temperature, it is probable that the higher temperature will have its effect on the mineral also but in a quite indeterminate degree (see Fig. 2 and p. 44 of text, for an instructive illustration of one effect of temperature differences, as shown by curves I. and II.). In the second place, at the conclusion of any one test of a series the mineral must not be allowed to cool in the desiccator after removal of the latter from the thermostat, but must be taken out of it at once and inserted quickly into a capped weighing vessel before it has time to cool, for if allowed to cool before removal the final condition will approach that of room temperature and not be that of the experiment. Moisture may be condensed on the mineral and its containing vessel and the former may perhaps reabsorb water in

lower vapor pressures a vacuum was employed. The loss observed at the end of the last exposure was practically identical with that occasioned by a temperature of 100° for a few hours in dry air. Then the fractionation was continued at temperatures above 100°. The results of some of these tests will be given later (pp. 44–45).

The following table of analyses does not show all the determinations that were made. In fact, the first complete analysis of hewettite is omitted, because it was made before the need for bringing the mineral to a definite saturation as to water content was realized. That analysis confirmed, however, in all other essentials than water, the data contained in analysis I. below.

ANALYSES OF HEWETTITE AND METAHEWETTITE.

	Hewettite,[12] Peru, in Equilibrium with Water-vapor Tension of 21.8 mm. at 25°. I.	Metahewettite, Utah, in Equilibrium with Water-vapor Tension of 38.8 mm. at 35°. II.	
V_2O_5	68.19	70.01	
V_2O_4	1.21	——	
V_2O_3	——	.35	
MoO_3	1.56	.13	
CaO	7.38	7.25	7.25
MgO	none	.03	
K_2O	none	.09	
Na_2O	.15	.08	
H_2O (total)	21.33	21.30	(mean of 21.24, 21.31 and 21.34)
Fe_2O_3, etc.	.11	.19	
SiO_2	——	.80 (SiO₂ and Insol.)	
Insol.	.17		
	100.10	100.23	

Trace Li in I., none in II. A very little Cl in I. and II. Trace P_2O_5 and Se in II. No Ba or Sr found in either. V_2O_4 and V_2O_3 assumed, their amounts measured by consumption of permanganate when the mineral was dissolved in dilute sulphuric acid (see discussion of molecular ratios, p. 41).

addition to that condensed on its surface. In the third place, in cases when a vacuum is employed this should be relieved by air bubbling through acid of the same strength and temperature as that in the desiccator, if possible while the latter is still in the thermostat.

[12] An earlier analysis of more compact material from Peru, devoid of crystalline appearance, gave the following results:

Molecular Ratios.

In calculating molecular ratios for the minerals one is confronted with difficulties arising from the presence of vanadium of lower valence than 5, of molybdenum, and of small amounts of constituents other than calcium. These different problems will be taken up in order.

Vanadium.—In hewettite the presence of vanadium in the quadrivalent state may be regarded as probable in view of its existence in many of the specimens representing much less complete oxidation of patronite. In these cases it may be assumed with considerable probability that there exist vanadyl-vanadic oxides or salts, since artificial compounds of the kind are known. If a compound of this nature exists in hewettite, it demands a portion of the V_2O_5, and, if hydrated, a considerable percentage of water.

In metahewettite, however, there is great reason to believe that the vanadium of lower valence is trivalent. That it exists as a constituent of a silicate containing also aluminum and potassium was pointed out on p. 34. It will be so regarded. This stand is taken with full knowledge that a characteristic black ore from Paradox contains much of its vanadium in the quadrivalent state, as mentioned in our preliminary paper.[18]

Molybdenum.—The presence of molybdenum hampers some-

	Ia. Per Cent.	Mol. Ratio,
V_2O_5	66.8	4.79
V_2O_4	.7	
MoO_3	2.8	.26
CaO	4.3	1.00
H_2O (100° —)	13.9	10.03
H_2O (100° +)	6.9	4.97
Fe_2O_3, etc.	3.3	
SiO_2	1.2	
	99.9	

This analysis is given chiefly to show how deceptive the evidence afforded by sharp molecular ratios may be, for microscopical examination showed the material to be very far from pure. It also serves to show what different compositions similar appearing materials may have.

[18] *Jour. Washington Acad. Sci.*, *3*, 158, 1913.

what the drawing of conclusions as to the formulas assignable to the vanadates, since we know nothing positive as to its chemical condition and whether foreign to the vanadate molecule or a part of it, nor, if foreign what part if any of the water is to be assigned to it. So much may, however, be affirmed with positiveness from careful microscopical examination, that the sample of hewettite analyzed represents essentially a single homogeneous mineral and not a mechanical mixture of different minerals. This belief is supported by the behavior of the mineral when it is gradually brought into complete solution by successive treatments with much hot water, for the solution of the molybdenum keeps pace with that of the vanadium. The molybdenum may perhaps best be considered as forming calcium molybdate which is held in solid solution. This assumption has been made because it seems called for by the varying proportions of molybdenum in different specimens and by the difficulty of deducing a probable formula under any other assumption.

Other Constituents.—The absence of any acidic constituent in hewettite to offset the sodium forces us to group this with the calcium as part of the vanadate molecule, unless perchance there be an admixture of a vanadyl-vanadate (see p. 41). In metahewettite the potassium may be referred with a high degree of probability to the silicate of which mention has been made. This silicate requires a small part of the water. There is no evidence of such a silicate in the Peruvian mineral. The sodium and magnesium of metahewettite are not accounted for, but in part at least may belong to the silicate mentioned or to another, except in so far as the chlorine present in small amount may claim some of the sodium (also in hewettite). The amounts reported for sodium may be subject to considerable error in both analyses, and if in error are too high. The iron oxide is no doubt admixed.

After deducting MoO_3 and its equivalent of CaO and neglecting V_2O_4, V_2O_3 and all other minor constituents except Na_2O in I., the molecular ratios deducible with employment of the 1913 atomic weights are:

	I.	II.
V_2O_5	3.06	3.00
CaO	1.00	1.00
H_2O	9.61	9.20

The value for V_2O_5 in I. becomes 3.00 if enough is deducted to form an equi-molecular compound with the V_2O_4.

Before discussing the formulas of the minerals we must present a number of considerations bearing on their specific differentiation, in the course of which certain experimental data essential to a proper understanding of the subject will be given.

Evidences of Specific Distinction of Hewettite and Metahewettite.

The evidence which impels us to give different names to the Peruvian and North American minerals, in spite of the fact that they seem to have the same empirical formula, will now be set forth.

Optical and Crystallographic Differences.—Metahewettite is

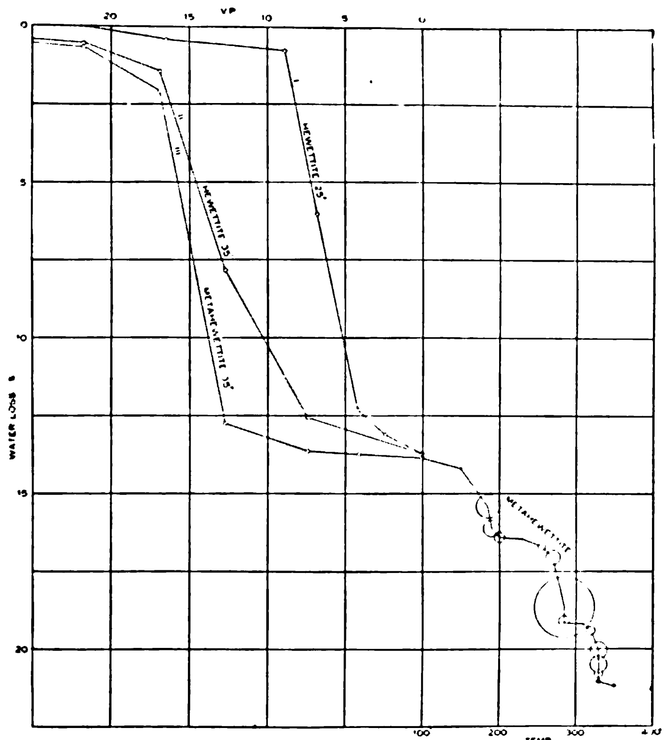


Fig. 2. Showing the course of dehydration of hewettite and metahewettite over sulphuric acid of different concentrations at room temperatures, and of metahewettite at temperatures above 100°.

darker red than hewettite and has two directions of strong absorption for light, while hewettite has only one. Metahewettite crystallizes in well-defined tabular forms or broad blades, hewettite in slender blades.

Loss of Water at Room Temperature.—There is a real difference in the behavior of the two minerals during progressive dehydration over sulphuric acid. Reference to Fig. 2 (curve III.) shows that at 35° metahewettite loses 10.65 per cent. of water in one drop from 17 mm. to 12.7 mm. vapor tension, nearly as much as hewettite loses in the two stages from 17 mm. to 7.4 mm. (curve II.). The points of observation shown in the several curves of the figure in almost all instances represent 24-hour exposures and are approximately equilibrium points. It was unexpectedly found that equilibrium at these points was practically attained in one day, for even after several days' exposure the losses in weight were seldom appreciably greater than after one day. This fact seems to support the argument that the curves indicate a real difference between the two minerals.[14] The curves shown are as a rule quite closely reproducible with different samples of mineral.

The very important effect of varying temperature at a fixed vapor tension of water in the desiccator is strikingly brought out by comparing curves I. and II. for hewettite. It is seen, for instance, that at 35° and 12.7 mm. vapor pressure the mineral loses nearly 8 per cent. of water against a little over half of 1 per cent. at 25° and the same pressure. Again, curve I. shows an equilibrium point at 6.05 per cent. water, curve II. at 7.85 per cent. To the question of why the two points do not appear in the same horizontal line if we are dealing with water of crystallization, two answers suggest themselves: Either (1) an intermediate hydrate which forms at 25° may not have appeared at 35° or, (2) the concentrations of acid used

[14] The lines connecting points of observation in the figure do not signify that the rate of loss was uniform for each unit-lowering of vapor pressure for the interval between two points. The actual loss for each interval would probably be more correctly expressed by a vertical line corresponding to some vapor pressure intermediate between those of the actual points of observation. Just where this line should fall might be determined by making observations with acids of smaller variation of vapor pressures and allowing more time for the attainment of equilibrium.

were not sufficiently close together to indicate all the hydrates formed.

Loss of Water When Heated Above 100°.—Other differences become apparent when the minerals are heated up to the point of losing the last molecule of water. The experiments showing this were made at the geophysical laboratory, the heating being done in an oil or a nitrate bath, both in an open tube and in tubes which were evacuated from time to time, and the loss determined by weighing the tubes.

Metahewettite, after losing 13.8 per cent. (six molecules) of water over strong sulphuric acid, was first heated gradually from 100° up to 350° during one hour with weighings at frequent intervals. Water was expelled abundantly at three stages near the temperatures 185°, 275°, and 340°. The loss at each stage as determined by these weighings and by repeating the heatings and weighings on another sample at favorable temperatures, was found to correspond to about 2.3 per cent., or one molecule of water at each stage[15] (see Fig. 2, lower portion). The sizes of the circles represent the loss of water at each heating divided by the time of the heating. This is the rate of loss of the water.

Hewettite, on the other hand, exhibits only one well-marked loss in weight when heated rapidly. Near 250° a loss corresponding to two molecules of water was observed. Near 300° and 350° there are evidences of increased rate of loss if the temperature is increased rapidly in 30 minutes. These losses correspond to one-half molecule each. These data are not shown in the figures.

[15] This method of rapid heating and frequent weighings affords surer indications of the existence of water in different stoichiometrical proportions than that of holding the mineral in dry air for a long time at successively increasing temperatures. By either method a rate of dehydration, not a condition of equilibrium, is measured. Fig. 2 shows two distinct breaks in the rate of water loss for metahewettite when it is heated rapidly as described in the text. The curves plotted for 5-hour periods of heating in a dry-air current (not shown in the figure) are without distinct breaks.

The temperatures at which water escapes most rapidly bear little relation to those employed in slow heating. Long heating results in a higher loss at a given temperature, and in complete dehydration at a much lower temperature, than very rapid heating.

Change of Color Due to Heating.—Metahewettite, dark maroon in color at first, becomes progressively darker red till the last molecule of water begins to escape, then the color becomes gradually lighter and finally yellow-brown. Concurrent with the last change is a breaking up of each crystal into a crystalline aggregate which retains the form of the original crystal.

Hewettite also darkens when water is lost. At the start, with nine molecules of water, it is mahogany red. The hydrate with three molecules is between carob-brown and liver-brown, the compound with one molecule chestnut-brown, and the anhydrous powder medal-bronze. No breaking down of structure was observed till the last molecule of water was lost.

Changes in Weight and Color after Dehydration.—The powders of both minerals were exposed after complete dehydration to sulphuric acid of sp. g. 1.10. Metahewettite from Utah, after dehydration by heating in air, regained its original weight and color in a few days, but after treatment under greatly reduced pressure the color was not restored, even after moistening, although the observed loss of weight had been the same in both cases. Hewettite, on the other hand, after heating to only 270° and while it still retained one molecule of water, neither regained its original color nor quite its original weight when placed in moist air. In one experiment, after heating to 350° and losing 20.75 per cent. in weight, it regained 19 per cent., but on again fractionating over the acids first used the rate of loss and the amount lost at each step were markedly different from those noted in the first fractionation.

Probable Formula.

Manifestly the empirical formula indicated for metahewettite nearly saturated with water at 35° is $CaO, 3V_2O_5, 9H_2O$. What it may have been when the minerals were originally deposited can not at present be said, although from the curve for water content under varying hygrometric conditions at summer temperatures (Fig. 2), which shows only very slight changes when the humidity is high, it might be argued that the limit of hydration has been reached with nine molecules of water. The effect of low temperatures is,

however, unknown, and it may be that the original water content was higher.

The same statements apply to hewettite, but here the excess of 0.6 molecule of water above nine molecules seems to be real, since it is by no means accounted for by the fraction of one per cent. represented by the nearly horizontal upper part of the curve, which fraction might be considered hygroscopic or absorbed water.[16] It is conceivable that this excess is connected with molybdenum, but much more likely that it has to do with a vanadyl-vanadic compound as already suggested (p. 41).

For the moment we will assume that the formula of both minerals, when holding the maximum amount of water, is CaO, $3V_2O_5$, $9H_2O$. Of what acid, then, are they salts?

The ratio of CaO to V_2O_5 shows that they cannot be salts of orthovanadic acid. Moreover, the known orthovanadates are very few in number and exhibit little stability, but pass readily into hexavanadates. From the fact that six of the nine molecules of water are quickly removable at ordinary temperatures in dry air and the others are much more firmly held, it might seem justifiable to assume six molecules of water of crystallization and three of constitution. Such disposition of them necessitates derivation of the minerals, as quarter-saturated salts, from the hypothetical acid $H_8V_6O_{19}$, an octobasic hexavanadic acid, a possible derivative of orthovanadic acid. We are confronted, however, with the fact that neither such an acid nor salts of it are known. Hexavanadates, however, derived from the tetrabasic acid $H_4V_6O_{17}$, also a derivative of orthovanadic acid, have been described and they resemble in general the two minerals in question, so far as can be determined from the meager data available.

Tetrabasic hexavanadic acid offers the possibility of two isomers of a salt of a bivalent metal. It seems then necessary to consider these minerals as acid salts of this acid, and the name metahewettite for one of them is not only justified but appropriate. If this reference is proper, only one molecule of water of constitution is possible

[16] Not all of the hygroscopic or absorbed water was necessarily removed in the first part of the dehydration over sulphuric acid.

and the other eight are water of crystallization. Against two of the eight being regarded as water of crystallization might be advanced the difficulty of removing the seventh and eighth molecules, but this can not be considered a weighty argument. Neither can the fact that the water content is so markedly affected by outside humidity and is susceptible of repeated removal and restoration be brought as a conclusive argument against the assumption of water of crystallization, for it must be remembered that loss of water is not accompanied by rupture of the crystalline structure, as is usually the case with true hydrates. On the other hand, in favor of water of crystallization, as opposed to water of absorption, must be placed the breaks in the curves of dehydration observed when the minerals are rapidly heated above 100°, and the fact that the content of water at a fixed atmospheric temperature does not bear a continuous relation to the outside humidity.

On the whole we are disposed to adopt the view that eight of the nine molecules represent water of crystallization and to report the formulas of both minerals as examined by us to be $CaH_2V_6O_{17} \cdot 8H_2O$. Under natural conditions mixtures of this and another salt of much lower hydration may and do often occur.

Inspection of the formulas of the artificial alkali and alkaline-earth hexavanadates throws no light on this problem, since the range in number of molecules of water is very wide for the normal alkali salts and the number is reported as 14 for the normal barium salt and 9.5 for the normal magnesium salt. Acid salts of bivalent metals do not seem to have been prepared, unless Ditte's calcium "trivanadate" $CaO,3V_2O_5,12H_2O$, is such a salt, perhaps identical with one of the minerals described by us. Its description, however, does not fit our minerals, since it is reported as very soluble in water and as having the luster of gold. The evidence of the salts mentioned points, however, to the possibility that hewettite and metahewettite may have held more than nine molecules of water when formed, unless the fact that only slight changes occur in their water content with high atmospheric humidity at summer temperatures negatives such a possibility.

Under the name alaite K. A. Nenadkevich has given a very brief

description[17] of a dark red, silky, soft and dense, moss-like mineral to which he assigned the formula V_2O_5,H_2O. The description fits hewettite very well in the main and it will be of interest to learn if on further study alaite may not prove to be a calcium vanadate related to, if not identical with, hewettite or metahewettite. Alaite is one of a number of vanadium and uranium minerals occurring in the Province of Ferghana, Russian Turkestan.

Pascoite.

Not observed in the surface deposit at Minasragra, Peru, but formed on the walls of an exploratory tunnel since its excavation, is a mineral representing a further stage of neutralization by calcium than is shown in hewettite. The specimens examined were among those brought from Peru by Mr. Hewett and the analysis given below was made several years ago in the laboratory of the U. S. Geological Survey. The name proposed, pascoite, is from Pasco, the province in which the locality of occurrence lies.

Crystallographically, this mineral is unsatisfactory, since it occurs only in minute grains and clusters of grains, arranged in a way indicative of a crustaceous deposit—as though they were secondary in origin and had been precipated as a crust about preëxisting masses. No well-developed crystals suitable for goniometric measurement were observed, and only here and there in the crystalline aggregates were minute crystal faces seen. No distinct cleavage was noted, although occasionally indications of an imperfect pinacoidal cleavage were observed in grains under the microscope. The fracture is conchoidal. In color this mineral ranges from dark red-orange to yellow-orange; the more homogeneous masses being uniformly red-orange throughout. In thin flakes it is translucent and the clearer individuals are vitreous to subadamantine in luster, especially on flat crystal faces which glisten here and there in the aggregate. The streak is cadmium-yellow and the hardness about 2.5. The specific gravity is about 2.457, determined in methylene iodide and benezene, on the clearest and most homogeneous material. This value may be slightly low because a crystalline aggregate, instead of a single crystal, was

[17] *Bull. Acad. Sci. St. Petersburg,* p. 185, 1909.

used, and minute air spaces therefore may have been present between the grains.

In the powder form the grains are usually irregular in shape and colored more or less intensely in shades of orange, red, and yellow. Pleochroism is noticeable; α = light cadmium-yellow; β = cadmium yellow; γ = orange. Absorption $\gamma > \beta > \alpha$. The refractive indices were determined by the immersion method; mixtures of methylene iodide, arsenic tribromide and arsenic sulphide (realgar) being used for the purpose. Owing to the color of the mineral, however, the phenomena on which the refractive index determinations by this method rest were less sharply marked than usual, and the probable error is correspondingly larger.

$$\alpha = 1.775 \pm .005$$
$$\beta = 1.815 \pm .005$$
$$\gamma = 1.825 \pm .005$$

The birefringence is strong and gives rise to high interference colors, even in comparatively thin grains. By direct determination, $\gamma - \alpha$ was found roughly to be about .050. The optic axial angle was measured by the double screw micrometer ocular[18] on sections showing only one axial bar and also on a section normal to the acute bisectrix. For sodium light 2V was found to be $50°.5 \pm 1°$ or 2E is about 100°; for lithium red light 2V is about $56° \pm 3°$, or 2E, about 115°. The determination in lithium light was much less satisfactory and accurate than that in sodium light. The dispersion of the optic axes is very considerable with $2V_{Li} > 2V_{Na}$ and its effect is clearly marked in the interference figure. The appearance of the interference figure shows, moreover, remarkably strong crossed dispersion—so strong in fact that in white light a section normal to the acute bisectrix never extinguishes completely, but near the position of extinction for light of any wave-length shows abnormal interference colors in characteristic tones, especially of green and orange. On a section nearly normal to the acute bisectrix the position of total extinction for sodium light made an angle of about 8° with that for lithium light. This angle, $\gamma_{Li}:\gamma_{Na} = 8°$, is only ap-

[18] *Am. J. Sci.*, *24*, 317–369, 1907.

proximatetly correct, and may be several degrees in error, owing to the weakness of the lithium light source used, and consequent lack of sharpness of position of total extinction.

The above optical data indicate that this mineral is in all probability monoclinic in crystal system, with its optic axial plane normal to the plane of symmetry. To summarize, the determinative optical characteristics of this mineral are: Crystal system, probably monoclinic; axial ratio, unknown; cleavage, poor and probably after 010. H, about 2.5; sp. g., about 2.46. Color, dark red-orange to yellow-orange; luster, vitreous to sub-adamantine. Pleochroism, noticeable, γ = orange; β = cadmium-yellow; α = light cadmium-yellow. Absorption, $\gamma > \beta > \alpha$. Refractive indices, $\alpha = 1.775 \pm .005$; $\beta = 1.815 \pm .005$; $\gamma = 1.825 \pm .005$.

Birefringence is strong. $2V_{Na} = 50°.5 \pm 1$. $2E_{Na}$ about 100°. $2V_{Li} = 56° \pm 3°$.; $2E_{Li}$ about 115°. Dispersion, crossed and strong, Optical character —. Plane of optic axes normal to plane of symmetry.

On the whole, the material is homogeneous and comparatively free from inclusions and suitable for chemical work. Here and there foreign material was observed, but in the material selected for chemical analysis it was not present in sufficient quantity to veil seriously the chemical relations.

Pascoite melts readily, forming a deep red liquid, and is easily soluble in water.

Analysis of Pascoite.

	Per Cent.	Mol. Ratio.
V_2O_5	64.6	3.18
MoO_3	.3	——
CaO	12.6	2.00
H_2O 100° —	13.8	6.87
H_2O 100° +	7.8	3.88
Undet. and loss	.9	——
	100.0	

The ratios are not as satisfactory as could be desired. The values approach those required from the formula $Ca_2V_6O_{17} \cdot 11H_2O$, which calls for: V_2O_5, 63.76; CaO, 13.10; H_2O, 23.14. Very recent tests show that almost no loss of water occurs at room temperatures until

the surrounding humidity is reduced practically to zero. When dehydrated over P_2O_5 the color is no longer orange but dirty yellow. After rehydration in moist air the color is much brighter yellow, but without any trace of the original orange.

As with hewettite and metahewettite, the amount of water evolved at and below 100° is almost exactly removable by exposure over strong sulphuric acid at room temperature for one or two months and much more rapidly in a vacuum. Above 100° further loss begins, but is complete only at a temperature of perhaps 300°. No experiments have been made as yet to trace the progress of dehydration at temperatures above 100°. Further tests on this mineral are needed and will be made if opportunity offers.

The arguments advanced (pp. 46–48) for hewettite and metahewettite with respect to their chemical classification apply to pascoite also. In this case, if the assumptions made for the former minerals are justified, we have normal calcium hexavanadate with x molecules of water, at least 11 when the mineral was formed.

Analytical Procedure.

The methods of analysis need no special mention except as to the separation and determination of the vanadium and molybdenum. As a rule the portions used for water determinations served also for the other constituents. They were treated in a glass tube, with dry hydrochloric acid gas, after solution in nitric acid and evaporation to dryness in a porcelain boat on a hot plate. The brown vapors were collected in receptacles containing a little water. Two of these in series were sufficient, but a third was sometimes used, all so connected that no back suction of liquid was possible if the gas stream slackened. The material in the boat is attacked instantly the acid vapor reaches it, even without the aid of heat, but the reaction is not complete in one operation even when heat is applied after vigorous action ceases. It is necessary, usually, to remove the boat, to reconvert the contents to nitrates and to repeat the treatment with hydrochloric acid gas several times, and to wash out and dry the glass tube between each operation.

The molybdenum, less volatile than the vanadium, comes off only,

or at least for the most part, during the later periods and upon heating. If present in some quantity it reveals itself by a white crystalline deposit in front of the boat.

The contents of the receptacles and of the tube were finally evaporated in porcelain with sulphuric acid, which was then heated till fumes arose. After dilution, the deep blue solution was transferred to a flask, saturated with hydrogen sulphide gas, and heated while the gas still passed. The flask was then stoppered and allowed to stand, over night as a rule, before filtering. The molybdenum sulphide was roasted to oxide.

The filtrate was brought to boiling in a flask while passing carbon-dioxide gas until hydrogen sulphide was wholly expelled, then titrated in the flask at 70°–80° with permanganate. The vanadium was again reduced, this time with sulphur-dioxide gas, which in turn was expelled by boiling in a current of carbon dioxide, and the vanadium was again titrated. If desired the operations of reduction and titration were repeated. The values obtained after successive repetitions of the reduction by sulphur dioxide agreed well but were always somewhat lower than after reduction by hydrogen sulphide. The difference is no doubt due to the presence in the one case of a little free sulphur from the hydrogen sulphide, which consumes permanganate at the high temperature of titration. If the molybdenum sulphide has been filtered through paper instead of a Gooch crucible, permanganate is also consumed by organic extracts from the paper.

Summary.

Two apparently different calcium vanadates are described, which resemble each other very closely and have the same composition—$CaO \cdot 3V_2O_5 \cdot 9H_2O$—when holding their maximum water content at room temperatures. One of them—hewettite—occurs at Minasragra, Peru, and has been noticed on a single specimen from Paradox Valley, Colorado. The other—metahewettite—occurs at numerous localities in western Colorado and eastern Utah. Both minerals are sparingly soluble in water.

A third calcium vanadate—pascoite ($2CaO \cdot 3V_2O_5 \cdot 11?H_2O$)—is also described. This occurs with hewettite at Minasragra. It is very soluble in water.

The first and second minerals are regarded as hydrated acid hexavanadates—$CaH_2V_6O_{17} \cdot 8H_2O$—the third as a normal hexavanadate, $Ca_2V_6O_{17} \cdot 11\,?H_2O$.

The reasons for specific separation of hewettite and metahewettite are set forth in detail. The two minerals are so sensitive to changes in atmospheric humidity that their water content varies within wide limits at different times of the year. The removal of all or nearly all the water does not result in breaking down of the crystal structure, and until this has occurred the water is wholly or in great part taken up again when opportunity is offered.

The importance is emphasized of bringing all minerals that behave in this way to a definite maximum water content before analyzing them and of following carefully the course of dehydration under prescribed conditions. Detailed directions are given for such tests and for avoiding several sources of error.

Attention is also called to two fairly constant associates of metahewettite. One of these (also a constituent of carnotite ores) is a gray hydrous silicate of aluminum, trivalent vanadium, and potassium. The other is elemental selenium, the existence of which as a mineral species seems now for the first time established.

Acknowledgments.

Our thanks are due primarily to Mr. D. Foster Hewett for the material from Peru and to Messrs. T. V. F. Curran, R. H. McMillen and A. G. McNaughton, also to Mr. Frank L. Hess and Dr. W. T. Schaller of the U. S. Geological Survey, for North American material and for valuable information. Dr. Wm. Blum and Mr. E. C. McKelvy, of the Bureau of Standards, were of material assistance in arranging the thermostat and electric furnace used for the water determinations.

Bureau of Standards and Geophysical
Laboratory of the Carnegie Institution,
Washington, D. C., April, 1914.

THE INFLUENCE OF ATMOSPHERIC PRESSURE UPON THE FORCED THERMAL CONVECTION FROM SMALL ELECTRICALLY HEATED PLATINUM WIRES.

By A. E. KENNELLY and H. S. SANBORN.

(*Read April 24, 1914.*)

Object of Enquiry.

This paper describes the process and results of a research made at Harvard University in 1911, to determine the effect of change in atmospheric pressure on forced thermal convection from thin platinum wires. By forced thermal convection is meant the carrying away of heat from the surface of a wire by wind-motion, *i. e.*, by a rapid transverse movement of the wire through the surrounding air. This wind motion through the air dissipates the heat from the wire convectively. The rate of thermal convection depends upon the length and diameter of the wire, its surface condition, the temperature elevation of the wire above the air, the velocity of the motion, and the pressure of the air. The object of the enquiry was to determine the effect of the last-named variable—variation of atmospheric pressure—upon the thermal dissipation, the other quantities being kept constant.

History of the Enquiry.

The research here described was the outcome of an earlier investigation on "The Convection of Heat from Small Copper Wires," by Messrs. A. E. Kennelly, C. A. Wright and J. S. Van Bylevelt, presented at the Frontenac Convention of the American Institute of Electrical Engineers, June 28, 1909, and published at p. 363, Vol. XXVIII., part I., of the *Transactions* for that year. In that research, the forced convection of heat from a thin copper wire, electrically heated to a constant temperature, *i. e.*, maintained at a constant

electric resistance, was discovered to vary as the square root of the wind velocity, which was measured by the speed of the moving wire through otherwise tranquil air. In other words, it was discovered experimentally that in order to dissipate double the power from the wire, at constant resistance and temperature-elevation, it was necessary to quadruple the speed of the wire through the air. This relation was found to hold, within observation errors, for several different sizes of thin copper wire, and for various temperature elevations, between wind-velocities of 2 and 20 meters per second. Below 2 meters per second, the relation deviated towards the case of free convection from a hot wire at rest. That is, at low wind velocities, empirical corrections became necessary for the free convection which naturally occurs from a wire at rest, or moving at zero speed through the air. The possible application of the square-root law of wind cooling to anemometry was also pointed out.

After the results were published in 1909, our attention was drawn to papers by Professor Boussinesq in the *Comptes Rendus* for 1901, Vol. 133, p. 257, and the *Journal de Mathématiques,* 6th Series, Vol. 1, 1905, in which is given the theory of the convection of heat by a stream of liquid from the surface of a cylindrical rod, placed at right angles to the stream. The liquid is assumed to be incompressible and devoid of viscosity. The formula arrived at by Boussinesq, as given by Russell, is:

$$H = 8\theta\sqrt{\frac{s\sigma k V a}{\pi}},$$

where H is the heat carried off convectively per second from unit length of cylinder.

s is the specific heat of the liquid.
σ is the density of the liquid.
k the thermal conductivity of the liquid.
V is the velocity of the liquid.
a the radius of the cylinder.
θ the temperature elevation of the cylinder.

This means that the linear forced convection, or ergs per second per cm. of the cylinder, is proportional to its temperature-elevation above

the liquid, and to the square root of the specific heat, the thermal conductivity, the wind velocity, the fluid density and the wire radius.

Dr. Alexander Russell communicated an important paper on the theory of the subject to the Physical Society of London in July 1910,[1] developing and extending Boussinesq's formula.

Professor J. T. Morris has recently successfully applied the square-root law of forced-convection velocity to the measurement of wind-velocities, using an ingenious form of Wheatstone bridge for this purpose. His observations were communicated to Section G of the British Association in 1912[2] and also to "Engineering"[3] in 1913. His results have confirmed the application of the law for wires of various metals up to diameters of 0.3 mm.

The papers and deductions of Boussinesq were not known to us at the time we presented our former paper in 1909; but since the square-root law of velocity arrived at theoretically by Boussinesq in 1901–1905, for an incompressible non-viscous liquid, has been found to hold within errors of observation for ordinary air, it became desirable to ascertain whether the linear forced convection of air varied as the square root of the air pressure, as suggested by Boussinesq's formula.

Method of Measurement Employed.

The method followed and the apparatus used were respectively the same as those described in the A. I. E. E. paper of 1909, above referred to. A short length of the thin wire to be tested was held in a fork, and was driven by an electric motor at successively varied speeds in a large steel tank, the atmospheric pressure within which was kept constant in each series of tests; but was different in different series.

Test Wire.

The wire used in all of the tests here described was of good commercial platinum, No. 36 B. & S. gauge, with a mean diameter of

[1] *Proc. Physical Society,* 1910, Vol. XXII., also *Phil. Mag.,* October, 1910.

[2] Prof. J. T. Morris, "The Electrical Measurement of Wind Velocity," *The Electrician,* Oct. 4, 1912, pp. 1056–1059.

[3] J. T. Morris, "Distribution of Wind Velocity about a Circular Rod," *Engineering,* Vol. 96, pp. 178–181, Aug. 8, 1913.

0.114 mm. (0.0045 inch). In the tests of 1909, copper wires were used. The advantage of copper is that its resistivity temperature-coefficient is relatively large, and is fairly reliable. On the other hand, hot copper wires oxidize superficially when driven through the air, and are therefore subject to variation in convective dissipation, owing to this change of surface condition. As the test-wire in the new measurements had to be driven inside a steel tank, with only occasional inspections, it was decided to employ platinum, instead of copper; although the resistivity temperature-coefficient of the platinum was but little more than half that of copper; so that the resistance of such a platinum wire is not so sensitive to changes of temperature as a copper wire. Consequently, greater care was needed in the electrical measurements of resistance in the platinum test-wire, in order to determine the temperature elevation.

A measurement of the temperature-coefficient of resistivity of the platinum wire used was made by immersing 5.5 meters of it on a reel in an oil-bath, and measuring the resistance at twelve different temperatures between 0° C. and 100° C. As shown in Fig. 6, the results obtained lie close to the straight line:

$$\rho_t = \rho_0(1 + 0.002575t) \qquad \text{absohm-cm} \quad (1)$$

where ρ_t is the resistivity at $t°$ C. (absohm-cm.), and ρ_0 is the resistivity at 0° C. (absohm-cm.).

The particulars concerning the test wire are given in the accompanying Table:

TABLE I.

TEST-WIRE DIMENSIONS AND DATA.

Mean Diameter.		Cross-Sectional Area, sq. cm.	Linear Surface, sq. cm./cm.	Linear Mass, gm./cm.	Linear Res. at 0° C., absohms/cm.	Resistivity at 0° C., absohm-cm.	Temp. Coeff. of Resistivity 0° C.
mm.	Inch.						
0.114	0.0045	1.02×10^{-4}	0.0358	2.415×10^{-3}	1.28×10^{8}	1.306×10^{4}	0.002575

TEST-WIRE HOLDER.

The test-wire was held in a fork or frame, mounted on the shaft of the driving motor. The fork is indicated in Fig. 1. It is counterpoised by the sliding weight *f*. The test-wire is shown at *b*, held

straight and fairly tight, by the elasticity of the brass strip prongs *aa'*, aided by the tension-screws gg^1. Current is steadily supplied to the test-wire through slip-rings *cc'*, on which rest stationary copper gauze brushes of square cross-section 0.64 cm. (¼ inch) on each

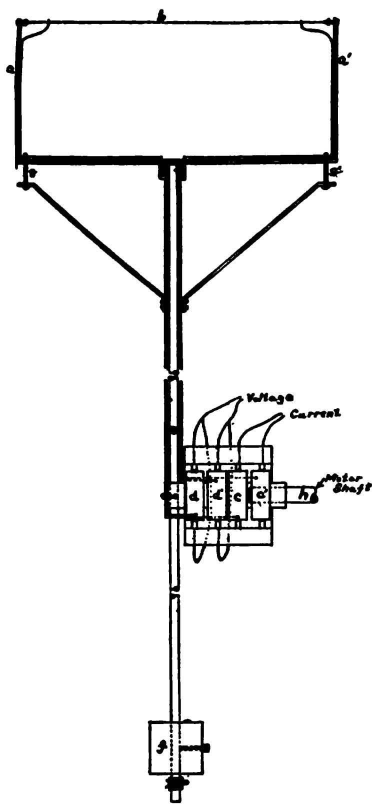


FIG. 1. Details of Rotatable Fork Supporting the Test Wire.

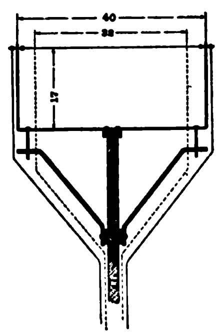


FIG. 2. Details of Fork. Dimensions in Centimeters.

edge. The details of the fork dimensions are shown in Fig. 2. At two points along the test-wire 32 cm. apart, pressure wires are soldered to the test-wire. These pressure wires are of platinum, of the same size as the test-wire. They connect with insulated copper wires fastened to the sides of the fork, and terminate in the slip-rings *dd'* carried by the motor shaft, on which rest two pairs of

stationary gauze brushes. The electrical connections are as shown in Fig. 3. The source of e.m.f. was a storage battery. The regulating resistance *RR* was so adjusted that the ratio of the p.d. between pressure-wires, to the current strength, was equal to a predetermined resistance. That is, the current in the test-wire was gradually increased until the ratio of the reading of the voltmeter *V* to that of the ammeter *A*, was found, by slide-rule, to give the correct resistance sought to be maintained in the test-wire at all wind speeds.

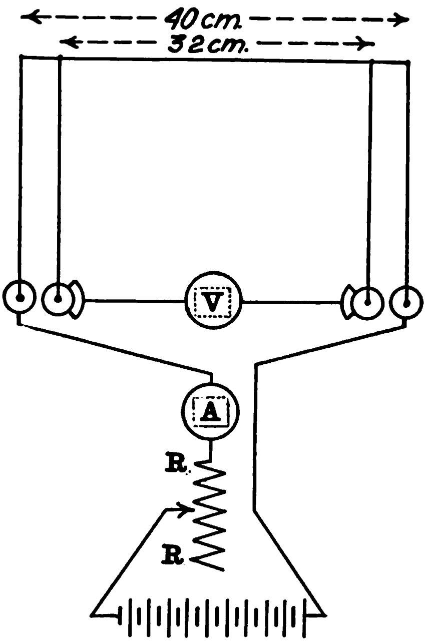


FIG. 3. Diagram of Electrical Connections.

As the driving speed increased, the current supplied to the wire had to be increased, in order to maintain this ratio V/A. When, on the contrary, the driving motor was brought to rest, the current in the test-wire had to be reduced to a relatively small value, in order to reproduce the ratio.

The fork was mounted on the shaft of a ½-HP. 115-volt direct-current shunt motor, arranged to run at adjustable speeds. The

wind speed of the test wire in cm. per sec. was taken as $2\pi \times$ cm. fork radius $\times$ speed of motor in r.p.s. At the other end of the motor-shaft was coupled a small magneto-generator for indicating, by its e.m.f., the speed of rotation. The fork-motor-magneto mechanism is illustrated in Fig. 4, supported on a wooden frame intended to be held in place inside the pressure tank, which is shown with the manhole open.

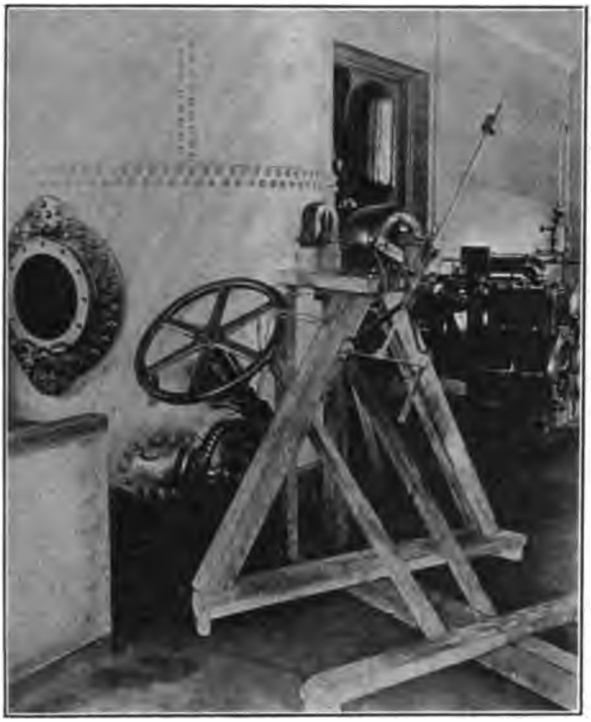


Fig. 4. Photograph of Fork, Driving-Motor, Magneto Speed-Indicator and Pressure Tank.

Pressure Tank.

The pressure tank in which the motor and fork were supported was a vertical steel cylinder of ½″ (1.25 cm.) steel plates, riveted. Figure 5 shows the dimensions of the tank, and also the position within it occupied by the motor and fork. The radius of the fork

to the test-wire; *i. e.*, the distance of the test-wire from the motor-shaft axis was 58.5 cm. (23″); and the radius of the pressure tank was 76 cm. (30″), leaving a clearance between the rotating wire and the tank wall of 17.5 cm. (7″). A larger pressure tank, allowing

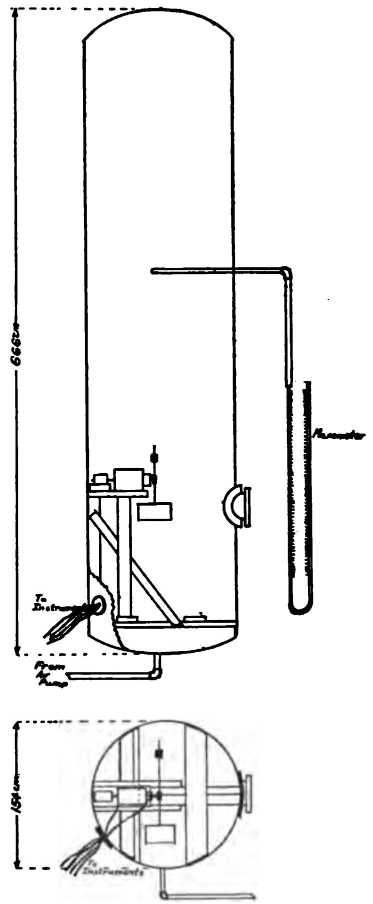


FIG. 5. Elevation and Plan of Pressure Tank showing position occupied by the Test-Wire.

more space and clearance for the revolving test-wire, would have been preferable; but the arrangement was the best that could be made with the apparatus at hand. The results obtained at any

single air-pressure were not so good as those obtained at normal atmospheric pressure in the open air outside the tank, with a larger fork radius, and free air-space. That is, the curves of linear convection against wind-velocity, on logarithm paper, showed more tendency to deviate from a straight line, in these tank tests than in open-room tests, both at low speeds and at high speeds. These deviations might perhaps be explained by air-churnings in the tank, due to the motion of the fork and wire in a somewhat confined space.

The insulated wires leading to motor, magneto, and test-wire, were brought out through holes in a wooden plug bolted air-tight over a manhole.

The speed of rotation of the motor inside the tank was measured in two independent ways; namely (1) by the e.m.f. of the little magneto-generator coupled to the motor, (2) by a contact made

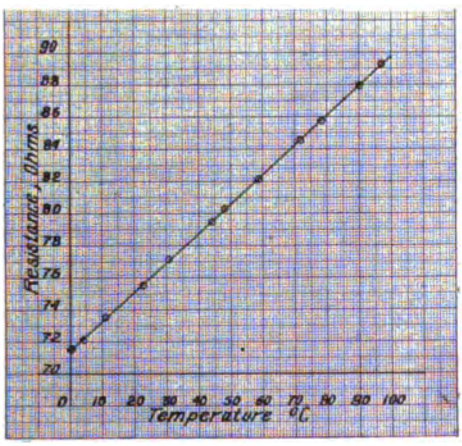


FIG. 6. Resistance in oil of 550 cm. of the Platinum Test Wire at different temperatures between 0° C. and 97° C. in order to determine the resistivity temperature-coefficient.

through a wire on the motor shaft once in each revolution, which gave a click in a telephone. The speed calibration of the magneto and its voltmeter could thus be checked, from time to time, by counting the telephonic clicks in one minute.

The pressure of the air in the tank was controlled by pumps connected with the tank. The tank was fairly tight and ordinarily held its pressure steadily during a test. A large glass U-tube containing mercury was connected with the tank. The difference of level between the mercury in the two arms of the U, corrected for temperature, gave the difference of pressure between the air inside and outside the tank. The absolute pressure of the air in the tank was thus the sum of the U-tube pressure and the corrected barometer pressure outside. This absolute pressure was expressed in "bars" or C.G.S. units (dynes per sq. cm.), by allowing 75.009 cm. of mercury to 1 megabar or 10^6 bars.[4]

Hot-Wire Temperatures.

Two hot resistances were selected for the 32 cm. length of test-wire in different series of tests; namely one at 8.44 ohms, and the other at 10.0 ohms, corresponding to temperatures of 410° C. and 558° C. respectively, by extrapolation from the calibration test between 0° C. and 100° C. indicated in Fig. 6. These temperatures are therefore inferred by resistance. If the temperatures of the wire actually differed from the above inferred values, the values of linear convection here deduced would be correspondingly changed; but the comparative results would be unchanged. So far as the main subjects of enquiry are concerned, it is sufficient that the wire returns to one and the same definite temperature when heated electrically to one and the same resistance. With the air-temperature in the tank in the neighborhood of 20° C., the inferred temperature-elevation of the test-wire by resistance was 390° C. and 538° C. About ten series of speed-measurements were made at each of these elevations, with different air-pressures.

The following table gives one series of tests as an example.

[4] "Les Récents Progrès du Système Métrique," Paris, Gauthier-Villars, 1907, pp. 30–31.

TABLE II.

SERIES OF MEASUREMENTS ON FEBRUARY 16, 1911. Observers A. E. K. and H. S. S. Pressure in tank 75.2 cm. Hg above that in room. Barometer 778 mm. at 14.5° C. Temp. air in tank 18.5° C. Mean absolute pressure in tank 2.04 megabars. Res. of test platinum wire between pressure wires kept at 8.44 ohms. Inferred temp. elevation 391.5° C. R. P. M. of driving motor = 1.14 × magneto voltage.

I. Magneto Volts.	II. Wind Velocity v cm./sec.	III. P. D. on 32 cm. E abvolts.	IV. Current in Wire I abs. amp.	V. Linear Dissipation $P_c = E.I/32$ abwatts/cm.	VI. Linear Dissipation per ° C. P_c/θ abwatts / cm. deg. C.	VII. $\frac{P_c}{\theta\sqrt{v}}$.	VIII. $\frac{P_c}{\theta\sqrt{v+30}}$.
		$\times 10^8$		$\times 10^7$	$\times 10^4$		
101	705.5	23.5	0.279	2.051	5.24	1973	1930
126	880.5	24.9	0.295	2.294	5.86	1975	1942
149	1041	26.0	0.308	2.501	6.390	1981	1952
174	1216	26.95	0.320	2.695	6.880	1975	1948
203	1418	27.8	0.330	2.873	7.340	1949	1928
225	1572	28.55	0.339	3.030	7.740	1954	1929
250	1747	29.3	0.348	3.194	8.160	1954	1935
274	1914	30.0	0.356	3.343	8.539	1952	1935
303	2117	30.6	0.363	3.476	8.879	1930	1915
310	2166	30.9	0.367	3.553	9.075	1950	1938
100	699	23.35	0.277	2.023	5.167	1954	1912
74	517	21.75	0.258	1.754	4.480	1970	1916
43	300.4	19.55	0.232	1.420	3.628	2092	1995
0	0	10.5	0.125	0.412	1.050	∞	1917

Column I gives the voltage generated by the magneto on the driving-motor shaft. From these readings, the speed of the test-wire through the air in the tank; or the "wind-velocity" v of Column II is directly derived in cm. per sec. Column III gives, at each steady speed, the P.D. between pressure wires in C.G.S. magnetic absolute units or "abvolts." Column IV gives the corresponding current strength in C.G.S. magnetic absolute units or "absamperes." The ratio E/I is approximately constant at 8.44×10^9 absohms, or 8.44 ohms. Column V gives the linear dissipation of heat from the wire, or the abwatts (ergs per second) per cm. of wire length. Column VI gives this linear dissipation per deg. Cent. of inferred temperature elevation. It will be seen that this varies from 1.05×10^4 abwatts per cm. and ° C. at standstill, up to 9.075×10^4 at 2,166 cm. per sec. Column VII gives the entries of VI divided by $\sqrt{v}$ giving a value nearly uniform about 1,960, until the velocity v falls to 300 cm. per sec. At standstill, of course, owing

to free convection, the value becomes infinite. If, however, we add 30 cm. per sec. to all the wind velocities v, to correct empirically for free convection as described in the paper of 1909, we obtain the values given in the last column VIII, which do not differ greatly from 1,930 abwatts per cm. ° C. and $\sqrt{v}$, at all speeds in the table.

It will be observed that no correction is made for loss of heat by radiation from the test-wire. That is, the linear dissipations in column VI are treated as though entirely due to convection. In our paper of 1909, a correction was attempted for radiation, on the basis

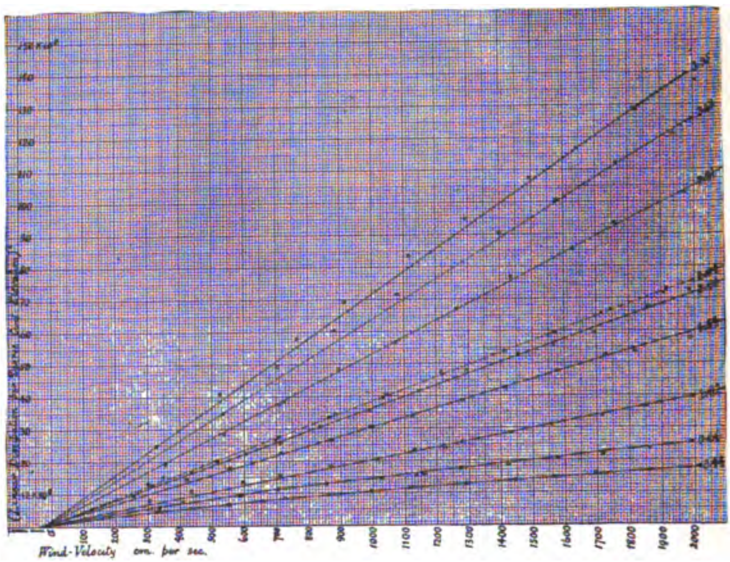


FIG. 7. Curves of $\left(\frac{P}{\theta}\right)^2$ against v, for $\theta = 390°$.

of Stefan's formula. Since, however, it has been pointed out by Dr. Langmuir[5] that the radiation from platinum according to Hagen and Ruben's formula is only a small fraction of that from a "black body," or perfectly non-reflecting radiator, at the same temperature, the radiation corrections in the case of Table II are nearly all less than 1 per cent. of the dissipation, and it has therefore been omitted throughout.

[5] "The Convection and Conduction of Heat in Gases," by Irving Langmuir, *Proc. Am. Inst. El. Engrs.*, June 25, 1912.

The relation found in our 1900 paper was that

$$P_c = k\theta\sqrt{v} \qquad \text{abwatts per cm.} \quad (2)$$

where P_c is the linear convection from the hot wire in abwatts per cm., θ the temperature elevation of the wire, in degrees Centigrade, v the wind-velocity or speed of transverse motion of the wire through the air in cm. per second, and k a constant depending, among other things, on the size and surface-condition of the wire. This formula was found to hold well between the wind velocities $v=200$ and $v=2000$ cm./sec. (7.2 and 72 km/hr. or 4.47 and 44.7 statute miles/hr.); but not to hold below $v=200$, unless 30 cm. per sec. were added as an empirical correction to all speeds to take free convection into approximate account. This empirical correction, applying fairly well, gave:

$$P_c = k\theta\sqrt{v+v_0} \qquad \text{abwatts per cm.} \quad (2a)$$

where v_0 is a virtual velocity of free convection approximating 30 cm. per second.

The relation indicated in (2) can be presented graphically by straight lines on logarithm-paper, but Professor Morris has employed the corresponding relation:

$$\left(\frac{P_c}{\theta}\right)^2 = k^2 v \qquad \left(\frac{\text{abwatts per cm.}}{\text{deg. C.}}\right)^2 \quad (3)$$

That is he plots the square of the observed linear convection per degree C., against the wind velocity, thus producing a straight line, if either (2) or (2a) applies. The procedure is followed in Figs. 7 and 8. Thus, taking Fig. 7, the broken straight line marked 2.04 corresponds to the results in an air-pressure of 2.04 megabars, and the observations in Table I appear on or near this line as small circles. Nine different series are indicated in Fig. 7 at pressures of 0.44, 0.66, 1.02, 1.54, 2.00, 2.04, 2.81, 3.48 and 3.95 megabars respectively, the first two corresponding to vacua, 1.02 to normal atmospheric pressure, and the six others to extra pressure in the tank.

It will be seen that the two lowest curves—vacua—deviate distinctly from straight lines. The remainder are drawn as straight lines, and the observations conform to them fairly well, except at the

two highest pressures 3.48 and 3.95 megabars. This means that equations (2) and (3) held satisfactorily from 1 to 2.8 megabars, but did not hold so well outside those limits of pressure.

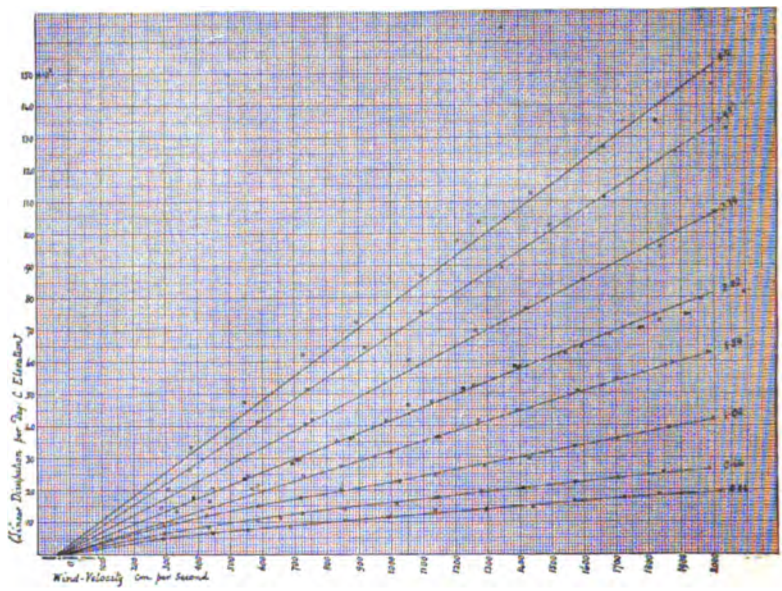


FIG. 8. Curves of $\left(\frac{P}{\theta}\right)^2$ against v, for $\theta = 538°$.

In order to eliminate, as far as possible, any disturbing influence on the forced convection in these tests due to the presence of water-vapor in the air contained in the tank, calcium chloride was kept in the tank. The measurements were all made between January 23 and February 17, 1911, at a time of the year when the air in Cambridge is ordinarily relatively free from moisture. In order to find whether moisture in the air had any considerable effect on the forced convection of heat from the test-wire, one test was repeated at one air-pressure (2 megabars) at each of the two temperature-elevations, first with the air after it had been exposed to the calcium chloride, and second with the calcium chloride removed and a dish of water set over night in its place. The actual difference in the humidity of the air in the tank was not measured, but it was supposed that there

would be a marked difference. It will be seen in Fig. 7 that the small circles on the 2.04 megabar line, representing the dried air test, fairly coincide with the small crosses representing the air test in the presence of water. The same is true for the 2.02 megabar line of Fig. 8. Consequently, the effect of moisture in the heat convection of moving air has not yet been determined from our tests, although it would seem reasonable that in view of the very appreciable known thermal capactiy of aqueous vapor, the effect of moisture might have been apparent.

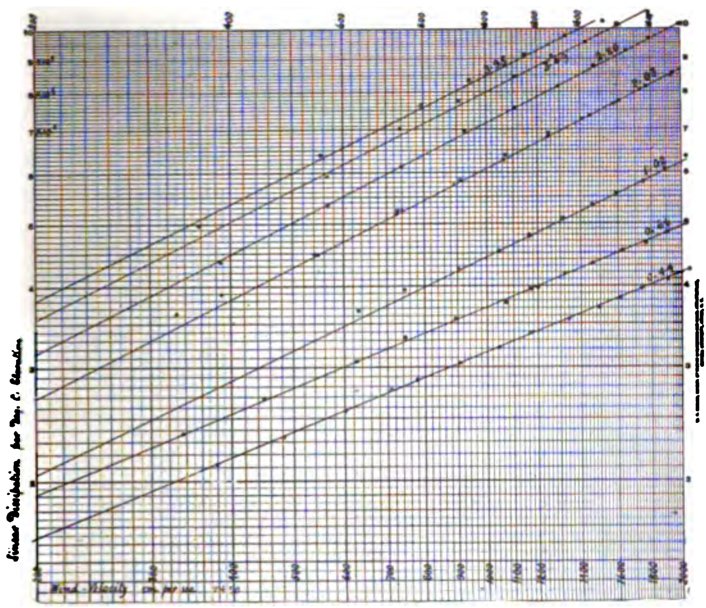


FIG. 9. Graphs of $\left(\frac{P}{\theta}\right)$ against v at different pressures for $\theta = 390°$ C. logarithm paper.

It will be seen that the observations in Figs. 7 and 8 indicate a relation:

$$\left(\frac{P_c}{\theta}\right)^2 = k^2(v + v_0) \qquad \left(\frac{\text{abwatts per cm.}}{\text{deg. C.}}\right)^2 \quad (4)$$

where v_0 is a velocity in the neighborhood of 30 cm. per second, which may be assumed as the empirical correction due to free convection

from the test-wire when held stationary in the air. Professor Morris's method of graphic representation has the advantage that it indicates directly the magnitude of the empirical correction v_0. If we take $v_0 = 30$, we have from (4)

$$\frac{P_c}{\theta} = k\sqrt{v + 30} \qquad \frac{\text{abwatts per cm.}}{{}^\circ\text{C.}} \qquad (5)$$

Figs. 9 and 10 give the graphs of P_c/θ on logarithm-paper, for the various sets of observations. It will be seen the observations lie not far from straight lines. These lines have a gradient of 1 : 2, or correspond to a square-root law, or exponent of 1/2 as in (5); except for the vacua (0.44 and 0.66 megabar), where the gradient is approximately 4 : 10; or would more nearly indicate a relation

$$\frac{P_c}{\theta} = k(v + 30)^{0.4} \qquad \frac{\text{abwatts per cm.}}{{}^\circ\text{C.}} \qquad (6)$$

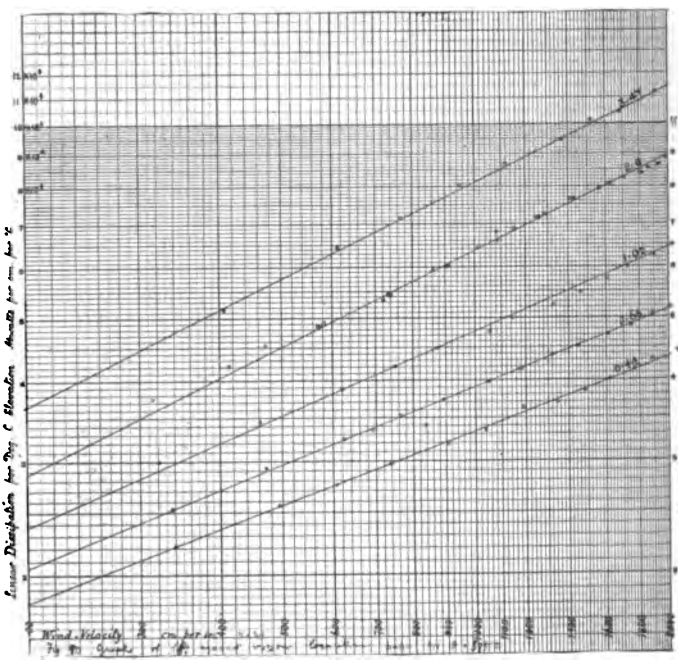


FIG. 10. Graphs of $\left(\frac{P}{\theta}\right)$ against $v + 30$ on logarithm paper for $\theta = 538°$ C.

Considering the results indicated in Figs. 7 and 8, it appears that the slopes of the various straight lines are nearly proportional to the atmospheric pressure. This means that, at least to a first approximation:

$$\left(\frac{P_c}{\theta}\right)^2 = pk'(v + v_0) \qquad \left(\frac{\text{abwatts per cm.}}{\text{deg. C.}}\right)^2 \qquad (7)$$

where $pk' = k^2$

Consequently, when in Professor Morris's diagram, the atmospheric pressure p changes, the ordinates are increased in like proportion. We have then

$$P_c = \theta\sqrt{pk'(v + v_0)} \qquad \frac{\text{abwatts per cm.}}{\text{deg. C.}} \qquad (8)$$

so that the linear convection is nearly proportional not only to the square root of the velocity, but also to the square root of the atmospheric pressure. This agrees with Boussinesq's formula (0) when it is remembered that the air-density σ of the medium is proportional to the pressure p. The remaining constant k' involves, among other things, the diameter of the wire. According to Boussinesq's formula (0), the constant k' should be proportional to the square root of the wire diameter. A special investigation should be directed to this question: but the measurements recorded in our earlier paper of 1909 seem to indicate a higher ratio than the square root.

TABLE III.

VALUES OF k' IN THE EXPRESSION $P_c/\theta = \sqrt{k'p(v + v_0)}$ derived from the series of observations at different atmospheric pressures p, at the velocity $(v + v_0) = 1{,}000$ cm. per sec.

$\theta = 390°$.		$\theta = 538°$.	
p Bars.	k'	p Bars.	k'
3.95×10^6	1.87	4.0×10^6	1.98
3.48 "	1.87	3.47 "	1.96
2.81 "	1.85	2.79 "	1.90
2.04 "	1.83	2.02 "	2.00
2.00 "	1.76		
1.54 "	1.915	1.54 "	2.01
1.02 "	1.96	1.02 "	2.16
0.66 "	2.15	0.66 "	2.39
0.44 "	2.14	0.44 "	2.50

Table III shows the value of k' in formulas (7) and (8) for the various series of measurements appearing in this report at the velocity $v = 1{,}000$ cm. per second. It will be seen that k' varies between 1.76 and 2.50, with a mean value near 2.0.

Anemometer Measurements.

A wind-velocity measuring apparatus, or electric anemometer, was constructed of the same thin platinum wire as that used in the preceding tests (0.114 mm. diameter). A length of 25 cm. of this

Fig. 11. Experimental Anemometer.

wire (10 in.) was supported vertically, between insulated clips, in a steel G frame shown in Fig. 11. Pressure taps, of the same size platinum wire, were soldered on to the vertical test-wire, at a distance

of 15 cm. (5.9 in.) apart. The vertical test-wire was then placed at the spot where the wind was to be measured, and heated by electric current. It thus served to measure horizontal wind-velocity in any direction.

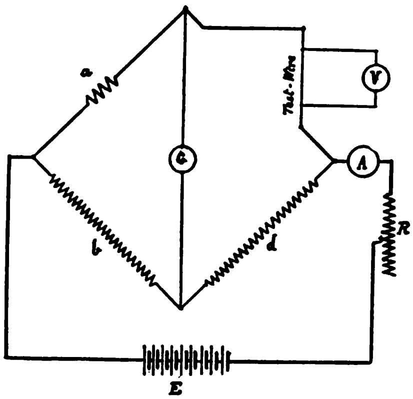


FIG. 12. Connections of Test-Wire for Indirect Measurements of Wind Velocity.

Two methods were used, one indirect-reading, the other direct-reading. In the indirect method, the connections were as shown in Fig. 12. Here the test-wire, shunted by a voltmeter, is placed in a Wheatstone bridge of unequal arms, so that the current in the test-wire side is ten times stronger than in the opposite side *bd*. The bridge is set for balance at a predetermined resistance and temperature of the test-wire. Whatever may be the horizontal velocity of the wind blowing over the test-wire, there is some current strength supplied to the bridge through a meter *A*, which will restore balance and zero current in the galvanometer *G*. When this balance is obtained, the readings of the ammeter *A*, and voltmeter *V*, are noted. Their product *VA*; or the voltage square V^2, is proportional to the power dissipated in the 15 cm. of test-wire, from which the velocity of the wind can be deduced with the aid of suitably prepared tables or curves. The advantage of this method is its relatively high precision. Its disadvantages are that it requires to be adjusted for each observation, and in gusty winds, it is not possible to secure a Wheatstone-bridge balance long enough to obtain readings of either *V* or *A*.

In the direct-reading method, the connections are as shown in Fig. 13. Here the test-wire is connected across 110-volt lighting mains, through an adjusted resistance, which may consist of incandescent lamps, so as to receive as nearly a constant heating current as is practicable. The voltmeter V is connected to the potential taps, 15 cm. apart on the vertical test-wire. The apparatus is then set up at the place where the wind is to be measured. The four leads are

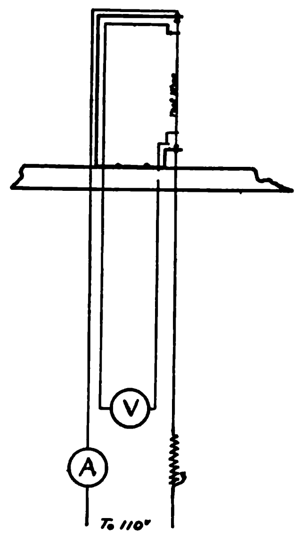


FIG. 13. Connections for Direct-Reading Type of Anemometer.

of any convenient length, and are bound up into a weatherproof cable. From calibration measurements made on a sample of the test-wire in a motor-driven fork, the linear convective dissipation of heat for any safe given linear resistance of the wire is known. As the horizontal component of wind-velocity increases, the temperature of the platinum test-wire falls, since no provision is made in this case to restore the initial temperature. A calibration curve has therefore to be prepared for a given exciting current, whereby the readings of the voltmeter, which may be a recording instrument, become convertible into wind-velocities. A set of calibration curves is given in Fig. 14 to the left-hand scale, both for 1.5 amperes and 2.0 amperes, constantly sup-

plied to the test-wire from the 110-volt circuit. It will be seen that when the wire is greatly cooled by the wind, a very appreciable correction for the temperature of the wind enters into the result; although at a temperature elevation of say 300° C, this correction would be comparatively small. It is necessary to set the current at such a value that when the wind fails, the test-wire shall not be dangerously heated. It was found that with a platinum test-wire of 0.114 mm. diameter, as used in these measurements, 1.5 amperes was a suitable current for wind-velocities up to 15 km. per hour. Thus, as indicated in Fig. 14, with a wind of 10 km. per hour, and a temperature of 10° C., the voltmeter reading was 4.3 volts. When, however, the wind-velocities were higher, the current was increased to 2 amperes, which, in still air, raised the wire temperature to visible redness. At 30 km. per hour, and 10° C. wind temperature, the p.d. on 15 cm. was then 5.8 volts.

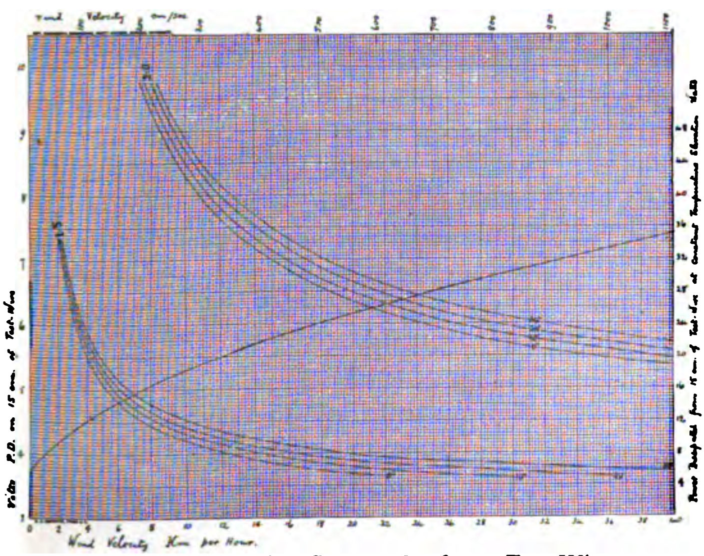


FIG. 14. Calibration Curves of 15° cm. Test Wire.

The *G*-support and test-wire were fastened to a pole and supported out of doors, exposed to the weather for some weeks. The apparatus appeared to be durable, and constant in its indications throughout that time, except that the solderings of the platinum tap-

wires across the test-wire, introduced an element of weakness. After exposure of some days, the test-wire was apt to break at a soldered point. In subsequent out-door trials, therefore, the tap-wires were simply twisted on to the test-wire. These tap-points involve a certain small error due to their cooling effect on the test-wire. It is, therefore, desirable to make the tap-wire as thin as is practicable, in view of both mechanical strength and electric resistance.

In a wind, the readings of the voltmeter anemometer are always fluctuating; but a mean value at any moment can always be estimated. A heavily damped voltmeter may conveniently be used.

The rising parabolic curve in Fig. 14 indicates, to the right-hand scale, the watts convected from 15 cm. of the same test-wire at different wind-velocities up to 1,100 cm./sec. or 40 km./hr. with constant resistance maintained in the wire; *i. e.*, by the method of Fig. 12. It is evident that for high wind-velocities, the indirect method of constant test-wire temperature is more sensitive than the direct-reading method of constant current. For lower velocities, however, the direct-reading method is much the more convenient, but requires to be corrected for the wind-temperature.

In the use of the constant-current direct-reading method, tungsten lamps have an advantage for the regulating resistance of Fig. 13 in that they tend to compensate for changes in the resistance of the test-wire at different wind-velocities; or to act as ballast resistance for the closer maintenance of constant current.

Conclusions.

1. The forced convection of heat from a thin platinum wire at constant temperature moved transversely through the air, varies not only approximately as the square root of the speed, but also approximately as the square root of the air-pressure (or air-density), in accordance with Boussinesq's formula.

2. At air-pressures below half an atmosphere, the above square-root relation was sensibly departed from. At pressures above 3 atmospheres, the relation was also departed from. Although the departures in both cases exceed the limits of observation errors, it is not certain whether they may have been due to imperfections in the

apparatus, such as eddy-currents in the confined air, caused by air-churning in the pressure-tank. In other words, it is uncertain whether, if a more spacious pressure-tank could have been used, with a longer fork radius, the square-root relation might not have held better than it did at the upper and lower air-pressures.

3. The effect of moisture in the air, upon the forced convection of heat from a thin wire, seems to be small, and has not yet been determined.

4. The forced convection of heat from a thin wire at constant temperature in air of variable pressure may be regarded as varying approximately with the square root of the mass of air displaced by the wire per second, or as $\sqrt{v\sigma}$, where v is the wind-velocity of displacement and σ the density of the air.

5. When the air-pressure p remains constant, $(P_c/\theta)^2$ or the square of the linear convection per deg. C. elevation, plotted against wind-velocity v, gives a straight line for a thin metallic wire. When, however, the air-pressure p varies, then it seems that, at least to a first approximation, $(P_c/\theta)^2/p$ plotted against wind-velocity v, gives one and the same straight line, for that wire, for all values of p from 0.5 to 2.0 megabars.

6. A thin vertical platinum wire about 25 cm. long, after being calibrated in a motor-driven fork, can serve as a recording wind measurer or anemometer. The record requires to be corrected both for the temperature and pressure of the air. The degree of precision obtainable is greatest at low wind-velocities.

THE UNITED STATES AS A FACTOR IN WORLD POLITICS.

By L. S. ROWE, Ph.D., LL.D.

(*Read April 24, 1914.*)

There is probably no characteristic of American political life that has made a deeper impression on foreign students of our institutions than the fact that while all matters of domestic policy are subjected to the most searching analysis, questions of foreign policy do not in normal times become the topic of public discussion. It is not until our international relations have reached a critical stage that real public interest is aroused. In most cases, however, before public opinion has become crystallized, the national executive has committed the country to a certain line of action. Thereafter the country's policy is determined by the logic of events rather than by the dictates of public opinion.

In comparing the French, German and British magazines and newspapers with those of the United States, one is impressed with the fact that while the European journals are constantly discussing questions of foreign policy, little or no attention is given to this subject in our American journals until some problem has reached so critical a stage that it imperils the peace and safety of the country. The causes of this contrast are to be found not in the more serious character of foreign journals, but in the fact that the American people have been accustomed to confine their thinking on public affairs to questions of domestic policy.

Under our system of government this attitude involves a real danger because it removes our foreign relations from the control of public opinion, and makes them dependent upon the personal views of the President of the United States, subject to such influences as may, for the time being, be dominant in Washington. When public opinion does assert itself, such assertion usually comes as the result of a wave of popular feeling, obeying an emotional impulse. The

root of the difficulty is that the national thought of the people of the United States has failed to keep pace with the changes in the international position of the country. The early policy of our country, as well as our national thinking during the infancy of the republic, was concentrated on the idea of national isolation—freedom from entangling alliances with European countries, and the separation of American from European interests. National thought has remained in this stage of development whereas national power and national influence have long advanced beyond these narrow confines. We have attained the dignity of a world power, but our national thought has not advanced to a consciousness of the responsibilities which this position involves. The great problem now confronting the country is to bring about closer harmony between these two factors.

We are at the present moment witnessing one of the most serious consequences of this lack of adjustment which is affecting the international position and influence of the United States to a degree which cannot help but arouse the grave concern of every thoughtful and patriotic citizen. In a brief period of fifteen years we seem to have sacrificed the position of leadership in the maintenance of world peace, and have become one of the disturbing factors in international affairs. How is it, it will be asked, that a nation which through the contributions of more than a century has gained an enviable position as a leader in the great movement for the advancement of international goodwill, should within so short a space of time sacrifice this enviable position and come to be looked upon by all nations of western civilization as an uncertain factor in the orderly development of international relations.

Every student of international law and of world politics has been deeply impressed by the important part played by the United States in placing the conduct of international relations on a distinctly higher plane. It seems, at first glance, extraordinary that during the first half century of its existence a nation so weak and in many respects so unorganized should have been able to exert so important an influence on international law. When, however, we stop to reflect that during the first decades of the nineteenth century the United States held the balance of power amongst the nations of western civilization, the apparent paradox is readily explained.

The farseeing statesmanship of the founders of the republic led to the adoption, as a cardinal principle of American foreign policy, that the United States must be kept free not merely from entangling European alliances, but from any participation in the conflicts then raging in Europe. This principle of aloofness from European entanglements led to the assertion of those principles of American neutrality which, while serving primarily the interests of our national integrity, accomplished the still larger purpose of laying the foundations for the modern law of neutrality. In performing this service the United States contributed toward eliminating some of the most fruitful causes of international irritation, thereby promoting the interests of world peace.

It has been the laudable ambition of successive secretaries of state to continue and to strengthen those traditions which gave to the country a position of such unique power amongst the nations of both eastern and western civilization. In spite of these efforts, however, there is noticeable during recent years a distinct falling off in our international prestige. Little by little, the confidence of the peoples of Europe and of the American Continent has been undermined until to-day we find ourselves in a situation which possesses none of the elements of that splendid isolation which so long characterized the position of Great Britain and which, if not remedied, is likely to deprive us of the possibility of carrying to a successful conclusion a mission which constitutes the chief glory of American foreign policy during the first century of our national existence. It is, therefore, a matter of real national moment to inquire into the causes which have brought about this change, and to seek a remedy if such exists.

Of the elements contributing to the present situation, some are of long standing, the cumulative effects of which are now being felt, while others are of comparatively recent development. Amidst the splendid record of achievement during the first century of our national existence there looms up one aspect of our policy which has been a cause of deep concern to successive presidents and to successive secretaries of state. I refer to the inadequacy of our national legislation for the protection of resident aliens. A long

series of massacres, beginning with the Chinese massacre at Rock Springs, Wyoming, in 1895 and ending with the lynching of Italians in 1899, 1901 and 1910, have placed our national government in the humiliating position of acknowledging to foreign powers that although the sole responsibility for the conduct of our foreign relations rests with the federal authorities, they lack the power to fulfill the most fundamental of international obligations—the duty to bring to justice the persons responsible for such crimes. The matter was referred to as early as 1899 by President McKinley, who, in his annual message of December 5th, said:

"For the fourth time in the present decade a question has arisen with the government of Italy in regard to the lynching of Italian subjects. The latest of these deplorable events occurred at Tallulah, Louisiana, whereby five unfortunates of Italian origin were taken from jail and hanged. . . . The recurrence of these distressing manifestations of blind mob fury directed at dependents or natives of a foreign country suggests that the contingency has arisen for action by Congress in the direction of conferring upon the Federal courts jurisdiction in this class of international cases where the ultimate responsibility of the Federal Government may be involved."

The matter was again vigorously taken up by President Roosevelt in his message of December, 1906, in which he said in referring to the difficulties that had arisen because of educational discrimination against the Japanese in California:

"One of the great embarrassments attending the performance of our international obligations is the fact that the statutes of the United States are entirely inadequate. They fail to give to the national government sufficiently ample power, through United States courts, and by the use of the army and navy, to protect aliens in the rights secured to them under solemn treaties which are the law of the land. I, therefore, earnestly recommend, that the criminal and civil statutes of the United States be so amended and added to as to enable the president, acting for the United States government, which is responsible in our international relations, to enforce the rights of aliens under treaties. There should be no particle of doubt as to the power of the national government completely to perform and enforce its own obligations to other nations. The mob of a single city may at any time perform acts of lawless violence against some class of foreigners which would plunge us into a war. That city by itself would be powerless to make defense against the foreign power thus assaulted, and if independent of this government it would never venture to perform or permit the performance of the acts complained of. The entire power and the whole duty to protect the offending city or the offending community lie in the hands of the United States government. It is unthinkable that we should continue a policy under which a given locality may be

allowed to commit a crime against a friendly nation, and the United States government limited, not to preventing the commission of the crime, but, in the last resort, to defending the people who have committed it against the consequences of their own wrong doing."

In 1909, in his inaugural address, President Taft emphasized this serious defect in the conduct of our foreign relations, in the following words:

"By proper legislation we may, and ought to place in the hands of the federal executive the means of enforcing the treaty rights of such aliens in the courts of the federal government. It puts our government in a pusillanimous position to make definite engagements to protect aliens, and then to excuse the failure to perform those engagements by an explanation that the duty to keep them is in states or cities, not within our control. If we would promise we must put ourselves in a position to perform our promise. We cannot permit the possible failure of justice, due to local prejudice in any state or municipal government, to expose us to the risk of a war, which might be avoided if federal jurisdiction was asserted by suitable legislation by Congress and carried out by proper proceedings instituted by the executive in the courts of the national government."

It is clear that no nation can shirk the responsibilities of its international obligations without arousing widespread opposition. The constitutional authority granted to our federal government is sufficiently comprehensive to include all powers necessary to meet our international obligations. We cannot permit our states, which occupy no international status, to plunge us into irritating controversies with foreign countries. The dignity of the national government and the demands of national self-respect require that the federal executive be given statutory powers sufficiently broad and that the federal judiciary be given jurisdictional authority sufficiently comprehensive to enable the national government to do its full duty in the protection of the person and property of aliens resident within our borders. The first step in this direction is the enactment of a law giving to the federal courts jurisdiction over all cases in which the treaty rights of a citizen or subject of a foreign country are involved. A bill to this effect has been before the Congress of the United States on several different occasions. Its precise text is as follows:

"Be it enacted by the Senate and House of Representatives of the United States of America, in Congress assembled, that any act committed in any state or territory of the United States in violation of the rights of a citizen

or subject of a foreign country secured to such citizen or subject by treaty between the United States and such foreign country, which act constitutes a crime under the laws of such state or territory, shall constitute a like crime against the peace and dignity of the United States, punishable in like manner as in the courts of said state or territory, and within the period limited by the laws of such state or territory, and may be prosecuted in the courts of the United States, and upon conviction, the sentence executed in like manner as sentences upon convictions for crimes under the laws of the United States."

That the federal government has ample power to enact such legislation has been repeatedly affirmed by the Supreme Court of the United States. In Baldwin *vs.* Frank (120 U. S. 678) one of the leading cases on the subject, the question involved was whether the Civil Rights Acts were applicable to a conspiracy to deprive Chinese subjects, residing within a state, of rights secured to them by treaty. In the course of its opinion the court said:

"The precise question we have to determine is not whether Congress has the constitutional authority to provide for the punishment of such an offense as that with which Baldwin is charged, but whether it has so done.

"That the treaty-making power has been surrendered by the states and given to the United States is unquestionable. It is true also that the treaties made by the United States and in force are part of the supreme law of the land, and that they are as binding within the territorial limits of the states as they are elsewhere throughout the dominion of the United States.

"That the United States have power under the Constitution to provide for the punishment of those who are guilty of depriving Chinese subjects of any of the rights, privileges, immunities or exemptions guaranteed to them by this Treaty, we do not doubt. What we have to decide, under the questions certified here from the court below, is whether this has been done by the sections of the Revised Statutes specially referred to."

Again, in the Debs case (158 U. S. 564) the Court held:

"The entire strength of the nation may be used to enforce in any part of the land the full and free exercise of all national powers and the security of all rights entrusted by the Constitution to its care. . . . If the emergency arises, the army of the nation, and all its militia, are at the service of the nation to compel obedience to its laws."

All this tends to prove not only that we have been remiss in the performance of our international obligations, but that such remissness has not been due to any defect in our national Constitution but to the failure of Congress to extend the jurisdiction of the federal courts and to grant specific authority to the federal executive to

fulfill manifest international obligations. Indeed, it would seem from the decision in the Debs case that even without specific statutory authority, the national executive may do far more than has hitherto been done in maintaining the supremacy of the federal treaty making power.

The failure of Congress to make adequate provision for the protection of resident aliens has aroused resentment not only in the states whose *nationals* have suffered most severely, but has seriously injured our reputation for fair dealing throughout the civilized world. The remedy for this situation is so simple that there is no excuse for further delay in making it effective.

A second influence which has played an important part in estranging the goodwill of foreign countries is the widespread belief that there exists in the Congress of the United States a tendency to force upon the executive a narrow and technical interpretation of treaties. Secretary Hay once said of certain senators who attempted to defeat every treaty presented to the Senate, that their idea of a treaty was a document which gained everything for the United States and gave nothing to the other party. The ruthless way in which the Congress of the United States has at times swept aside treaty obligations, and the unwillingness to bring national legislative policy into harmony with our international obligations, have created the impression that the promises of the United States cannot be depended upon, and that even the best intentions of the President and his advisers are apt to be thwarted by the action of Congress.

The culminating point of a series of instances was reached in the provision of the Panama Canal Act exempting American coastwise shipping from the payment of canal tolls. Whatever may be our opinion as to the desirability of the exemption clause viewed as a question of domestic policy, it is clear from the history of the Clayton-Bulwer and of the Hay-Pauncefote treaties and from the testimony of those who assisted in their negotiation that the United States made no attempt to reserve to itself the right to give preferential treatment to its own merchant vessels. The privileges acquired by the United States under the Hay-Pauncefote Treaty involved certain concessions on the part of Great Britain, for which she ex-

acted the observance of the principle of equality of treatment. It would be a reflection on our country's reputation for fair dealing if, after securing the abrogation of the Clayton-Bulwer Treaty, we were to repudiate the concessions, the making of which rendered possible the ratification of the Hay-Pauncefote Treaty.

It is fortunate for the world's peace that there is rapidly developing a body of international opinion to which the policy and conduct of individual nations must conform. Violations of the standards set by this opinion place the offending nations under the ban of international disapproval. With each year the commercial and social relations between nations are becoming closer. This increasing interdependence means that national policy must be made subservient to international right and to international obligation. No nation is a law unto itself, and it is evident that even our concept of national sovereignty must be subjected to revision in order to conform more closely to those larger principles of international reciprocity and fair dealing, upon which the maintenance of western civilization so largely depends. Just as competition has gradually given way to coöperation in the industrial world, so in international affairs the concerted action of states and the idea of mutual obligation as between states are gradually taking the place of the more primitive principle that every nation may formulate its national policy on the basis of national interests regardless of the higher standards of conduct now prevailing in the society of states.

Fortunately, for the good name of the United States, the President has courageously taken a position which has not only aroused the admiration of the civilized world but has placed our country under a debt of obligation. In his address of March 5, 1914, to the Congress of the United States he sounded a note which served to impress upon the nation the sacredness of treaty obligations. Speaking of the Hay-Pauncefote Treaty he said:

"We consented to the treaty; its language we accepted, if we did not originate it; and we are too big, too powerful, too self-respecting a nation to interpret with a too-strained or refined reading the words of our own promises just because we have power enough to give us leave to read them as we please. The large thing to do is the only thing we can afford to do, a voluntary withdrawal from a position everywhere questioned and mis-

understood. We ought to reverse our action without raising the question whether we were right or wrong and so once more deserve our reputation for generosity and for the redemption of every obligation without quibble or hesitation."

The magnitude of the President's service goes far beyond the vindication of the Hay-Pauncefote Treaty. These words and the determination which lies back of them place the international relations of the United States on a distinctly higher plane, and, if properly supported by the united opinion of the country, will do much toward regaining for the United States the enviable position which we once occupied. All secondary and party interests must be made to bow before that higher standard of international dealing which the President so vigorously champions.

A third influence which has played an important part in arousing opposition to the United States has been the tendency to permit new doctrines of American foreign policy to masquerade under the cloak of the Monroe Doctrine. In the adjustment of our relations with Mexico, with the islands of the West Indies, and with the countries of Central America, we have fallen into the error of endeavoring to build up our relations on the basis of the negative principles formulated in 1823. Instead of clearly and definitely facing the fact that these sections of the American continent occupy an exceptional relation toward the United States, and building up our policy on the basis of this exceptional relationship, we have formulated vague principles purporting to be based on the Monroe Doctrine which have aroused the suspicion, the distrust, and even the hostility of the most progressive countries of South America. We should clearly and definitely recognize the fact that everything that affects the progress, the stability and the well-being of the islands of the West Indies, of Mexico and of Central America, is a matter of immediate and direct concern to the United States. It is a concern different in kind from that which affects our relations with the countries of South America. The acquisition of Porto Rico and our exceptional relations with Cuba, have made of the United States a West Indian power. The construction of the Panama Canal and the acquisition of the Canal Zone have made of the United States a Central-American power, and finally, the fact that Mexico is our

neighbor, that large bodies of American citizens have taken up residence there, and that vast American interests amounting to over a billion dollars, are invested in Mexican enterprises—all these factors indicate the necessity of developing a policy toward these three sections of the American continent based on the positive national interests involved rather than on the negative principles of the Monroe Doctrine. We should frankly proclaim to the world that basic national interests demand that these sections of the American continent shall not only remain free from European complications, but that the primary requisites for the preservation of civilization shall be maintained. Continued disorder, the disregard of fundamental human rights, the undermining of respect for property—all these constitute elements which vitally affect the well-being and safety of the United States. This does not mean either the extension of a proctectorate or an unwarranted interference with the domestic affairs of these countries, but it does mean that the United States cannot remain indifferent to the existence of conditions which menace the fundamentals of civilization.

The positive principles of foreign policy which this exceptional situation demands, rest in part on the fact that the sections of the American continent above referred to have become important sources of the food supply of the American people, and that the possibility of reducing the cost of living of the American workingman depends in large measure on the uninterrupted use of these sources of supply.

This does not mean that the United States should pursue a selfish or ruthless policy in dealing with these countries. On the contrary, the permanent interests of the United States are best subserved by prosperous, independent, self-respecting and progressive neighbors. Our policy toward them should be conceived in the most elevated spirit of helpful coöperation, but the basis of this coöperation should be and must be the maintenance of the fundamental requisites of civilization.

The Monroe Doctrine was formulated to accomplish two specific purposes:

1st. To prevent further European colonization on the American continent, and

2d. To prevent the extension of the European political system to the United States, the overthrow of the domestic institutions of an American state by European influence, or the control of the political destinies of an American state by any European power.

These two leading principles, which were of an essentially negative character are still vital principles of the foreign policy of the United States, in the maintenance of which every state of the American continent is deeply interested. The cordiality of our relations with the countries of South America demands that the Monroe Doctrine be limited to its original content. If this is done, there need be no fear of wounding the sensibilities or arousing the opposition of the countries of South America.

When, therefore, in our relations with the islands of the West Indies, with Central America or with Mexico, it becomes necessary to go beyond the negative principles of the Monroe Doctrine, and enforce positive principles of foreign policy, let us take such a step fully cognizant of the fact that we are not acting under any supposed principles of the Monroe Doctrine, but on the basis of a policy dictated by the requirements of the special conditions in the Mediterannean section of the American continent. Special agreements looking toward coöperation for the maintenance of stability, such as the reorganization of the San Domingan finances, do not rest on any principle of the Monroe Doctrine, but are dictated by the exceptional relationship above referred to.

Our Mexican policy is another of the influences that has reacted unfavorably upon the international position of the United States. No one will doubt for a moment the lofty ideals which have actuated the President in the formulation of his policy, but it is also clear that in spite of all protestations to the contrary our Mexican policy has aroused a marked feeling of opposition amongst the peoples of Central and South America, and has served to foster secret understandings between European governments for the purpose of protecting what they regard as their national rights and the rights of their citizens. We must always bear in mind that whatever may be our personal views with reference to the Monroe Doctrine it has always been regarded by the European countries as an expression

of the intention of the United States to reserve the countries of the American continent as a special field for its commercial and industrial influence.

The unwillingness of the United States to allow European governments to intervene in Mexico for the protection of the interests of their citizens and subjects, combined with the reluctance of the United States to accept the full responsibilities which this position involves, has served to accentuate the feeling of opposition to the United States which has been growing so rapidly within recent years.

Furthermore, the attempt on the part of the President to deal with the Mexican situation as if it were part of a general Latin-American problem instead of facing it squarely as a problem *sui generis,* involving an exceptional relationship between neighboring countries, has aroused the bitter opposition of the countries of Central and South America. The insistence of the United States on the retirement of a provisional President is looked upon as a form of unwarranted dictation, and as an indication of a settled purpose on the part of the United States to assert a kind of political supervision over the republics of the American continent.

In conclusion, I desire to refer to a recent occurrence which has given rise to serious misgivings both in Europe and in the countries of Central and South America. In an address delivered before the Southern Commercial Congress, the President of the United States announced a new principle of American foreign policy, the purpose of which seems to be the gradual financial emancipation of the countries of Central and South America from their present dependence on European capital. In the course of this address, the President attacked

> "the material interests that had influenced the foreign policy of certain Governments in their relations with the nations of Latin-America."

He declared it to be the duty of the United States,

> "to assist the nations of this hemisphere in their emancipation from the material interests of other nations, so that they might enjoy constitutional liberty unrestrained." "You hear," he said, "of concessions to foreign capital in Latin-America . . . States that are obliged to grant concessions are in the position that foreign interests are apt to dominate their affairs. Such a state of things is apt to become intolerable. It is emancipation from this inevitable subordination that we deem it our duty to assist."

It is true that the President restricted himself to a declaration against "concessions," and it would seem that to his mind this term involves the idea of special privilege or monopoly. The nearest English equivalent of the Spanish word "*concessión*" is our own legal term "franchise." It is true that in many of the countries of Central and South America such franchises include the grant of monopolistic privileges. It is also true that under the cloak of such franchises many abuses have been committed, but we must bear in mind that the unsettled political conditions prevailing in many of these countries and the exceptional risks to which foreign capital is subjected, have made it necessary to offer exceptional inducements in order to attract foreign investors. If we stop to reflect on the extraordinary inducements which were offered to foreign capital during the early history of the United States, and on the great service which such capital rendered to our national development, we can readily see that any policy, the effect of which is to discourage foreign investments in Central and South America, cannot help but retard the development of those sections of the continent. We may deplore the fact that in many of the republics of the American continent there has been a wasteful and at times a corrupt distribution of franchises and special privileges, but it is a serious question whether it is either our duty or our right to undertake to determine or even to suggest the standards or conditions to which the investment of foreign capital should conform.

At all events, let us not close our eyes to the fact that the formulation of this policy has aroused serious misgivings throughout the countries of the American continent, as it is looked upon as an unwarranted assumption of control over their liberty of action. In Europe the President's pronouncement is regarded as confirmatory of a suspicion, which has been growing within recent years, namely that the United States has embarked upon a national policy, the purpose of which is to reserve the less advanced countries of the American continent for the economic exploitation of American capital.

Whatever the ultimate judgment on the appropriateness of the principles or the wisdom of the policy formulated in the President's Mobile speech, it should be made clear that this new orientation of

our foreign policy is not a part of the Monroe Doctrine, and has no organic relation to the fundamental principle upon which the Monroe Doctrine rests, namely, national safety and self-protection. It is a new and strange principle which has aroused the opposition of the countries for whose benefit it is intended, and has engendered bitterness of feeling amongst European peoples. If it is to be maintained it must justify itself by basic reasons of national interests and international obligation entirely independent of the Monroe Doctrine.

These, in brief, are the more important influences that have aroused the opposition of many sections of the American continent and the animosity of Europe, and have placed the United States in a position of international isolation. It would be idle to argue that we have been the victim of circumstances because, as we have seen, the position in which the United States finds herself at the present time is traceable to the fact that our national thought and national consciousness have not kept pace with our international responsibilities. The most serious aspect of this condition of isolation is that it prevents us from fulfilling the high mission in international affairs which, by reason of our exceptional geographical position, by reason of our exceptional relationship to European as well as to American affairs, we are manifestly called upon to perform. The words spoken by Mr. Root at the Fourth Pan-American Conference at Rio Janeiro set the standards which should ever remain before the American people:

> "We wish for no victories but those of peace; for no territory except our own; for no sovereignty except the sovereignty over ourselves. We deem the independence and equal rights of the smallest and weakest member of the family of nations entitled to as much respect as those of the greatest empire, and we deem the observance of that respect the chief guaranty of the weak against the oppression of the strong. We neither claim nor desire any rights, or privileges, or powers that we do not freely concede to every American Republic. We wish to increase our prosperity, to expand our trade, to grow in wealth, in wisdom and in spirit, but our conception of the true way to accomplish this is not to pull down others and profit by their ruin, but to help all friends to a common prosperity and a common growth, that we may all become greater and stronger together."

THE MORPHOLOGY OF THE PASSAMAQUODDY LANGUAGE OF MAINE.

By J. DYNELEY PRINCE.

(*Read April 25, 1914.*)

The Passamaquoddy Indians of Maine, together with the Maliseets (Milicetes) or St. John's River Indians of New Brunswick form a single linguistic group of the eastern Algonquin family known as *Wabanaki,*' people of the dawnland,' or 'East.' It is estimated that there are about three hundred and fifty people in each clan. The other members of the group are the Micmacs of Nova Scotia, the Abenakis (a corruption of Wabanaki) or St. Francis Indians of Quebec, and the Penobscots of Oldtown, Maine, the two latter clans also forming a linguistic group similar to that of the Passamaquoddies and Maliseets.[1] The name "Passamaquoddy" is a corruption of *pestumo'kat,* 'one who catches pollack fish' (*Gadus Pollachius*) = *peska'tum.* This term has been applied to the tribe only in comparatively recent times. The headquarters of the Passamaquoddies are at Pleasant Point, Maine (*Sipáyik*), where the remnants of the tribal organization still exist. Here, for example, dwelt Sopiel Selmo, the keeper of the Wampum Record, a mnemonic system of wam-

[1] For the Wabanaki group, cf. my articles: "Notes on the Language of the Eastern Algonquin Tribes," *Amer. Jour. Philol.,* IX., pp. 310–316; "The Wampum Record," *Proc. Amer. Philos. Soc.,* 1897, pp. 479–495; "Forgotten Indian Place-names in the Adirondacks," *Jour. Amer. Folk-Lore,* 1900, pp. 123–128; "Some Passamaquoddy Witchcraft Tales," *Proc. Amer. Philos. Soc.,* XXXVIII., pp. 181–189; "Notes on Passamaquoddy Literature," *Annals N. Y. Academy of Sciences,* XIII., pp. 381–386; "The Modern Dialect of the Canadian Abenakis," *Miscellanea Linguistica in Onore di Graziodio Ascoli,* 1901, pp. 343–362; Leland and Prince, "Kulóskap the Master," Funk and Wagnalls, New York, 1902; "The Algonquin Noun," Proceedings of the Congress of Orientalists, Rome, 1904; "Algonquin Religion," Hastings, Dictionary of Religions, s. v. "God"; "A Micmac Manuscript," Proceedings of the Congress of Americanists, Quebec, 1908; "A Passamaquoddy Aviator," *Amer. Anthropologist,* XI.

pum shells arranged on strings in such a manner, that certain combinations suggested certain sentences or ideas to the narrator, who, of course, knew his record by heart and was merely aided by the association of the shell combinations in his mind with incidents of the tale, song or ceremony which he was rendering (Prince, *Proc. Amer. Philos. Society,* December 3, 1897, pp. 479–495). With Selmo, however, died the secret of this curious system, but the laws and customs thereby recorded have been preserved and published in the *Proceedings of the American Philosophical Society* (*loc. cit.*). There is also a large amount of oral literature handed down by these Indians, a quantity of which exists in the manuscripts of the Hon. Lewis Mitchell, Indian member of the Maine Legislature. These documents are now in my possession and I expect to publish their material in an exhaustive work on the Passamaquoddy tribe and language. Some matter of this character has already appeared, both in the *Proceedings* of this Society (XXXVIII., pp. 181–189), and also in Leland and Prince, "Kulóskap the Master," New York, 1902, a popular exposition of the eastern Algonquin folk-lore.

The object of the present paper is to discuss briefly the chief peculiarities of the Passamaquoddy idiom, as it is now in use. No detailed presentation of the morphology of this dialect has been made as yet, although some of its features have been noticed. Nearly all the materials for the present article have been gathered orally from the Passamaquoddies and tested by means of the Mitchell manuscripts.

PHONETICS.

The phonetics of the dialect are comparatively simple. The system followed herein gives to the vowels the Italian pronunciation, except *â* = English *aw* in 'awful' and the indeterminate vowel (*Schwund*) which is indicated by the apostrophe '. There are no nasal vowels in Passamaquoddy, as in Abenaki. *H* has the value of the simple breathing, but the inverted comma ' is the well known Algonquin glottal catch, pronounced like a very soft Arabic medial *Ḥe*. *Ch* often represents a palatal *tsʸ* and between vowels has a tendency to approach *j* = *dsʸ*. The constants *p, t,* k are voiceless surds often approaching *b, d* and hard *g* between vowels, but never

uttered like the English *p(h), t(h), k(h)*. *S* between vowels is sometimes occasionally pronounced almost *z*. The combination *w'* is the whistle peculiar to the Algonquin languages occurring at the end of syllables: *ke'kw;* pronounced *keḥkwu*. *W* in general is a weak consonant; it is almost equivalent to the *u*-vowel. It generally disappears after any prefix, as seen in the paradigm of *wikwus: nikwus; kikwus,* etc. It should be noted that the *l* carries an inherent vowel and is consequently pronounced very like the Polish barred *l*. *N* before consonants, as in the syllables *nta-; nki-* is pronounced with its own inherent vowel *'n*. The spelling of the Mitchell manuscripts is a mixture of English and French influences, frequently using *b, d, g* for *p, t, k; j* for *ch* and an arbitrarily varying system of vocalization. No attempt has been made to follow it in this paper.

The Passamaquoddy is not especially rich in consonantal clusters, although more so than some of the other Algonquin idioms, as for instance the Fox. The following table covers nearly all the consonantal groupings which appear, and even some of these are not true clusters, as they occur in many cases with vocalic *w* and *y*. The Indian manuscripts show many apparent clusters, but they are often really separated by the *Schwund* which no Indian ever writes. Thus clusters with *l, m, n* are suspicious, owing to the probability of inherent vowels. The *Schwund* and inherent vowels may be represented by *e*.

CLUSTERS.

chw: echwéchi, 'must.'

chy: apachyáie, 'when I come.'

k'm: w'-mikmaupaul'tínia, 'they feast together.'

ks: ĕyiks, 'when they are' ('were').

kskw: chwopnokskwâk'n, 'anchor.'

kt: w'máche-m'siktihîk'n, 'he begins to punish him,' but here the *k* probably represents *k'*.

kw: akwét'n, 'canoe.'

kw'p: nisukmékw'p'n, 'as we (paddled) together.'

lk: túlkiu, 'thus' (real).

lh: amalhi'takwuk, 'they make music'; *hel'* 'fawn.'

lkw': alkw'minaútikuk, 'in their midst' (suspicious).

lp: hólpin, 'he sits.'
lt: -toltîtit = participial ending 3 p. pl. (real).
mk: el-ámkikap, 'when they came ashore.'
mkw': kikúmkwuk, 'suckers' (fish).
ms: n-olámsittam'n, 'I pretend to': *lams-,* 'blow.'
msk: ámskowás, 'first.'
nk: (nasal): *k'tunkian,* 'when you have hunted' (real).
nk: ankwoch, 'sometimes' (real).
ns: nsámakwán, 'water' (*n* with inherent vowel: *'n*).
pk: apkulámsek, 'they are overwhelmed by the wind.'
pkw': achápkwalusk, 'perch' (fish).
pn: chwopnokswâk'n, 'anchor' (not *p'n*).
ps: ásups, 'grebe' (sort of crane).
psk: ápskeduk, 'than.'
pt: appatáptow'k, 'they come back'; *náchipt,* 'fetch' (imper.).
pw: apwitowatíchil, 'they stand.'
sh: apakweshes, 'red-headed wood-pecker' (= *s-h;* not the English *sh*).
sk: mosk, 'find.'
skw': peskw, 'one.'
sm: iklísm'n, 'white man' = 'Englishman'; *isměkwes,* 'fish-hawk.'
sn: p'snut, 'basket.'
sp: kúspem, 'lake'; *spāswḗu,* 'morning.'
st: chestésit, 'he being angry'; *stáknut,* 'green.'
sw: k'loswâk'n, 'word.'
th: petholatíchihi, 'when they arrive.'
tk: atkéyi, 'string, cord'; *uskitkamikw,* 'world.'
tm: chepakatm'n, 'that you shall marry.'
ts: mits, 'eat' (imper.).
tsk: mitskun, 'animal dung.'
tw: lakutwâk'n, 'treaty.'

The intonation of the Passamaquoddy is highly tonic. Almost every accented syllable indicates a voice-lift. The voice is dropped on the syllable following the tone; half raised again on the third syllable and dropped again on the fourth: *lakutwâk'n;* pronounced ^{la}*kut*^{wâ}*k'n*. This peculiarity appears to be distinctively Passama-

quoddy, as the kindred Maliseets speak very monotonously and with no especially noticeable voice-lift on the accented syllable. The Abenakis also have a monotonous tone, amounting practically to a drawl.

MORPHOLOGY.

In Passamaquoddy, as in all the other Algonquin idioms, the words are really only indifferent themes, which may be used either in a nominal or a verbal sense. This phenomenon is too well known to require elaboration, but it may be illustrated by the appearance of such indeterminate roots in all the parts of speech; thus, *sak,* 'be strong, rule'; as seen in *w't-ach'wi-saki'tón'l,* 'he must rule'; *saklēyo,* 'it is hard, difficult'; *saklikwáso,* 'it is served up strong'; *sákem,* 'chief'; *sak'mawámat,* 'he who is chief'; *k'ti-sak'mawé'lul,* 'I wish to make you chief'; *sák'ma-wikwām,* 'chief's house'; *sak'mawel-mékw'tech,* 'may it be hallowed' = 'made like a chief,' etc. Similarly, the root *os,* 'shine,' appears in *kís-os,* 'luminary' (probably = 'what can [*kis-*] shine'); hence = 'moon, month'; *el-āse,* 'what shines'; *nip-aúsit,* 'what shines at night'; specifically 'the moon' (*nip* = 'night'), etc. (see just below on *w'li*).

THE SUBSTANTIVE.

All substantives, including adjectival formations, are divided into two classes, animate and inanimate, no attention being paid to sex-gender, which is expressed either by prefixing or suffixing some determining word, or else by the use of a distinct expression; *kínche-més,* 'king' (literally: 'King James,' the name of the first king whose name the Indians heard); but. *kínchemes-iskwé,* 'queen,' with the feminine suffix *-iskwé* (= 'squaw'); *mûin,* 'bear,' but *nápeskw,* 'she-bear'; *pût'p,* 'whale,' but *skwemé'kw,* 'she-whale,' etc. It should be noted that many substantives which we should regard as inanimates are treated as animates in Passamaquoddy and conversely; as *akim,* 'snow-shoe' (an.); but *sat-y-il,* 'blue-berries' (inan. pl.). Genuine inanimates are, *e. g., t'm'hîk'n,* 'axe'; *wik-wam,* 'house,' and words of a similar character.

The plural ending for animate nouns is invariably *-k* and for inanimate *-l,* preceded as a rule by indeterminate connecting vowels;

mûs, 'moose'; pl. *mûs'k; ĕs,* 'clam'; pl. *ĕs'k.* Nouns ending in *-p* and dentals like *skí'tap,* 'man,' insert *-y-* and use the distinct ending *-ik;* pl. *skitápyik;* cf. also *pílchisáp-yil,* 'trouser-braces' (inan.); *píljis* = English 'breeches' + *ap,* 'hang'; lit. = 'hang-breeches.' Participial forms in *-t,* like *ĕ́pīt,* 'woman,' palatalize the *ty* into *ch;* pl. *ĕ́pichik* for *ĕpityik.* The same phenomenon of *y* after labials and dentals appears in *wikhîk'nepyĕ́i,* 'inkstand' (*wikhîk'n-nep,* 'book-water' + *-ei,* 'thing'), and in *met-y-ēwéstakw,* 'he is saying' (*met-* = prolonged action + *y* + *ēwest,* 'speak' + participial ending *-akw*). Note also *sat-y-il* (inan.), 'blue-berries.' I can find no trace of the connecting vowels *a, o* and *e* (*i*) used after specific consonants, as in Abenaki (cf. Prince, "Ascoli Memorial," p. 349). Sometimes, however, after *-s* a plural in *-w'k* occurs, as *kísos,* 'month'; pl. *kis'w'k,* but, on the other hand, *kakakûs,* 'crow'; pl. *kakakûs'k.* There seems to be a natural affinity in the labial and dental for the connecting *-y-*.

THE ADJECTIVE.

Adjectives may be used as separate indeclinable particles preceding the noun; as *k'chî skí'tap,* 'great man'; *sigi m'tápekwin,* 'fierce warrior.' They may also appear in this indeclinable form adverbially, as *kátamayíwi-k'túnkiyik,* 'while absent they hunt' (lit. 'not' = *katama* + *y,* connecter, + 'being' = *i-wi*). Very common is the use of adjectives ending in *-ko* for animates and *-k'n* for inanimates: *w'lîko skí'tap,* 'good man'; pl. *w'lîkow'k skitápyik; w'lik'n wíkwām,* 'good house'; pl. *w'likn'l wíkwām'l.* The participial *-it* (an.) and *ek* (*ik*) (inan.) is a common adjectival ending as *w'likit,* 'handsome' (an.); *w'likek* (inan.). When the adjective is inflected, it must agree with its nouns, which it may follow or precede: *sāks'l picheyíkil,* 'long stockings' (= *sāks'l*). Some adjectives end in *-l,* as *nék'm'kil skí'tap,* 'big man,' which is not an obviative *l* (see below, Substantival Modifications, No. 1). Adjectives may be formed from nouns by the endings *-wi* as *skitáp-wi,* 'human,' and *-(k)ewi: wíkwamkéwi,* 'homelike.'

The element *w'lî-(oli-)* may also be used adjectivally without inflection, as *w'lî haás,* 'good horse'; *w'lî wíkwām,* 'good house,' and in innumerable combinations both nominal and verbal. Per-

haps no better example of Passamaquoddy polysynthetic formation can be had than the following partial list of the combinations possible with *w'lî: w'lithátm'n,* 'he is glad of it'; *k-elwut,* 'good'; *w'lāmto,* 'he is good natured'; *w'lásewanwechiyánia,* 'they are pleased with him'; *w'laswélt'm'n,* 'he thanks for it'; *olilélm'len,* 'as I wish you well'; *olínm'n'l,* 'he rejoices at it'; *w'lápémkuk,* 'it benefits hims'; *uliotwâk'n,* 'benefit' (n.); *w'l-wíku,* 'he lives well'='is rich'; *w'tách'wi-w'l-ánkeyowáw'l,* 'they must take good care of him'; *w'lapĕwit,* 'beautiful'; *w'lapēwiū,* 'handsome man'; *w'leyów'l,* 'he treats her well'; *w'litham'l,* 'he consents'; *w'l-okhedím'k,* 'sport, game,' etc.

THE PRONOUN.

The demonstrative pronouns are *yut,* 'this' (nearer); *wut,* 'this'; *na,* 'that,' indeclinable; and *nit, nit,* 'that.' See below for the obviative inflection. The relative for both classes and numbers is *eli-,* followed by the participle: *eli-uskichínwit,* 'he who is an Indian'; *eli-meksit,* 'who finds.' Sometimes the relative is expressed by the participle alone: *askowaltíchik,* 'those who wait.' The interrogative pronoun is as follows: animate *wen,* 'who'; obv. *wen'l;* also='someone'; obviative pl.=*wenihi;* inanimate: *ke'kw,* 'what'; also='something'; pl. *ke'kw's'l,* 'some things, things.'

A highly important feature of the language is the combination of both nouns and verbs with personal pronouns, by which means most of the inflection is carried on. In the following table of personal pronouns, it will be observed that the Passamaquoddy, like its Algonquin congeners, has two first persons plural; an inclusive and an exclusive, the first of which implies that the person and persons addressed are included with the speaker, while the second form excludes the person or persons addressed; *i. e.,* the first='I, you' and 'they,' and the second='I' and 'they.'

SEPARABLE PERSONAL PRONOUNS.

I, *nīl.*	We (incl.), *kîl'n.*
Thou, *kīl.*	We (excl.), *nîl'n.*
He, she, it, *nĕg'm.*	You, *kîl'wau.*
	They, *nĕg'mau.*

The primitive elements of the first, second and third persons are *n, k* and *w(o)*, prefixed in the separable pronouns above to certain demonstrative elements which are practically identical in all the dialects. These elements *n, k, w(o)* may also be prefixed to nouns, to indicate possession, and to verbs, to denote conjugational inflection. It will be noticed, however, that the separable pronoun of the third person is represented by a demonstrative particle *neg'm* (Abenaki *ag'ma;* Delaware *nekama*). This has no connection with the *w(o)*-prefix, but is a combination of the ordinary demonstrative *n(a; ni)*, 'that one' + the asseverative *ga,* seen in *gak* (*passim*), and the possessive *-m* (see below).

The following diagram will illustrate the Passamaquoddy method of combining the pronominal *n, k, w(o)* with the animate and inanimate forms of nouns in order to denote the possessive relation.

Def. An.

nimía níkwus, 'I see my mother.'
k'nimía kíkwus, 'Thou seest thy mother.'
w'nimíal wikwús'l, 'He sees his mother.'
k'nimiánna kikwús'n, 'We (incl.) see our mother.'
nimianna nikwús'n, 'We (incl.) see our mother.'
k'nimiáwu kikwús'wu, 'You see your mother.'
w'nimiáw'l wikwús'w'l, 'They see their mother.'

Def. Inan.

nimí'ton nt'm'hîk'n, 'I see my axe.'
k'nimí'ton k't'm'hîk'n, 'Thou seest thy axe.'
w'nimí'ton w't'm'hîk'n, 'He sees his axe.'
k'nimí'tonen k't'm'hîk'nen, 'We (incl.) see our axe.'
nimí'tonen n't'm'hík'nen, 'We (excl.) see our axe.'
k'nimí'tónia k't'm'hík'n'wu, 'You see your axe.'
w'nimí'tónia w't'm'hîk'n'wu, 'They see their axe.'

Def. An. Plural.

nimíuk nikwús'k, 'I see my mothers.'
k'nimîuk kikwús'k, 'Thou seest thy mothers.'

w'nimía wikwus, 'He sees his mothers.'
k'nimiánnawa kikwúsn'w'k, 'We (incl.) see our mothers.'
nimiánnawa nikwúsn'w'k, 'We (excl.) see our mothers.'
k'nimiáwu kikwús'w'k, 'You see your mothers.'
w'nimiáwu wikwusw'l, 'They see their mothers.'

Def. Inan. Plural.

n'mí'ton'l nt'm'hîk'n'l, 'I see my axes.'
k'nimí'ton'l k't'm'hîk'n'l, 'Thou seest thy axes.'
w'nimi'ton w't'm'hîk'n'l, 'He sees his axes.'
k'nimi'tonén'w'l k't'm'hîk'nen'l, 'We (incl.) see our axes.'
nimi'tonén'w'l n't'm'hîk'nen'l, 'We (excl.) see our axes.'
k'nimi'ton'w'l k't'm'hîk'n'w'l, 'You see your axes.'
w'nimi'ton'w'l w't'm'hîk'n'w'l, 'They see their axes.'

Indef. Animate.

nimía wíkwus, 'I see a mother'; *wikwus'k,* 'mothers.'
k'nimía wíkwus, 'Thou seest a mother'; *wikwus'k,* 'mothers.'
w'nimía wikwús'l, 'He sees another'; *wikwus.*
k'nimiáp'n wíkwus, 'We (incl.) see a mother; *wikwus'k,* 'mothers.'
nimiáp'n wíkwus, 'We (excl.) see a mother'; *wikwus'k,* 'mothers.'
k'nimiá'pa wíkwus, 'You see a mother'; *wikwus'k* 'mothers.'
w'nimiánia wikwus'l, 'They see a mother'; *wikwus,* 'mothers.'

Indef. Inanimate.

nimi'to t'm'hîk'n, 'I see an axe'; *t'm'hîk'n'l,* 'axes.'
k'nimí'to t'm'hîk'n, 'Thou seest an axe'; *t'm'hîk'n'l,* 'axes.'
w'nimi'to t'm'hîk'n, 'He sees an axe'; *t'm'hîk'n'l,* 'axes.'
k'nimi'tonḗp'n t'm'hîk'n, 'We (incl.) see an axe'; *t'm'hîk'n'l,* 'axes.'
nimi'tonḗp'n t'm'hîk'n, 'We (excl.) see an axe'; *t'm'hîk'n'l,* 'axes.'
k'nimi'tó'pa t'm'hîk'n, 'You see an axe'; *t'm'hîk'n'l,* 'axes.'
w'nimí'tow'k t'm'hîk'n'l, 'They see an axe'; *t'm'hîk'n'l,* 'axes.'

The scheme of the possessive prefixes and suffixes for nouns is then as follows:

Animates.		Inanimates.	
Singular.		Singular.	
n——	*k*——*'n*		*k*——*en*
k——	*n*——*'n*	*n*——*'l*	*n*——*en*
w——*l*	*w*——*w'l*	*k*——	*k*——*'wu*
	w——*w'l*	*w*——	*w*——*'wu*
Plural.		Plural.	
n——*k*	*k*——*n'w'k*	*n*——	*k*——*-en'l*
	n——*n'w'k*		*n*——*-en'l*
k——*k*	*k*——*w'k*	*k*——*'l*	*w*——*w'l*
w——(*i, o*)	*w*——*w'l*	*w*——*'l*	*k*——*w'l*

When a noun begins with a vowel or with an *l* (really *'l* with inherent vowel), a dental is inserted after the pronominal prefix: ***nt-akim***, 'my snow-shoe'; ***nt-latwewâk'n***, 'my language.' When a noun begins with *w*, as *wíkwus*, the *w*-prefix coalesces with the initial, as shown above. Many substantives beginning with *m*, especially those denoting a part of the body, lose their *m* when inflected possessively: ***m'huk***, 'body'; ***n'huk***, 'my body,' etc.

Substantival Modifications.

The following seven noteworthy modifications of the substantive appear in Passamaquoddy:

1. The so-called obviative or accusative-ending of the third person occurs only when the animate noun stands in connection with a verb or possessive pronoun in the third person. There is no inanimate obviative. This accusative, which is peculiar to all the Algonquin dialects, is denoted in Passamaquoddy by *-l* in the singular, and in the plural often by the absence of any ending, or by *-i*, *-o*. The following instances will suffice to illustrate the application of this form: (*a*) *w'nimíal haásw'l*, 'he sees the horse,' but *k'nimía haás*, 'thou seest the horse'; (*b*) *w't-aásw'l*, 'his horse'; pl. *w't-aás*, 'his horses'; *wikwús'l*, 'his mother'; pl. *wikwus*, 'his mothers'; (*c*) to express a dative relation: *w'mílan haásw'l skitápyil*, 'he gives (him) the horse to the man,' but *k'mílen haás*, 'I give thee a horse'; cf. also in the participle: *nōtáchil*, 'those who hear him.' As to form, note the sing. obv. *skitápyil*, 'man'; *mûinyil*, 'bear,' but pl. *skitá'pihi; mûini; haáso; mûso*, 'moose'; *ma'tekwéss-o*, 'rabbits';

wá'sis, 'children'; *hamwes,* 'bees.' Note the double obv. ending in *ép'sîyil,* and also regularly *ép'sî,* 'trees,' from *ep's.* The obv. pl. is irregular in the demonstrative pronouns, which are inflected as follows: *wut,* 'this'; obv. sing. *wut'l,* and *wahat;* pl. an. *wut'k;* obv. *wutihi* (often *wahat*); pl. inan. *wut'l; yut,* 'this' (nearer); obv. sing. *yut'l,* and *yahat;* pl. an. *yukt,* obv. *yu'tihi* (often *yahat*); inan. pl. *yu't'l; na,* 'that' (indeclinable); *ni, nit,* 'that' (nearer); obv. sing. *ni't'l,* and *nihit;* pl. an. *nikt;* obv. *nihit* (rarely *ni'tihi*); pl. inan. *nilt'l.* The distinction between the singular and plural obviative is often not observed, even in nouns and verbs.

This case frequently appears as the subject of a sentence. *pechihalina w'skinosís'k w'nichálkw'l,* 'then comes the lads' uncle'; *mach'kowdîtit pesēsmowi nisumatíchihi,* 'as soon as their star-husbands have gone away.' Sometimes the obviative is omitted entirely: *w'néklan lámp'kwinóskwesís'k,* 'he leaves the water-sprites'; a fairly common phenomenon. The obviative frequently appears in verbs: *wítāpekamáspenihi,* 'he was a friend to them.'

There is no trace in Eastern Algonquin of the so-called surobviative or third personal accusative of the Cree and Ojibwe.

2. The locative-instrumental is expressed by *-k;* pl. *-ikúk,* which has the force of a number of English prepositions; viz., 'at, by, from, in, into, on, to,' according to the directive force of the verb with which it stands in construction: *Péssank,* 'at Bar Harbor'; *kowás'k,* 'by means of a log,' a common locution; *wéchi-notgát'wul-to w'tûn'k,* 'they crawl out of his mouth'; *móskesí'to paskán'wikúk,* 'he crawls out of the pits'; *wíkwām'k,* 'in' or 'into the house,' according to the sense of the verb; *kowásnok,* 'on a log'; *k'm'tkinans'n'k,* 'to our land,' etc. The locative *-k* is regularly inflected with the possessive suffixes as follows: *nt'm'hîk'nk,* 'on my axe'; *k't'm'hîk'n'k,* 'on thy axe'; *w't'm'hîk'nk,* 'on his axe'; *k't'm'hîk'n'n'k,* 'on our axe' (incl.); *nt'm'hîk'n'n'k,* 'on our axe' (excl.); *k't'm'hîk'n'w'k,* 'on your axe'; *w't'm'hîk'n'w'k,* 'on their axe.'

3. The vocative element *-tuk* as in *nitapé'tuk,* 'O my friends'; *wásistuk,* 'O my children,' used only with the plural, had originally a dubitative meaning='as many as there are.' This force is still existent in Cree and Ojibwe.

4. The possessive suffix *-m* contains the same demonstrative element as the *-m* of *neg'm,* 'he, she, it': *nt-aás'm, k't-aás'm,* 'my, thy horse,' etc.

5. The diminutive *-sis* is very common: *t'm'hîk'nsís,* 'little axe'; *pílskwesís,* 'girl' (*pil,* 'young' + *skwe,* 'woman' + *sis*), etc.

6. Here must be noted also the movable future *-ch,* which may be affixed indifferently to nouns or to verbs: *k'nimíol-ch,* 'I shall see you,' but *wikwām-ch nimi'ton,* 'I shall see the house.' There is another more vivid future expressed by *lî,* 'go,' preceding the verb: *k't'lî-nimíol,* 'I shall (am going to) see thee.'

7. Finally in this connection, similar to the *-ch* is the movable conditional particle *-p,* as *nt'liáp'n'p sámakwān'k skatúchi pískononōk,* 'we (excl.) should go upon the water, if there were no fog'; here with the verb, but *tahalo-p ke'kw-yali kwilwatakw,* 'as if they were seeking something' (*ke'kw*). Sometimes this appears doubled: *nil'p-lo nt-étum-níswinén'p,* 'I should take it along.'

So far as I am aware there is no interrogative state such as occurs in Ojibwe. As in all Algonquin dialects, the genitive relation is expressed often by means of simple apposition: *aut niméskw'k,* 'road (*aut*) of the spirits'; *ótenesís w'lî-p'maus'wín'w'k,* 'a village (*otenesis*) of good people'; often, however, by the locative *-k;* *sipsís'k skwú't'k,* 'birds of (in) fire' (*skwu't'k*). Sometimes the genitive is indicated by the possessive relation: *w'skínōsís w'nichálkw'l,* 'the lad his uncle'.

As shown above, the dative is expressed by means of the verb and obviative substantive, if the verb is in the third person.

THE VERB.

The imperative is the simplest form of the verbal root, as is the case in most languages; thus: *kwaskw,* 'run thou'; *kwaskw'híkw,* 'run ye'; *kwaskwech,* 'let him run.'

The present tense is the main tense of the Passamaquoddy verb, as from it the future, conditional and past are formed by means of suffixes. In fact, it may be truly said that the present is the only real tense of the language. The present is often used for the past in vivid narrative. The following example of the present of the

intransitive stem *p'mauso-*, 'walk, live' (=*p'mî*= prolongation + *aus*, 'live'), will suffice to show the combination of this intransitive form with the pronominal elements: *np'maus*, 'I walk'; *k'p'maus*, 'thou walkest'; *w'p'maúso*, 'he walks'; 'is walking'; *k'p'maús-îp'n*, 'we (incl.) walk'; *np'maús-îp'n*, 'we (excl.) walk'; *k'p'maús-í'pa*, 'you walk'; *w'p'maús'w'k*, 'they walk.' Note also the participle *p'múiso*, 'he is in the act of walking'; *kwen-aúsit*, 'as long (*kwen*) as he lives,' or *m'sî-eli-p'maúsit*, 'all the time (*m'sî*) while (*eli-*) he lives.' Sometimes the singular of the intransitive verb ends in *-in*: *n'kwáskwin*, 'I run'; (*k'*)*kwáskwin*, 'thou runnest'; *w'kwásk-win*, 'he runs.'

The combination of the pronominal elements with the transitive verb is the most difficult feature of the language and is sufficiently illustrated by the following paradigms and scheme of prefixes and suffixes. Note that all forms marked in the following schedules with a single asterisk are indefinite; those with two asterisks show a dative force (cf. **We—Thee,** Incl. in the Paradigm); forms with three asterisks have both indefinite and definite force, while all forms not designated are definite. All forms in Italics are negative and must be preceded by the negative particles *kat, katama, skat*, 'not.' All forms in Roman are positive. The *-ep, -p, -s, -epus* forms in parentheses indicate the imperfect or conditional-subjunctive. **Ind.** = Indefinite; **Def.** = Definite; **P:** = the Imperfect (Past); **An.** = Animate; **Inan.** = Inanimate. All forms in Roman are positive.

PARADIGM OF NIM—"TO SEE."

I—Thee: k'nimíol(-ep); *k'nimiólo(-p; -pus)*. When the verb-stem ends in -l, as mil, 'give,' this form becomes k'milen.

I—Him-Her:[8]nimía (-p); *nimiáwi (-p; -s)*.

I—It: Ind. nimi'to (-p'n); *nimi'towi (-p; s)*. **Def.** nimi'ton(-ep); *nimi'towun (-ep)*.

I—You: k'nimiólpa (-p); *k'nimioló'pa(-pus)*.

I—Them (An.): Ind. nimía; *nimiawíwu (-p)*. **Def.** nimíuk; **P.** nimiá-penik; *nimiawíwuk;* **P:** *nimiawiwápenik*.

[8] In this verb, the forms: *nimía, nimi'to, nimi'ton'l* stand for *n'nimía, n'nimi'to*, etc., with the prefix *n'* of the first person, assimilated to the *n* of the verbal root. The second personal *k'* also assimilates to a *k*-stem: *kwás-kwin*, 'thou runnest.'

I—Them (Inan.): nimí'ton'l; **P:** nimi'tonépenil; *nimí'towunul;* **P:** *nimi'towunépenil.*

Thou—Me:[4] k'nimí-h-i (-p; -pus); *k'nimihíwi(-p).*
Thou—Him-Her: k'nimía(-p); *k'nimiáwi(-p).*
Thou—It: **Ind.** k'nimíto(-p); *k'nimi'towi(-p).* **Def.** k'nimíton(-ep); *k'nimí'towun(-ep).*
Thou—Us: k'nimiáp'n(-ep; -es); *k'nimiawíp'n (-ep).*
Thou—Them (An.): **Ind.** k'nimía; *k'nimiawíwu.* **Def.** k'nimíuk; *k'nimiawíwuk;* **P:** k'nimiápenik; *k'nimiawiwápenik.*
Thou—Them (Inan.): **Ind.** k'nimi'tónia; *k'nimi'towuno.* **Def.** k'nimi'ton'l; *k'nimí'towun'l.*

He-She—Me: nimióg'n(-ep); *nimióg'wi(-p).*
He-She—Thee: k'nimióg'n(-ep); *knimióg'wi(-p).*
He-She—Him-Her: **Ind.** w'nimía(n);[5] *w'nimiáwi (-p'n).* **Def.** w'nimíal; **P:** w'nimiáp'n; *w'nimiawíwul;* **P:** *w'nimiawíp'n.*
He-She—It: **Ind** w'nimi'to (-p)' *w'nimi'towi(-p).* **Def.** w'nimi'ton(-ep); *w'nimi'towun(-ep).*
He-She—Us: k'nimióg'nen(-ep); *k'nimiog'wínen(ep).*
He-She—You: k'nimióg'wu (-ep); *k'nimiog'wíwu (-p).*
He-She—Them (An.): **Indef.** w'nimía; *w'nimiawíwu.* **Def.** w'nimíuk; **P:** w'nimiápenik; *w'nimiawíwuk;* **P:** *w'nimiawiwápenik.*
He-She—Them (Inan.): w'nimí'ton'l; **P:** w'nimi'tonép'n; *w'nimi'tówunul;* **P:** *w'nimi'towunépenil.*

We (Incl.)—Thee: k'nimiólp'n (-opus); *k'nimiolóp'n;* **P:** *k'nimiolopóp'n (-us),* but note k'pechíptolnén(-ep) = **Def.** 'we bring it to thee'; *k'pechiptolónen(-ep).*
We (Incl.)—Him-Her: **Indef.** k'nimiáp'n (-ep): *k'nimiawíp'n(-ep).* **Def.** k'nimiánen (-ep); *k'nimiawínen (-ep)*; k'nimianna; *k'nimiawinna.*
We (Incl.)—It: **Indef.** k'nimi'tonép'n(-ep); *k'nimi'towíp'n (-ep).* **Def.** k'nimí'tonén (-ep); *k'nimi'towinén (-ep).*

[4] The element *-h-* is a connecting consonant here.
[5] The *n*-form, really participial, is often definite; see note 9 below.

We (Incl.)—You:[6] k'nimiólp'n (-ep and -opus); *k'nimiolóp'n;* **P:** *k'nimiolopóp'n.* This form has a similar dative combination to that in **We (Incl.)—Thee** above, i. e., k'—olnen; *k'—olonen.*

We (Incl.)—Them (An.): Indef. and **Def.**[7] k'nimiánnawa; **P:** k'nimiannawápenik; *k'nimiawinnawa;* **P:** *k'nimiawinnawápenik.* **Def.** k'nimiánnawuk; **P:** k'nimiannawépenik; *k'nimiawinnawuk;* **P:** *k'nimiawinnawépenik.*

We (Incl.)—Them (Inan.): k'nimi'tonénwul; **P:** k'nimi'tonenépenil; *k'nimi'towinén'wul;* **P:** *k'nimi'towinenépenil.*

We (Excl.)—Thee, Him-Her, etc., differs from the above forms only by the n-preformative instead of the k'.

You—Me:[4] k'nimi-h-í'pa; *k'nimi-h-iwí'pa.*

You—Him-Her: Indef. k'nimiá'pa (-p); *k'nimiawí'pa (-p).* **Def.** k'nimiáwa (-p'n); *k'nimiawíwa(-pn).*

You—It (Indef.): k'nimitó'pa(-p'n); *k'nimitowí'pa(-p'n).* **Def.** k'nimitónia; **P:** k'nimitoniáspenil; *k'nimi'towúno;* **P:** *k'nimi'towiwáspenil.*

You—Us:[4] k'nimi-h-íp'n (-ep; epus); *k'nimiyawíp'n(-ep; epus).*

You—Them (An.): Indef. k'nimiánia; *k'nimiawiyania.* **Def.** k'nimiáwa; **P:** k'nimiawépenik;[7] *k'nimiawíwu;* **P:** *k'nimiawiwépenik.*

You—Them (Inan.): Indef. k'nimi'tónia; *k'nimi'towúno;* **P:** k'nimi'tonépenil; *k'nimi'ton'wiwépenil.* **Def.** k'nimi'ton'wul; *k'nimi'towún'wul.*

They—Me: nimióguk; **P:** nimiogópenik;[8] *nimiog'wíwuk;* **P:** *nimiog'wiwépenik.*

They—Thee: k'nimióguk; **P:** k'nimiogópenik; *k'nimiog'wíwuk;* **P:** *k'nimiog'wiwépenik.*

[6] Note that **We-Thee** is identical with **We-You,** no distinction being made here between the singular and plural object. But cf. **They-Thee** and **They-You,** where the distinction is made!

[7] Note the apparently arbitrary difference in vowel in the past: *-wápenik* for the **Indef.** and *-wépenik* for the **Def.** Observe that *k'nimiánnawa* is used both with indefinite and definite nouns. When the noun is not expressed *k'nimiánnawuk* is used.

[8] The *o* in *-ópenik* is plainly due to vowel harmony from the *ŏ* inherent in *nimiŏguk,* but note the *ŏ* in *w'nimi'towiwŏpenik,* 'they do not see it,' where the *o*-vowel seems to be due to assimilation to the negative *w.*

They—Him-Her: **Indef.** w'nimiánia; **P:** w'nimiápenik; *w'nimiawiyánia;* **P:** *w'nimiawiwápenik.* **Def.** w'nimiáwul; **P:** w'nimiawápenik; *w'nimiawíwul;* **P:** *w'nimiawiwápenik.*

They—It: **Indef.** w'nimí'towuk; **P:** w'nimi'tópenik; *w'nimi'towíwuk;* **P:** *w'nimi'towiwópenik;*[8] **Def.** w'nimi'tónia; **P:** w'nimi'toniápenil; *w'nimí'towúno;* **P:** *w'nimi'towuniápenil.*

They—Us: k'nimióg'n'wuk; **P:** k'nimióg'nópenik; *k'nimiog'wín'wuk; k'nimiog'wiwápenik.*

They—You: k'nimióg'wuk; **P:** k'nimiogópenik; *k'nimiogowíwuk;* **P:** *k'nimiogowiwépenik.*

They—Them (An.): **Indef.** w'nimiánia; **P:** w'nimiawápenik; *w'nimiawawiyánia;* **P:** *w'nimiawiwápenik.* **Def.** w'nimiáwa or -wul; **P:** w'nimiawépenik; *w'nimiawíwu* or *-wul;* **P:** *w'nimiawiwápenik.*

They—Them (Inan.): **Indef.** w'nimi'tónia; **P:** w'nimi'toniápenil (-aspenil); *w'nimi'towúno;* **P:** *w'nimi'towuniápenil (-aspenil).* **Def.** w'nimi'tón'wul; *w'nimi'towún'wul;* **P:** w'nimi'tonwépenil; *w'nimi'towunépenil.*

Participial Forms.*

I—Thee: nimíolún; táyowe k'nimíol; *nimiólowun; tayowe k'nimiólo.*

I—Him-Her: nimían; táyowe nimía; *nimiawíwun; táyowe nimiáwi.*

I—It: nimí'toan; táyowe nimí'to(n);[9] *nimi'towíwun; tayowe nimi'towi.*

[9] The *n*-form seems to be optional.

I—You: táyowe k'nimólpa; *táyowe k'nimioló'pa.*

I—Them (An.): táyowe nimíuk; *táyowe nimiawíwuk.*

I—Them (Inan.): nimí'toanul; *nimi'towíwanul.*

Thou—Me: nimihíyin; *nimihiwíyin.*

* These forms are used in subordinate as well as in hanging clauses; thus, *nimíolun,* '(when) I see thee,' is also expressed by the finite construction, *táyowe k'nimíol.* In many instances the purely participial form seems to be lacking, as in **I-You, I-Them,** etc. I believe, however, that there are participial forms for all the combinations; possibly forms which my Indian informant did not happen to think of at the moment. In the kindred Canadian Abenaki, it is possible to place purely participial forms for all almost all the possible phrases (Prince, *Miscellanea Linguistica in Onore di Graziodio Ascoli,* p. 358, Ascoli Memorial).

Thou—Him-Her: k'nimíyas;[10] nimían; *nimiawíyan.*
Thou—It: k'nimitoánes;[11] nimi'toan; *nimi'towíwun.*
Thou—Us: k'nimiyápenus;[11] nimiyáp'n; *nimiawíp'n.*
Thou—Them (An.): k'nimíuk; nimían(?);[9] *nimiwíyan.*
Thou—Them (Inan.): nimí'toanul; *nimí'towiwánul.*

He-She—Me: nimíhit; *nimihikw.*
He-She—Thee: k'nimíog:[12] *k'nimióg'wi.*
He-She—Him-Her: táyowe w'nimíal; nimiá'tit; *nimiyakw.*
He-She—It: nimi'toan; *nimi'towiwun.*
He-She—Us: táyowe k'nimióg'nen; *k'nimiog'wínen.*
He-She—You: táyowe k'nimióg'wu; *k'nimiog'wíwu.*
He-She—Them (An.): w'nimiá'tit;[13] *w'nimia'tíkw(?).*
He-She—Them (Inan.): táyowe w'nimí'towul; *w'nimi'towiwul.*

We—Thee: táyowe k'nimiólp'n; *táyowe k'nimiolóp'n (-ópus).*
We—Him: táyowe k'nimiánen or k'nimiáp'n; *táyowe k'nimiawíp'n; nimiyáwin(-us).*
We—It: táyowe k'nimí'tonen; *táyowe k'nimi'towínen; nimi'tow'-néwin(-us).*
We—You: táyowe k'nimiólp'n; *táyowe k'nimiolóp'n(opus).*
We—Them (An.): táyowe k'nimiánnawuk; *táyowe nimiáwan(-ós-penik).*
We—Them (Inan.): táyowe k'nimi'tonénwul; *táyowe nimi'tow'-néwin(-óspenil).*

You—Him-Her: táyowe kílwau k'nimiyáwa; *táyowe k'nimiyawíwa.*
You—Him-Her: táyowe kílwau k'nimiyáwa; *táyowe k'nimiyáwiwa.*

[10] The ending *-s* is common in the past (cf. **We-Them,** participle) and may often be substituted for the characteristic *-p,* or even combined with it, as *-pus* (**We-Thee; We-You**). I suspect that *k'nimíyas, k'nimi'toánes, k'nimiyápenus* are really past forms here; i. e., that my informant understood the English 'see' in the past sense, following the New England dialect of English where 'see' = 'saw.' The *nimían*-form, which is purely participial, seems to be indeterminate, as it means 'I seeing him,' 'thou seeing him, them.' This is also the case with Abenaki *namihoan, namitoan.*

[11] Finite forms; *nimi'toan* and *nimiyáp'n* are the real participles.

[12] Note absence of the finite *-n*; *k'nimiōg'n,* 'he sees thee.'

[13] It is strange to find the *w*-prefix before a participial form; cf. **They-Them** in this list.

You—It: táyowe k'nimi'tónia; *tayowe k'nimi'ton (-iaspenil)*;[14] *sic!*
You—Us: tayowe k'nimihíp'n; *tayowe k'nimiawíp'n.*
You—Them: nimiówuk; *nimiowíwuk.*
You—Them (Inan.): táyowe k'nimi'tónia; *táyowe k'nimi'towúnwul.*

They—Me: nimihiyóguk;[15] *nimihiog'wíwuk.*
They—Thee: táyowe k'nimióguk: *táyowe k'nimiog'wíwuk.*
They—Him: táyowe w'nimiáwul; *táyowe w'nimiawíwul.*
They—It: táyowe w'nimi'tónia; *táyowe w'nimi'towúno.*
They—Us: táyowe k'nimióg'n'wuk; *táyowe k'nimiog'n'wín'wuk.*
They—You: táyowe k'nimióg'wuk; *tayowe k'nimiogowíwuk.*
They—Them (An.): táyowe w'nimia'títit[18] or nimiá'tit; *táyowe w'nimiawíwu.*
They—Them (Inan.): táyowe w'nimia'tótit; *tayowe w'nimi'towún'-wul.*

Passive.

'I am seen,' nimióguk (='they see me'); (*katama*) *nimiog'wíwuk.*
'Thou art seen,' k'nimíuk;[16] (*katama*) *k'nimiokiū.*
'He is seen,' w'n'míkw'so; (*katama*) *w'nimikw'siū.*
'We are seen,' k'nimiokép'n; (*katama*) *k'nimiokép'n.*[17]
'You are seen,' k'nimioképa; (*katama*) *k'nimioképa.*
'They are seen,' nimíkw's'wuk; (*katama*) *nimikw'siwíyik.*

'To Have' (An. Object Sing.).

'I have,' nt-í-wa (-p);[18] *nt-i-yiwáwiw (-wip'n).*
'Thou hast,' k't-í-wa (-p); *k't-iyiwáwiw (-wip'n).*
'He has,' w't-í-wul; **P:** w't-i-wap'n or w't-wáspenil; *w't-i-wawíwul;* **P:** *w't-i-wawíp'n;* but w't-i-yowan, 'he has them.'
'We (Incl.) have,' k't-i-wáp'n (-ep); *k't-i-yiwawíp'n (-ep).*
'You have,' k't-i-wá (-pn); *k't-i-yiwáwina (-p'n).*

[14] This can hardly be correct as a negative form. It seems to be a past form, owing to the *-s-* in *-áspenil.*
[15] Apparently a pure participial formation. The finite form 'they see me' = *nimióguk.*
[16] Apparently a pure passive. Note the finite *k'nimióguk,* 'they see thee.'
[17] It is strange to find no distinctively negative form here.
[18] Probably should be *nt-i-wap'n, kt-i-wap'n* in the past.

'They have,' **Indef.** w't-i-yánia; **P:** w't-i-yápenik; *w't-i-yiyawiyánia.*
Def. w't-i-yáwul; **P:** w't-iyawáp'n; *w't-iyawíwul.*

'TO HAVE' (INAN. OBJECT ALSO = 'TO BE').

n't-í-yin (-es; -ep'n).
k't-í-yin (-es; -ep'n).
w't-í-yin (-es; -ep'n).
k't-i-yíp'n (-es; -ep'n).
k't-i-yí'pa (-es; -ep'n).
w't-i-yínia; **P:** w't-i-yipenik.
w't-í-wul; **P:** w't-i-yipenil.

IRREGULAR ELEMENTS OF "TO HAVE" AND "TO BE."

Third Person Singular: al-ech, 'let it be'; el-e-sin,[19] 'where he is'; eyik, 'he (really 'it') is'; eyit, 'where he is'; el-eyit, 'where he is'; el-i-y-ijil = obviative of eyit; eyin, 'it being'; meskw nit el-i-nook, 'before this is so' (neg.);[20] tan ot'l-i-yin,[21] 'however it may be'; ch'wi-l-eyo, 'it must be'; w't-ach'wi-t-iwal, 'he must have it'; kis-iyit,[22] 'it having been'; w'-kichiyawi-wanyogonia, 'they have enough of it.'

Third Person Plural: m'si ayale, 'all who are'; eyoltitit, 'they being' (reflex.) eli-y-oltitit, 'as they are' (reflex.); el-igek, 'they who are'; etutek, 'they being'; eyilit; obv. eyilijil, 'where they are'; iakw, 'where they are'; weji-ya-witits, 'where they were' (with -s = Past).

SCHEME OF VERBAL PREFIXES AND SUFFIXES.

	ME.	THEE.	HIM-HER.	IT.
I		k—ol, -el (-en)	n—a, u	* n—to
		k—olo	*n—awi*	* *n—towi*
				n—ton
				n—towun

[19] Note *e* for *i* in *el-e-sin, ěyik, ěyit,* due to vowel harmony.
'he has taught me.'

[20] *Meskw,* which is used in the sense 'before,' really means 'not yet,' and therefore takes the negative verb.

[21] Note the phonetic *l* before the vowel root and the connecting *-t-* between the prefix and the *l.*

[22] *Kis,* 'already,' is the sign of the perfect tense; thus, *nkis-ake'kimkon,* 'he taught me.'

Thou	k—i _k—iwi_		k—a, u _k—awi_	* k—to * _k—towi_ k—ton _k—towun_
He-She	n—gon, g'n _n—g'wi_	k—gon, g'n _k—g'wi_	*w'—a(n) *_w—awi_ w—al _w—awiwul_	*w—to *_w—towi_ w—ton _w—towun_
We (Incl.)		*k—olp'n *_k—olop'n_ **k—olnen _k—olonen_	* k—ap'n * _k—awip'n_ ** k—anen, anna _k—awinen, awinna_	* k—tonep'n * _k—towip'n_ k—tonen _k—towinen_
We (Excl.)		*n—olp'n *_n—olop'n_ **n—olnen _n—olonen_	* n—ap'n * _n—awip'n_ n—anen, anna _n—awinen, awinna_	* n—tonep'n * _n—towip'n_ n—tonen _n—towinen_
You	k—i'pa _k—iwi'pa_		* k—a'pa * _k—awi'pa_ k—awa, u _k—awiwa_(_u_)	* k—to'pa * _k—towi'pa_ k—tonia _k—towuno_
They	n—oguk _n—og'wiwuk_	k—oguk _k—og'wiwuk_	*w—ania *_w—awiyania_ w—wul _w—awiwul_	*w—towuk *_w—towiwuk_ w—tonia _w—towuno_

	Us.	You.	Them (An.).	Them (Inan.).
I		k—olpa _k—olo'pa_	* n—a, u * _n—awiwu_ n—uk _n—awiwuk_	*** n—ton'l _n—towun'l_
Thou	k—ap'n _k—awip'n_		* k—a, u * _k—awiwu_ k—uk _k—awiwuk_	* k—tonia * _k—towuno_ k—ton'l _k—towun'l_
He-She	k—og'nen _k—og'winen_	k—og'wu _k—og'wiwu_	*w—a, u *_w—awiwu_ w—uk _w—awiwuk_	***w—ton'l _w—towun'l_

We (Incl.)		k—olp'n *k—olop'n* **k—olnen ***k—olonen*	* k—annawa * *k—awinnawa* k—annawuk *k—awinnawuk*	*** k—tonen'wul *k—towinen'wul*
We (Excl.)		*n—olp'n **n—olop'n* **n—olnen ***n—olonen*	* n—annawa * *n—awinnawa* n—annawuk *n—awinnawuk*	*** n—tonen'wul *n—towinen'wul*
You	k—ip'n *k—awip'n*		* k—ania or * k—awa *k—awiwiyania* k—awu *k—awiwu*	* k—tonia * *k—towuno* k—ton'wul *k—ton'wun'wul*
They	k—og'n'wuk *k—og'win'wuk*	k—og'wuk *k—og'wiwuk*	*w—ania **w—awiyania* w—awa, u *w—awiwul*	*w—tonia **w—towuno* w—ton'wul *w—towun'wul*

A study of the above forms will indicate that the main points of difference between the indefinite and definite combinations lie in the third person singular and throughout the plural. Although, as pointed out above, there is really only one tense, the present, there are certain endings which may be affixed to denote the past relation and the conditional-subjunctive. Thus, the *-p, -ep, p'n* and *-s*-elements are used for the past and conditional-subjunctive alike, and even appear in combination together, as *p's; ep's; op's.* A careful distinction must be made between the past-conditional *-p'n* and the *-p'n* of the first person plural as seen in **Thou—Us** and also in the intransitive verb as given above.

There is also, as shown, a passive voice, the distinctive element of which is *-k, -s* and in combination *-kw's.* Note also the passive forms *m'skówa,* 'he will be found'; *w'metapéksin,* 'it is finished'; *milkónia wásís'l,* 'they are given to the child'; *weswéphogónia,* 'that they be taken back'; *nŏtakw'siánp'n,* 'I was heard' (*nŏt-*)*; kitwitasso,* 'it is called.' Sometimes a reflexive is used for a passive: *elokélit,* 'what was done.' The reflexive, not indicated in the paradigms, is expressed in various ways; *e. g.,* by an *l-,* insert: *w'm'tyayew'lít-el-ín,* 'as if they were playing together'; *kinw-el-úswiū,* 'it shows itself'; *nt'lî-kisi-kwálp-el-es,* 'I will change myself,; *mache-*

kaú-d-il-it, 'as soon as they have gone off together,' etc. Motion is expressed by *pech-: 'pechiyan,* 'when I come'; *w'p'chitakan,* 'he sends him'; *pechi-pawatmat,* 'he is always desirous'; *pechipha,* 'he brings him,' etc.

'Must' is expressed by the insert *ach'wi: w't-ách'wi-sakitón'l,* 'he must rule'; and in the future: *k't-ách'wi-t'lî-wíchiy'n,* 'thou shalt be (*t'li-*) compelled to take heart.'

Desire is indicated by *ktî-: w'k'tî-nimíal,* 'he wishes to see him'; *k'tî-eló'kelít,* 'what he wishes to do.'

There are many other such particles too numerous to mention in an article of this length.

As in all other Algonquin idioms, in the combined forms, the second person always takes precedence over the first and the first person over the third. Thus, in the forms *k'nimíol,* 'I see thee,' where the second person is the object and in *k'nimiáp'n,* 'you see me,' where the second person is the subject, the second personal element comes first. In such forms, however, as *nimía,* 'I see him'; *nimi'to,* 'I see it,' the first person appears in the first place.

The sign of the negative is the infixed *u*-vowel which, as shown above, frequently appears as *o* and often as *w*.

The use of the participle is most varied. Thus, it may take the place of the relative form as *nit pawálkwak,* 'this is what is wanted' (passive indicated by *-kw*), or it may be used to denote the action of the verb governed by a preposition *wechi-nimíolun,* 'in order for me to see thee,' or else it may be employed as a conditional: 'if' or 'when I see thee' = *nimíolun.* The negation of the participle is formed in the same way as the negative of the finite forms, *viz.,* by infixation of *u, o, w.*

Any noun may be verbalized by the ending *e-: w'skitáp-e,* 'he is a man'; by *-ewi: tan etúchi w'skitápewi,* 'so long as he is a man'; by *-ewiū: w'skitápewíū,* 'he becomes a man'; also by participial endings: *w'skitápewít,* 'he who is a man'; or by *-(w)eleso: w'skitápe-wéleso,* 'he becomes a man.'

Practically all the Passamaquoddy verbs are conjugated after the above model, most of the minor variations which occur being due to phonetic peculiarities.

NUMERALS.

The numerals up to five present three forms; *viz.*, a form used chiefly in counting, and adjectival animate and inanimate forms, as indicated by the following list. There is no trace of peculiar numerals used only with certain classes of substantive, as, for example, with round objects, etc., such as occur in Ojibwe.

	In Counting,	Animates.	Inanimates.
one	...........*nekw't*	*peskw;* obv. *-'l* *peskw* 'one, each' occasionally: *nekw't*	*peskw* occasionally: *nekw't*
two	.............*tā́bū*	*nĭswuk* obv.: *nĭsō*	*nĭsn'l*
three	.............*sist*	*nṓwuk* obv.: *nōhō*	*nṓw'n'l*
four	.............*nḗu*	*nḗwuk* obv.: *nēwō*	*nḗw'n'l*
five	..............*nā́n*	*nā́n'w'k* obv.: *nānō*	*nā́n'w'l*
six	.........*kamáchin*	*kamáchin-keswuk* obv. *-kesō*	*kamáchin-kesn'l*
seven	........*'lwîk'n'k*	*'lwîk'n'k-keswuk* obv. *-kesō*	*'lwîk'n'k-kesn'l*
eight	.......*'km''lchin*	*'km''lchin-keswuk* obv. *-kesō*	*'k''mlchin-kesn'l*
nine	......*eskw'nátek*	*eskw'nátek-keswuk* obv. *-kesō*	*eskw'nátek-kesn'l*
ten	...........*m'tul'n*	*m'tul'n-keswuk* obv. *-kesō*	*m'tuln'-kesn'l*

From eleven to fifteen, the numerals are formed from the tens by affixing for the animate and inanimate *-ankoso-wuk;* obv. *ankoso; ankosow'l,* respectively: *nisánkosowuk skitayik,* 'twelve men'; *nsánkoso-wuk* (obv. *-wo*); inan. *-w'l,* 'thirteen'; *newánko-,* 'fourteen,; *nananko,* 'fifteen.' From sixteen to nineteen the affixed element is *-kesánko: kamáchin-kesánkosowuk* (obv. *-kesánkoso*); inan. *-kesánkosow'l,* etc.

From twenty on, the cardinal elements are as follows: *nisínsk-,* 'twenty'; *nsínsk-,* 'thirty'; *nēowinsk-,* 'forty'; *nanínsk-,* 'fifty'; *kamáchin-kesínsk-,* 'sixty'; *'lwîk'n-kesínsk,* 'seventy'; *'km''lchin-kesínsk,* 'eighty'; *eskw'nátek-kesínsk,* 'ninety'; *nekw'tát'kw-,* 'hun-

dred.' The animate and inanimate plurals are made from these forms by affixing the element; animate *-ke'kw'sowuk* (obv. *-ke-'kw'so*); *inan. -ke'kw'-sow'l;* as *nēowinsk-ke'kw'sowuk skitápyik,* 'forty men,' etc.

The ordinals, with the exception of *amsk'was,* 'first,' are formed from the cardinals by adding *-ewéi* (*nisewéi,* 'second'; *nōwewéi,* 'third,' etc.), until tenth which is *nekw'tinskewéi.* To the element *-anko* of the -teens is added the ending *-wewei; nēowánkowewéi,* 'fourteenth,' and to the ending *-insk* is added *-ekewei;* as *nisinske-kewéi,* 'twentieth.'

The numerals are usually inflected adjectivally preceding their substantives, but they may be used indeclinably, as *eskw'nátek-kesōk'niū,* 'nine days.'

The following love-song will serve to illustrate both the present musical style of the tribe, which is undoubtedly influenced by the Roman Catholic Gregorian chants of their missionary priests, and also the construction of the language. It should be noted that the last syllable *û* of the song must be prolonged as much as possible, and finally allowed to end with a rapid expulsion of the breath, this is the so-called "die-away" which is a characteristic of much of the American music.

Peskî k't-el-ápin elmî-nelemwík
Lonely thou lookest up-stream
Elmî-sîkwak-lo takwâk'nwi-lok-lo
In spring and in autumn;
Chîptuk k'nimihí-sa kwilakweyún
Perhaps thou mayest see me seeking thee.
Kuwēnodîn Û; kuwēnodin Û.
It is long O; It is long O!


Pes - kî k'te - la - pin el - mî -
ne - lem-wik; el - mî - sî-kwak-
lō ta-kwa-k'n - wi - lok - lō chip-
tuk ke-ni - mi - hî - sa kwi - la-kwe-yun
ku - wê - no - din
u ku - we - - no - din u

Commentary.

In the above song, *peskî* is adverbial from *peskw,* 'one; lonely'; *k't-el-apin:* *k* = second person + the infixed *-t-* before a vowel + *el,* the element of prolongation, 'thou art doing it' + *ap,* 'look' + the intransitive *in,* seen above in the conjugation of *kwaskw,* 'run'; *elmî-nelemwík:* *elmî* = 'being; while there' + *nelemwík,* which also occurs in the form *nul'muk,* showing the indeterminate vowel = 'up' —here probably 'up-stream'; *elmî-sikwak-lo:* *elmî,* here, = 'during' + *sîkwak,* a musical prolongation of *sîkw'k,* 'in spring' + *lō,* the asseverative particle; *takwâk'nwi-lok-lo:* *takwâk'n,* 'autumn,' + the adjectival *-wi* + the verbal inan. *-lok* + the asseverative *-lo;* *chîp-tuk,* probably contains the same element as the future *-ch* + the verbal *p't* + the inan. participle *-uk* = 'when it may be'; *k'nimi-hi-sa,* 'thou seest me' with the conditional *-sa;* *kwilakwiyun:* *kwil,* 'seek,' + the formative *ak* + *(w)iyun,* participial, = **I — Thee;** *kuwēnodin U:* *kwēn,* 'be long,' + the intransive endings *-(o)din* + the exclamation *U* = 'Oh.'

DISCUSSION OF "A KINETIC THEORY OF GRAVITATION."

(PLATE I.)

BY CHARLES F. BRUSH.

(*Read April 24, 1914.*)

I. GRAVITATION IS DUE TO INTRINSIC ENERGY OF THE ETHER.

At the Minneapolis meeting of the American Association for the Advancement of Science I had the honor to outline "A Kinetic Theory of Gravitation,"[1] which is in substance briefly as follows:

The ether is assumed to be endowed with vast intrinsic kinetic energy in wave form of some sort capable of motive action on particles, atoms or molecules of matter, and propagated in every conceivable direction so that the wave energy is isotropic. The waves are of such low frequency, or otherwise of such character, that they pass through all bodies without obstruction other than that concerned in gravitation. Distribution of the ether's energy is uniform throughout the universe except as modified by the presence of matter.

Atoms or particles are imagined to be continually buffeted in all directions by the ether waves like particles of a precipitate suspended in turbulent water. There are no collisions because neighboring particles follow very nearly parallel paths.

Each particle or atom of matter is regarded as a center of activity due to its energy of translation initially derived from the ether. There is continual absorption and restitution of the ether's energy, normally equal in amount; but the ether is permanently robbed of as much of its energy as is represented by the mean kinetic energy of the particle or atom. The particle or atom thus has a field of influence extending in all directions, or casts a spherical energy shadow, so to speak, the depth or density of the shadow varying with the inverse square of distance. The energy shadow of a body of matter

[1] *Science*, March 10, 1911; *Nature*, March 23, 1911.

is the sum of the shadows of its constituent parts. The energy shadows of two gravitating bodies interblend, so that the energy density between them is less than elsewhere, and they are pushed toward each other by the superior energy density, or wave pressure, on the sides turned away from each other.

That the ether really *is* endowed with vast intrinsic energy in some form or forms is the belief of many eminent physicists, and it seems to me highly probable that *all* energy has its source and destination in the ether; that is to say, that energy in all the various forms in which we observe it, comes in some way from the ether and is energy *of* the ether. This view does not in any manner conflict with the principle of conservation of energy.

In support of my contention that etherial energy is the cause and essence of gravitation, I wish to emphasize particularly, what seems to me an obvious fact, that the energy acquired by a falling body comes from the ether, and is restored to the ether when that body undergoes negative gravitational acceleration.

In this connection I cannot do better than quote Lord Kelvin's description of the collision of two very large bodies through the influence of gravitation. In his "Popular Lectures and Addresses" (Vol. 1, 413–417) he says:

"To fix the ideas think of two cool solid globes, each of the same mean density as the earth and half the sun's diameter, given at rest, or nearly at rest, at a distance asunder equal to twice the earth's distance from the sun. They will fall together and collide in exactly half a year. The collision will last for about half an hour, in the course of which they will be transformed into a violently agitated incandescent fluid mass flying outward from the line of motion before the collision and swelling to a bulk several times greater than the sum of the original bulks of the two globes. . . . The time of flying out would probably be less than half a year when the fluid mass must begin to fall in again towards the axis. In something less than a year after the first collision the fluid will again be in a state of maximum crowding towards the center, and this time even more violently agitated than it was immediately after the first collision; and it will again fly outward, but this time axially towards the places whence the two globes fell. It will again fall inwards, and

after a rapidly subsiding series of quicker and quicker oscillations it will subside, probably in the course of two or three years, into a globular star of about the same mass, heat and brightness as our present sun."

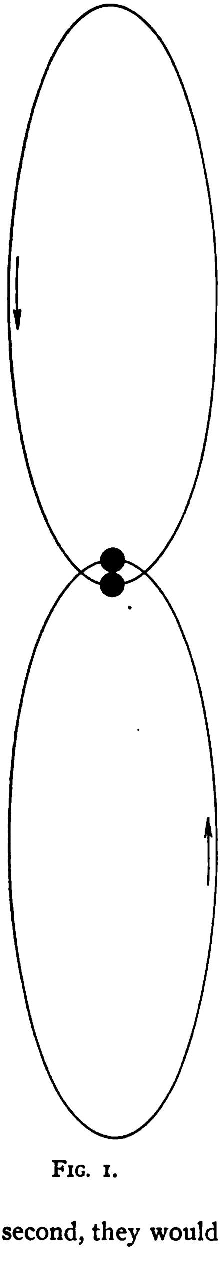


FIG. 1.

Undoubtedly this is a substantially correct description of what would happen under the conditions named. The two cold bodies would acquire *from some source external to themselves* the vast energy represented by the heat of the sun, heat sufficient to maintain the enormous solar radiation millions of years without sensible diminution. And this vast accumulation of energy would occur in half a year, largely in the *last few days* before collision. There is, to me, no conceivable source of this energy other than the ether. It may be argued that the two cold bodies, as a gravitating system, initially possessed all this energy in the form of "potential energy of position." This is a most convenient expression, but it affords no explanation of the *source* of the energy until, as I pointed out at the Washington meeting, we take the energy-endowed ether into partnership as an essential part of the system. Certainly the energy could not be resident in the two cold motionless globes. For a homely illustration, think of two golf balls joined by a stretched thread of rubber; they form an attracting system and possess "potential energy of position" or separation, but the energy does not reside in the balls, it is in the stretched rubber thread.

Later in his description Lord Kelvin says: "If, instead of being at rest initially, . . . each globe had a transverse velocity of three quarters (or anything more than .71) of a kilometer per second, they would just escape collision, and would revolve in ellipses

round their common center of inertia in a period of one year, just grazing each other's surface every time they came to the nearest points of their orbits." (Assuming of course that the globes were sufficiently rigid to escape disruption by tidal forces.)

To aid in forming a mental picture of this last case described by Lord Kelvin, in which the two globes fall together but do not collide, I have made a diagram (Fig. 1) of the two elliptical orbits; and in order to show the globes of appreciable size, the orbits are made very much less excentric than Kelvin's premises call for. The globes are shown at perihelion, just escaping collision. Of course, the globes in falling from aphelion to perihelion would gather the same amount of energy that they did in the case of collision, where their motion was arrested and their kinetic energy was thus converted into heat; but without collision the vast energy acquired during positive acceleration from aphelion to perihelion would disappear during negative acceleration from perihelion to aphelion, and be transformed back to the ether whence it came.

The sun and planets of the solar system, and the planets and their satellites, because of the excentricity of their orbits, continually go through the same kind of cycle described by Lord Kelvin, differing from that only in degree. For instance, the earth in its six months' passage from aphelion to perihelion falls about three million miles toward the sun, and gains in orbital velocity about five eighths of a mile per second. It thus acquires new kinetic energy from the ether which, if it could be manifested as heat, would be sufficient to evaporate all the oceans, lakes, and rivers, heat the dry earth to vivid incandescence, and vaporize much of it; the earth would become a miniature sun. And all this energy is restored to the ether during the next half year while the earth is moving from perihelion to aphelion.

With the idea in mind that a falling body gathers energy from the ether, and restores all of it to the ether when raised the same distance against gravitation, *by any means*, homely examples are at once suggested; thus, a stone thrown upward and falling again, does it in the reverse order, and a common clock pendulum goes through, and repeats the cycle with almost the regularity of a sun and planet.

In the theory of gravitation under discussion, the only new postu-

late is that some or much of the ether's intrinsic energy is *kinetic* and consists of some sort of wave motion or energy flux, whereby a disturbance at any point in the free ether is ultimately felt everywhere else, diminishing in intensity, of course, with the inverse square of distance from the seat of disturbance.

It is not difficult to conceive of kinetic energy in the ether quite apart from matter. Radiation is one form of such energy, and when once launched in the ether it is persistent and quite independent of its source. Interstellar space is alive with wave energy radiated from countless suns, and at points far removed from any single sun this energy is approximately isotropic. Of course this known isotropic wave energy in the ether of space is far too feeble to play any appreciable part in gravitation, and I call attention to it only for the purpose of showing that one sort of free isotropic wave energy in or of the ether in celestial space is already a known phenomenon.

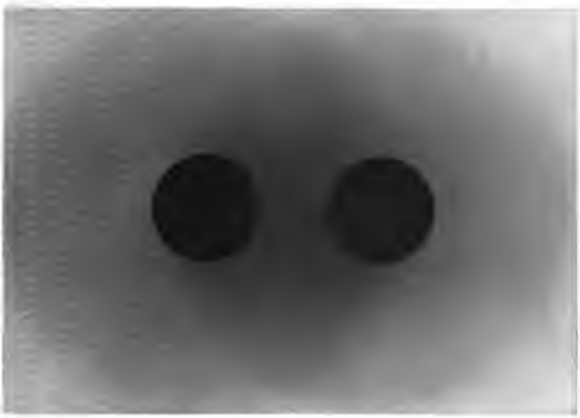
Probably the ether waves concerned in gravitation are not the transverse kind known to us, though it is not difficult to think of transverse waves of great amplitude, embodying great energy, and of such great length that they pass freely through all bodies without appreciably heating them—even electrical conductors. (Incidentally, we cannot be sure that the intrinsic energy of the ether does not impart some low degree of temperature to matter, because we know of nothing in nature at the absolute zero of temperature or anywhere near it.) It seems more likely, however, that the ether waves of gravitation are longitudinal, or otherwise consist in an energy flux which, by reason of its universal presence, has not been made manifest except by gravitation.

It is easy to understand how the supposed spherical field of influence, or energy shadow, surrounding any body of matter may be initiated, but just how it is maintained may never be known; though I hope to have something to say in this connection in a future discussion. But that the field of influence actually *is* maintained seems certain; gravitation itself is a demonstration of it.

The simplest mental picture of the supposed field of influence which I can think of is a spherical energy shadow, and I have endeavored to make this conception visible in Figs. 2 and 3 as light

Fig. 2

Fig. 3



shadows cast by black spheres. Of course the shadows as here represented are enormously exaggerated. Plate I., Fig. 2, represents a single sphere and the adjacent parts of its spherical shadow. (Obviously a body of *any* shape will cast a shadow substantially spherical.) Plate I., Fig. 3, shows two neighboring spheres with their shadows interblending. The greater depth of shadow between the spheres is clearly indicated, and it is into this deeper shadow that the two gravitating bodies are supposed to be pushed by the superior energy flux from right and left.

Some curious and interesting secondary phenomena are suggested by this conception of the mechanism of gravitation. One of these may be described as follows: Imagine two bodies, such as those of Fig. 1 or Plate I., Fig. 3, falling toward each other by reason of their mutual attraction. They are continually accelerating, and absorbing energy from the ether waves or energy flux pushing them toward each other, whereby these waves pass through and beyond each body *slightly depleted of their energy*, and thus offer less than normal resistance to the advance of the other body; that is to say, the energy shadow between the bodies, into which they are pushed, grows deeper and deeper as they approach, not only because of their lessening distance from each other, but also because of their increasingly rapid transformation of energy as they gain velocity. And it does not matter if the two attracting bodies differ greatly in mass, like the sun and a planet or the earth and a tennis ball, because they will equally acquire momentum, and each will affect the other in the manner described. Stated concisely this means, if my premises are tenable, that Newton's law of inverse squares is not rigidly true for *accelerating* bodies; but that for positively accelerating (approaching) bodies the force of attraction increases a little faster than the inverse square of distance. The force of attraction instead of varying as $1/D^2$ as it does for bodies at rest or in uniform motion, varies as $1/D^{(2-x)}$ for bodies accelerating in the line of attraction, wherein x is a very small quantity which appears to vary with the rate of energy transformation or velocity of fall. When acceleration is negative, that is to say, when energy transformation is *from* the accelerating body *to* the ether, x becomes positive.

Let us consider the effect of x on a planetary orbit: If the orbit is circular, $x=0$ because there is no change of velocity; but if the orbit is excentric, x obviously grows in value and importance with the excentricity, though always equaling zero at aphelion and perihelion. Fig. 4 illustrates the sun and a planet at aphelion in an ex-

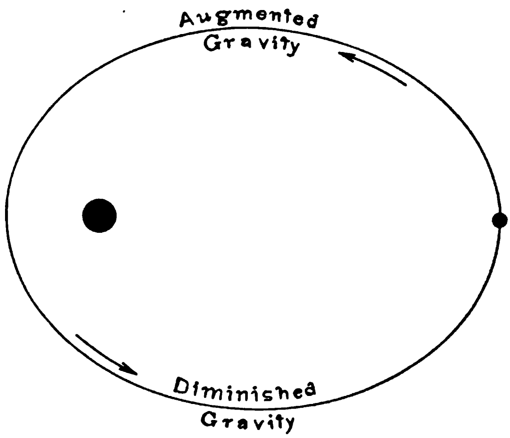


FIG. 4.

aggeratedly excentric orbit. As the planet moves from aphelion to perihelion, normal attraction between the sun and planet is augmented by the positive acceleration of both as before explained; and while the planet moves from perihelion to aphelion normal attraction is diminished by negative acceleration.

If I am not mistaken in my mechanics, the gravitational disturbance above described will slightly change the shape of the orbit, and cause a continual advance in the position of perihelion by advancing the line of apsides. Probably the effect is too small to be detected in the case of any of the planets of the solar system except perhaps Mercury, because of the small excentricity of their orbits; but the high excentricity of Mercury's orbit possibly may reveal it, and I hope it may be found adequate to account for some of the anomalious secular advance of the perihelion of Mercury's orbit. I shall be glad to have my astronomical friends investigate this.

The orbit of the moon is not very excentric, but she moves toward and away from the sun almost the full diameter of her orbit

every month. Perhaps the gravitational disturbance I have suggested may aid in explaining some of her more obscure motions; and I hope it will be found to have a slight accelerating tendency so as to compensate the slight retarding tendency of which I shall treat in the second division of this paper.

2. TRANSMISSION OF GRAVITATION CANNOT BE INSTANTANEOUS.

Laplace at first sought to explain the secular acceleration of the moon's mean motion by ascribing to gravitation a finite velocity of propagation. Later he said:[2] "The time of its transmission, if it were sensible to us, would be particularly evinced in the acceleration of the moon's motion. I suggested this as a means of explaining the acceleration which is observed in this motion; and I have found that in order to satisfy observations we must ascribe to the force of gravity, a velocity seven millions of times greater than that of a ray of light. As the cause of the secular equation of the moon (c) is now well understood, we may affirm that the attraction is transmitted fifty millions of times more rapidly than light. We can therefore assume, without any apprehension of error, that its transmission is instantaneous."

I doubt if anyone who has bestowed careful thought on the subject, in the light of present-day physics, really believes this. To me, it is inconceivable that my change of position, as I walk across a room, is felt among the fixed stars while I am still walking; but the justly great name and fame of Laplace has stamped this dogma with the seal of authority, and for more than a century it has blocked the path of fruitful thought on the physics of gravitation.

Doubtless Laplace made no serious mathematical mistake in reaching his remarkable conclusion, but perhaps he erred in his choice of premises. He postulated[3] a "force" or "gravific fluid," "which rushes towards the sun with an immense rapidity; the resistance which the planet experiences from this current in the direction of the tangent, he conceives to produce a perturbation in the elliptic motion, like to the aberration of light." He then applied this

[2] "System of the World," Harte's translation, Vol. 2, p. 322.

[3] Harte's translation, Vol. 2, notes, p. 490.

conception to the case of the earth and moon. I have endeavored to visualize Laplace's conception in Fig. 5, in which E represents the earth, M the moon moving in the dotted line orbit in the direction indicated by the large arrow, and lines NE the "gravific fluid" rushing from all directions toward the earth. The orbital motion of the moon continually carries her laterally against the stream of "gravific

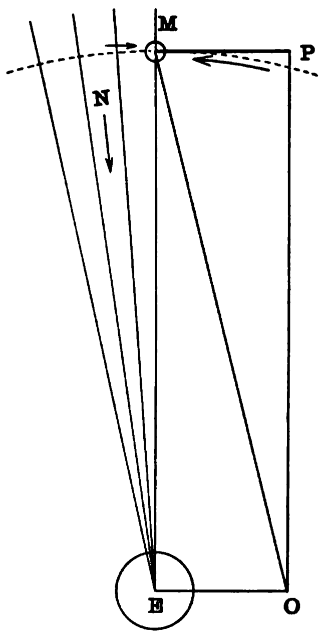


FIG. 5.

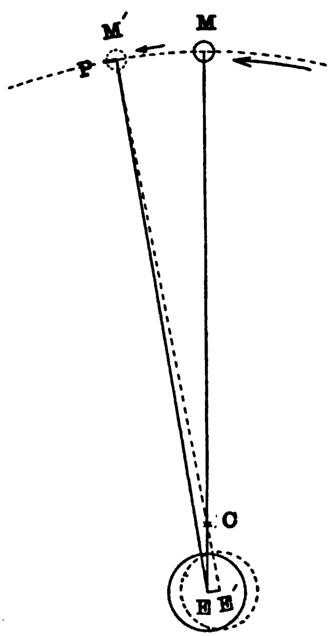


FIG. 6.

fluid," whereby she experiences a tangential retarding force, indicated by the small arrow, just as if a less rapid flow of "gravific fluid" came from that direction. Let the line MP represent the direction and value of the retarding force in terms of the centripetal force ME. Completing the parallelogram of forces, we find the line of the moon's attraction shifted from ME to MO. Clearly this would result in the moon taking an orbit in the form of a contracting spiral which would ultimately bring her to the earth. At the same time her actual velocity would continually increase (and her angular velocity still more so) because of her falling toward the

earth; gravity directly, and the retarding force indirectly conspiring toward this result. With the assumed velocity of light for the "gravific fluid," Laplace found that the angular acceleration of the moon's motion would be millions of times greater than necessary to account for her known acceleration.

I have thus outlined Laplace's conception of the mechanism of gravitation, which led to his famous conclusion of virtually infinite velocity of propagation, because I have not met with anything of the sort in modern text books of astronomy or physics; his startling conclusion is known to everyone, but his premises are generally forgotten; and further because I wish to have it clearly in mind for contrast with what is to follow.

Returning now to the theory of gravitation under discussion: In Fig. 6, E represents the earth and M the moon moving in the dotted line orbit in the direction indicated by the large arrow, both revolving about their common center of gravity C. Instead of showing C well inside the earth's circumference where it belongs, I have shown it outside, so as to enlarge certain details of the diagram and thus avoid confusion of lines.

The earth and moon are each supposed to cast a spherical energy shadow, or occupy the center of a spherical field of influence, as already indicated, into which the other is pushed by the slightly superior energy-flux coming from beyond it. The strongest push of each is toward the densest part or *origin* of the shadow of the other. Thus, if earth and moon were stationary, each would be pushed toward the center of mass of the other. But while the shadow, emanating so to speak, from the earth at any instant is being propagated outward to the moon's orbit, the latter will have moved to M', and the earth will have moved to E'. Clearly then, the moon at M' will not be pushed toward E', but toward E, which is the origin of the shadow into which it is being pushed. The centripetal force $M'E$ may be resolved into the radial component $M'E'$ and the tangential component $M'P$ equal to the displacement of the earth's center from E to E'. Clearly, the force $M'P$ is an *accelerating* force, and bears the same ratio to gravity at M' that $M'P$ bears to $M'E'$.

If the velocity of propagation of the energy shadow equals the

velocity of light, then the displacement of the earth's center from *E* to *E'* will be about 52 feet, and the tangential force *M'P* will be about one twenty-four-millionth of gravity at the distance of the moon.

Obviously, this very small tangential force will tend to make the moon's orbit an expanding spiral of very small pitch; but the vastly greater force of gravity will resist this tendency and nearly, but not quite, counteract it; the net effect being an extremely slow lengthening of the radius vector, and a very slight ***retardation*** of real as well as angular velocity. This paradoxical effect, of an accelerating force producing an orbital retardation, is explained by Sir George H. Darwin in his chapter on tidal friction and the genesis of the moon.[4]

I have made only a very rough estimate of the secular retardation of the moon's mean motion which this minute accelerating force will bring about, with gravitational transmission taken equal to the velocity of light, but have satisfied myself that it will amount to a very few seconds of arc only, in a century; and I do not claim that the velocity of light is the velocity of gravitational propagation unless the postulated ether waves are ultimately found to be transverse like those of radiation. I think it probable that they are longitudinal, or otherwise different from those of radiation. If this be true, the velocity of propagation may be several times greater than that of light, and the secular retardation of the moon correspondingly less.

I realize that any uncompensated retardation of the moon's motion will add to the present outstanding observed acceleration, if any; but am hopeful that the slight departure from Newton's law of inverse squares already suggested may, in connection with other motions of the moon, supply some of the necessary compensation. There is also a minute source of compensation, due to motion through the ether, which I intend to consider in another discussion.

CLEVELAND,
April, 1914.

[4] "The Tides," Chap. XVI.

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THE AFFINITIES AND DISTRIBUTION OF THE LOWER EOCENE FLORA OF SOUTHEASTERN NORTH AMERICA.

BY EDWARD WILBER BERRY.

(*Read April 25, 1914.*)

INTRODUCTION.

Three years ago I made a preliminary announcement before this Society[1] concerning the fossil floras of southeastern North America. I have, in the interim, completed a monograph of the extensive and especially well preserved plants of the Lower Eocene, and it is some of the results of this detailed study that are given in the present communication. This work has been done under the auspices of the United States Geological Survey, to the director of which organization I am indebted for permission to publish the following preliminary abstract. I also wish to express my great indebtedness to Dr. T. Wayland Vaughan, who has had general charge of the Coastal Plain investigations and to whom great credit is due for their comprehensive character.

PHYSICAL CONDITIONS INDICATED BY THE FLORA.

There is no part of North America so favorably situated for the study of the floras which preceded the present, extending backward

[1] *Proc. Amer. Philos. Soc.*, Vol. 50, No. 199, 1911.

to a time which marks the first recorded appearance of angiosperms, as that of the South Atlantic and Gulf states. No single part of North America contains so continuous a series of Tertiary deposits carrying fossil plants. In this area are found abundant floras in the lower and middle stages of the Eocene, a small flora in the Upper Eocene, considerable floras in the Oligocene, some in the later Miocene, and rather abundant fossil plants in the Pliocene, as well as numerous Pleistocene deposits carrying fossil plants. The Rocky Mountain region is rich in Eocene fossil plants and there are some Miocene floras, but practically no Oligocene or Pliocene floras are known. The Pacific coast region likewise furnishes Eocene and Miocene fossil plants but none of Oligocene age.

The fossil floras of the Coastal Plain are found in an area where it is possible to attain to some measure of accuracy in predicating the general character and course of ocean currents and winds and other physical features of the environment. On the other hand the western floras just mentioned grew in areas where vulcanism was great at times; in areas of great orogenic activity, where changes in topography were numerous and elevations of several thousands of feet are recorded; areas in which climatic conditions not only varied from place to place, but passed through a large cycle of secular changes. All these factors greatly complicate the floral history.

The floras of the southern Coastal Plain are moreover checked for the most part by very abundant marine fauna in intercalated beds, or the plant-bearing beds which represent the coastal swamps and the shallow water deposition of the old embayment merge laterally with the contemporaneous limestones or marls which were forming in more open waters along the coasts to the southward, so that there is a considerable body of facts bearing on depth, character of the bottom, and marine temperatures, with which to compare land temperatures. These criteria have been admirably worked out for the Florida area by Doctors Dall and Vaughan for the post-Eocene and their results furnished a reliable datum plan for the deductions to be derived from the study of the fossil floras of those times.

With the exception of fragments of the petrified stems of conifers, palms and dicotyledons the plant-remains are in the form of impressions, mostly of foliage, but with a goodly representation

of fruits and seeds, and in some few cases even flowers are preserved.

While the oscillations of the embayment area have been numerous they have been, as I have just mentioned, inconsiderable in amount, only a few hundred feet at most, and the coastal region has uniformly been one of slight relief. The various floras show an almost complete absence of upland types. This is in striking contrast to the European older Tertiary floras. The only large area of the globe which has been thoroughly studied—Europe—was far less stable than this region in Tertiary times and lying much farther toward the pole was subsequently subjected to the rigors of Pleistocene conditions whose influence never reached our southern states.

The paleobotanical record of the Atlantic and Gulf Coastal Plain furnishes a history which extends back as I have just mentioned beyond the oldest known angiosperms to a time (Lower Cretaceous) when the flora was made up almost entirely of tree-ferns, conifers and those interesting cycadophytes (Cycadeoidea) whose trunks are sometimes preserved with such marvelous perfection that the outlines of the embryos in the ovules can often be made out in detail. Coming a step nearer my present theme, a step of some millions of years from the Lower into the Upper Cretaceous, we find the first great modernization of the floras of the world due to the seemingly sudden evolution of the main types of angiosperms. These Upper Cretaceous floras are well represented in the Coastal Plain from Marthas Vineyard to Texas. They extend northward to Greenland and southward to Argentina in South America, and are found to indicate very different physical conditions from those which prevail at the present time. I do not intend, however, to dwell upon the Upper Cretaceous floras in this connection but pass to a consideration of the succeeding Eocene epoch of plant evolution.

The Eocene as defined by Lyell was marked by the dawn of the recent species of marine mollusca. It is equally well marked by the sudden expansion and evolution of modern types of plants after a long antecedent Cretaceous development. The floras become thoroughly modernized as compared with those which preceded them, although they are still very different in their general facies and distribution from those of the present.

In the earliest epoch of the Eocene known as the Midway, the relations of sea and land in the Gulf area differed in only minor particulars from that of the late Cretaceous. The waters of the Mississippi Gulf were, however, deeper. This factor combined with a much less influx of fresh water from the tributary streams, due in some measure to the low relief of the land, enabled marine faunas to reach well toward the head of the gulf. These faunas indicate subtropical bottom temperatures northward as far as Paducah, Ky. The

FIG. 1. Sketch map showing the approximate position of the shore line, A–A, at the beginning of the Wilcox transgression and B–B the area covered by the Wilcox sea during its maximum transgression. C–C, the extreme northern limit of the Willcox flora under existing climatic conditions.

known floras are very scanty and unsatisfactory and in the present state of our knowledge do not merit an extended discussion. The maximum transgression of the sea during the Midway epoch is shown on the accompanying sketch map (Fig. 1).

The Midway Eocene was succeeded by a long interval during which the sea is believed to have withdrawn southward at least as far as the position indicated on the accompanying sketch map (Fig. 1, *A–A*), since terrestrial conditions are known at the extreme base

of the Wilcox in the most southerly areas of their outcrop. This interval of emergence of the embayment area was followed by an equally long interval during which a great thickness of deposits was laid down that are collectively known as the Wilcox group. The character of these sediments and their faunas show that the Mississippi gulf was somewhat restricted and much shallower than in the preceding epoch, with true marine conditions prevalent only in its lower portion. The shores were low and relatively flat. They were flanked by current- or wave-built bars and separated from the mainland by shallow inlets or lagoons. The lower courses of the streams were transformed into shallow estuaries or broad swamps through which the smaller streams meandered.

The maximum area covered or underlain by Wilcox deposits is also indicated on the accompanying sketch map (Fig. 1, *B–B*) which shows approximately the shore line along which the vegetation migrated. As has been already remarked the Wilcox deposits have yielded one of the most extensive of known fossil floras, an assemblage of extinct species which sheds considerable light on the physical conditions of the marginal lands of Wilcox time.

Before taking up in detail the evidence of the flora I wish to point out certain general climatic conditions based on cosmic causes and deduced for the Wilcox from studies of recent climates.

It is to be noted that the factors governing atmospheric circulation are general and not local and the relatively slight changes in the relation of land to sea in Wilcox time as compared with the present are entirely too small to have caused much modification of existing conditions. Then as now there was a persistent area of high pressure over the North Atlantic and a low over the continent. Consequently the winds were prevailingly from the east. Cyclonic disturbances like those that originate today in the Gulf of Mexico or those more violent and widespread storms of the West Indian hurricane type which today originate in the Caribbean Sea would traverse at least a part of the Mississippi embayment. So large an area of shallow more or less landlocked water would have a very appreciable effect in raising total temperatures and in the prevention of widely separated extremes. At the same time it would increase the rainfall and increase the width of the marginal lands over which this augmented

rainfall would be effective. Whether or not this would be sufficient to furnish the subtropical conditions that the flora seems to indicate is doubtful. Speculation regarding the Eocene climate of the world as a whole is perhaps out of place, nevertheless it remains true that the sum total of paleontologic evidence indicates that the familiar succession of seasons or of types of vegetation in passing from the luxuriant tropics to the ice-capped poles did not hold good for the Eocene. Paleobotanists have long maintained that the existing climate is essentially a Pleistocene climate of an interglacial character and that for the great bulk of geologic time uniformity and not differentiation has been the rule rather than the exception. While the older paleobotanists were inclined to overestimate the conditions of torridity, it remains true that from the Lower Cretaceous until toward the close of the Oligocene, not to mention still older floras of more remote botanical affinities, whenever fossil floras are found, from beneath the Equator to within the Arctic circle, they show a degree of uniformity that proves that former climates were secularly unlike these of today and as is obvious this floral evidence would be equally convincing if all the vast number of fossil plants were simply called *Phyllites* as in Schlotheim's day and no attempt were made to determine their botanical affinity.

The student of fossil floras is naturally more sanguine and enthusiastic in predicting former physical conditions than perhaps is warranted by his facts. When however a common Upper Cretaceous flora can be traced from Texas to Greenland or when we find in the Eocene such unmistakable forms as *Artocarpus* leaves, *Engelhardtia* fruits, and nuts of the Nipa palm associated with forms as characteristic as ferns of the genus *Acrostichum* all extending almost across the temperate zone in both the eastern and western hemispheres it would seem that the burden of proof that climates were not very different from those of today rests with the physicist and not with the paleobotanist.

It may be noted that all of the Wilcox plants, almost without exception, are plants whose modern representatives inhabit the warmer parts of the earth. There is not a single strictly temperate type in the whole assemblage, the nearest approach to such types being in the genera *Juglans, Myrica, Magnolia, Cercis, Ilex, Nyssa,* and *Frax-*

inus, and in all of these genera or closely related ones there are existing tropical forms. None extend beyond the warmer parts of the temperate zone and some of these as in the case of *Juglans* and *Fraxinus* indicate in their compound leaves their tropical ancestry, as was first pointed out by Grisebach. The ferns are all tropical types and their relative unimportance in the Wilcox flora furthermore indicates that the major part of this flora is a strand flora. This is shown more especially by forms like the Nipa palm which never grows outside of tidal marshes, by *Conocarpus, Laguncularia* and *Avicennia* which inhabit like situations; by coastal marsh or lagoon plants like *Canna, Trapa* and *Sabalites;* and by the large number of strand types that inhabit beaches or the jungle behind the beach ridges or dunes. The more striking of these genera are *Myrica, Artocarpus, Ficus, Coccolobis, Pisonia, Anona, Capparis, Chrysobalanus,* various Lauraceæ, Apocynaceæ, Sapotaceæ and Leguminosæ, *Fagara, Drypetes, Metopium, Ilex, Celastrus, Sapindus, Dodonæa, Reynosia, Rhamnus, Myrcia, Eugenia, Laguncularia, Combretum, Terminalia, Cordia, Citharexylon, Exostema* and *Guettarda.*

It needs but a slight acquaintance with the existing Antillean flora or that of the Florida keys, or in lieu of actual acquaintance a perusal of the as yet too few ecological discussions of the flora of the American tropics or even of Schimper's classic Indo- Malayan strand flora to see at once that the general facies of the Wilcox flora is overwhelmingly that of a strand flora of which some of the elements indicate that they grew on the sandy beaches, others in muddy tidal flats, others between or behind dunes or beach ridges, and others in estuary bayous or marshes. None of the forms can certainly be considered as inland or upland types. Even genera like *Banksia* which is not usually considered a coastal type in the existing flora furnishes *Banksia marginata* Cav. to the coastal sand dunes of South Australia (Tepper) and several others species of the genus occur on the dunes of Queensland, Victoria and western Australia.

Little has been written of the plant associations of the American tropics and collectors notes almost always fail to adequately describe habitats. While the marginal Wilcox lands were low there was such a large area of continent to the northward to draw from, and the long coast furnishes such varied edaphic conditions, that the flora

was far richer than floras of small insular areas of the American tropics of the present, as for example, that of the Bahamas which are relatively close to mainland, where in addition to difficulties of introduction there is relatively great uniformity of edaphic factors and directly adverse factors such as winds, which limit the floral display.

Without pursuing the subject in greater detail it may be assumed to be proven that the Wilcox flora is a typical coastal flora. Compared with recent coastal floras it is at once apparent that its affinities are entirely with those of tropical and subtropical America. It has much in common with the Bahaman flora and that of the Florida keys, but is far richer in arborescent forms. Comparisons with the larger islands of the West Indies show more elements in common, such differences as are apparent being due to the prevalence of porous coral rock along these recent shores while the Wilcox shores were not of this character. The most complete agreement is furnished by the floras along the Caribbean coast from Central America to northern Brazil. A considerable number of genera found in the Wilcox flora do not range through the West Indies at the present time and the explanation seems to be that the Wilcox flora more closely resembles the original flora of the whole American equatorial region which became restricted during the epeirogenetic and climatic changes of the Miocene or Pleistocene and the elements now lacking in the West Indies never regained all of the area of distribution lost at that time.

It may seem improper to say that a flora with abundant forms of *Artocarpus, Nipa, Cinnamomum, Banksia,* etc., is entirely American in character but if the brief sketches in the botanical discussion which follows are read it will be obvious that these genera, oriental in the existing flora, were cosmopolitan in the early Tertiary, so that it would be entirely misleading to draw conclusions from existing distribution alone.

The Wilcox waters of the upper embayment were always shallow; there were fringing bars and lagoons as well as deltas, estuaries and swampy bayous. The deposits in places show river action and streams shifting about over sand flats. Regarding actual temperatures so little is known after all of the relations of modern plants to their climatic environment that results can only be qualitative and not quantitative.

It is obvious that the flora could not have existed if the region was ever visited by frost, and temperatures appear to have been like those found today on the Florida keys. Aside from the meteorological certainty that there was a wide coastal belt of abundant precipitation, there is the confirmation furnished by the flora itself. It would seem to me proper to compare the Wilcox flora with those of the regions to which the somewhat loosely used term subtropical rain forest is applied by plant geographers. Too little is known of the Midway flora for accurate comparisons. Compared with the Upper Cretaceous flora of the embayment area, in which however 40 percent of the genera are extinct, the Wilcox would seem to have become more tropical, a progression from what might be termed a warm temperate to a subtropical rain forest. On the other hand the floras as well as the faunas show a gradual increase of tropical conditions in the later Eocene which culminate in the Oligocene, the flora of which in southeastern North America is strictly tropical.

Lianas were apparently not as common in the Wilcox as they are in the existing floras with which it has been compared. No traces of the Bignoniaceæ, so common in the American tropics, have been detected, the scandent types being represented by *Lygodium, Aristolochia,* Malpighiaceæ, *Canavalia, Pisonia* (?), and *Zizyphus,* I am inclined to think that the great uniformity of climatic conditions together with the abundant rainfall have combined to make the Wilcox flora seem more tropical in character than was actually the case. That reef corals are not found in the Wilcox is, I believe, entirely due to physical conditions other than those of temperature as Vaughan[2] has shown to be the case so often in such a striking manner in recent seas.

I have indicated upon the sketch map (Fig. 1 *C–C*) what I conceive would be the northern limit of range of the Wilcox flora under existing climatic conditions in southeastern North America.

It would seem to be probable that most of the generic types of the Wilcox were differentiated by the close of the Cretaceous. If the equatorial region of America was the place of origin of a majority of those types which have not as yet been recorded from the Cretaceous as I believe to be the case, they must have spread northward along

[2] Vaughan, T. W., *Journ. Wash. Acad. Sci.,* Vol. 4, pp. 26–34, 1914.

the Mississippi Gulf either during the Cretaceous-Eocene interval, during the Midway or during the Midway-Wilcox interval. While the time available for this northward dispersal was thus sufficiently long to account for the migration of even the most slowly spreading forms a short statement on the adaptations and agencies of this dispersal is not without interest. The Wilcox genera with winged fruits or seeds are *Engelhardtia, Paraengelhardtia, Dodonæa, Paliurus, Fraxinus* and the Proteaceæ and Malpighiaceæ. None of these are capable of long flights except those of the last two families and these during high winds might readily be carried for miles along coasts, although it is doubtful if they could have crossed great stretches of open water, even through the agency of a West Indian hurricane. The heavier winged fruits such as those of *Engelhardtia, Paraengelhardtia, Paliurus, Dodonæa* and *Fraxinus* float readily, although as far as I know there is no experimental data to show how long they float in oceanic waters without losing their vitality. Certainly *Dodonæa* has reached the Bermudas in recent times through the agency of the Gulf Stream. Among the Wilcox forms more or less adapted for floating the following genera may be enumerated: *Nipadites, Canna, Taxodium, Pisonia, Sapindus, Sterculiocarpus, Trapa, Avicennia, Solanites, Exostema* and the Combretaceæ. Among the foregoing *Canna, Taxodium, Trapa* and *Exostema* are scarcely adapted for sea voyages while on the other hand, *Nipadites, Sapindus, Sterculiocarpus, Avicennia* and the Combretaceæ are singularly adapted for dispersal by ocean currents and would be in the van of forms colonizing the shores of the transgressing Wilcox sea.

A large number of the Wilcox genera had fleshy or drupaceous fruits and formed the dietary of both mammals and birds. Among these the following may be mentioned: *Myrica, Ficus, Coccolobis, Magnolia, Anona, Asimina, Chrysobalanus, Simaruba,* all the Lauraceæ, Ilicaceæ, Celastraceæ, Myrtaceæ, Ebenaceæ, Sapotaceæ, Meliaceæ, Euphorbiaceæ, Anacardiaceæ, *Zizyphus, Guettarda, Citharexylon, Cordia, Osmanthus, Icacorea, Rhamnus* and *Reynosia.* Many of these have crustaceous stones that pass uninjured through birds or are voided with their vitality unimpaired and these could undoubtedly be carried long distances over seas. Even in the case of soft seeds like those of a large number of the Leguminosæ it has been

found that birds that have eaten greedily often void them uninjured and others meet with fatalities before the seeds are digested and these constitute by no means unimportant factors in distribution. Clement Reid in his discussion of the origin of the British flora gives an instance of a dead wood-pigeon with beans sprouting from its crop, and when it is remembered what a great percentage of birds meet an untimely end it is conceivable that a single hurricane might readily be the means of introducing new forms from the Antilles upon the Wilcox coast. Other Leguminosæ, although more rarely, are dispersed by ocean currents, as is the case in an eminent degree with the modern *Entada* or snuffbox seabean.

All of the storms moved from the equator northward, the main ocean currents had the same general direction, while the prevailing winds were easterly so that all of these important factors combined in causing a relatively rapid introduction and spreading of forms along the Wilcox coasts, so that given favorable climatic conditions and many of the forms need not have taken the time to spread from Central or South America along continuous coasts.

Botanical Character of the Flora.

That the method by which the bulk of the determinations in the present study are made rests upon real and not fanciful affinities is of vital importance, since the resulting climatic and other physical data are largely controlled by these facts. The case is not as intricate or as hopeless as it might seem to the student who remembers the thousand of living and extinct genera. De Candolle estimated the total number of flowering plants to be about 250,000 species. This figure is swollen by the great multiplication of herbaceous species in recent geologic times. The ratio of arborescent to herbaceous types was much greater in the Tertiary than it is at the present time and it seems probable that trees were actually more abundant and varied than in the existing flora. This was certainly true for all Tertiary floras outside the torrid zone and may be readily proved by a consideration of the Eocene floras of North America, the Miocene floras of Europe or to cite an extreme case the Tertiary floras of the Arctic and Antarctic regions.

While the arborescent flora of the temperate zone is relatively

meager, trees increase in passing toward the equator. For example the state of Maryland which is in latitude 38° to 39° 30′ is in effect a cross section of the Coastal Plain, Piedmont Plateau and the Alleghany Mountains with great differences of climate, topography and soils. It is moreover the meeting ground for plants of northern and southern range. In spite of these facts there are only about 150 arborescent forms in its flora. On the other hand in Small's recently published Trees of Florida (1913) there are 366 native and naturalized arborescent forms, and if Florida furnished much altitudinal variation the number would be much larger. For example the arborescent flora of the Philippine Islands includes 665 native species and many additional introduced forms. Even remote oceanic islands if of sufficient size and topographic variety to overcome the adverse action of winds have a large arborescent flora. Thus the Sandwich Islands have 225 native species of trees distributed among 45 families, the larger being the Rutaceæ (32 species), Rubiaceæ (31 species), Campanulaceæ (15 species), Araliaceæ (14 species), Pittosporaceæ (12 species), Palmaceæ (11 species), Myrsinaceæ (11 species), and Malvaceæ (10 species).[3]

The general physical conditions of a remote geological epoch may be more or less completely deduced in advance from the character of the sediments: the run-off from the land can be approximated and consequently the altitude of the land and the probable rainfall as well as any periodicity in these factors. These are all reflected in the sediments. Work like that of Vaughan[4] on the deposits of the Florida Keys or that of Drew[5] on the part played by denitrifying bacteria in the formation of limestones, enables a careful paleobotanist to in a measure predict the character of the flora that clothed the marginal lands. In deposits that teem with the remains of marine life as do many of the Tertiary formations of southeastern North America it is possible to arrive at very close approximations of the actual temperatures of the coastal waters. It may be safely assumed that boreal or temperate floras did not flourish in proximity to trop-

[3] Rock, J. F., "The Indigenous Trees of the Hawaiian Islands," Honolulu, 1913.

[4] Vaughan, T. W., Carnegie Institution, Publication 133, 1910.

[5] Drew, G. H., Yearbook, Carnegie Institution, No. 10, 1911.

ical marine faunas: that plants reflected their environment in the past as in the present.

A considerable number of botanists love to dwell upon the temerity of their paleobotanical friends in venturing to determine leaf impressions. I admit at the outset that some determinations are much too sanguine, especially when based upon fragmentary materials. There is more or less convergence in foliar characters in unrelated or remotely related families and there is also considerable variation in the leaves of a single species, but the fact remains that foliar characters in general are more conservative than those derived from almost any other organs of plants. They are subjected to less complex environmental factors and always have been. It should be remembered that characters less essential in the vital activities of plants, such as leaf form, when once acquired, may continue practically unchanged for thousands of years and afford a surer clue to relationship than characters more immediately within the field of action of natural selection. This is shown by the persistence of fern fronds on the Paleozoic Pteridosperms; by the uniformity of Cycad-like fronds from the Permian to the Cretaceous; and by the striking persistence of dicotyledonous foliar types from the Mid-Cretaceous to the present. It is paralleled by Dall's observations on the persistence of superficial and ornamental shell characters in the Mollusca from the Cretaceous to the Recent.

The opinion I mention in a preceding paragraph is mainly the result of ignorance both of foliar characteristics and paleobotanical literature, and an unwillingness to spend the time necessary for a mastery of the subject. I have tested systematic botanists time and again with recent leaves and the results are not especially creditable. They generally know that leaves are green in color and that some are simple and others compound: they may even know whether the margins are entire or toothed, but the venation is usually a closed book. I know of but one manual that pretends to pay careful attention to foliar characters and that is Sargent's "Tree Book" and even here the figures pay no attention to venation.[6] In the tropics where flowers and fruits are often unobtainable or beyond reach it is easy to

[6] Sudworth's "Trees of the Pacific Slope" is the most admirable work in this respect that has ever been published.

learn to recognize most arborescent forms by their habit and foliage but to most botanists, systematic or otherwise, anything beyond the floral structure receives but scant attention.

It is reasonable to conclude that palms and tree-ferns were never boreal plants that have in the course of ages become restricted to the tropics as Naumayr once suggested in an effort to explain their presence within the Arctic circle on other than climatic grounds. Uniformity of conditions is the foundation upon which the whole fabric of our knowledge of past events rests and it is just as unscientific to assume that the carrying power of water was not conditioned by its velocity during the Tertiary as it is to assume that insolation, humidity, rainfall, winds and all the other factors that constituted the environment of the vegetation, had effects different in kind from their effects on the living flora.

In a study of this sort the chief emphasis should be based upon comparisons with the existing relatives of the fossil forms and not upon a search among the illustrations of works devoted to the study of previously described forms, often from remote regions, for what appears similar. The latter should not be neglected however and no descriptions are complete unless they include a discussion of the resemblances and differences of previously described forms that show similarities to the form in hand with their geologic and geographic destribution. Even the most trivial characters of the fossil should be carefully noted since all are or will become valuable in future studies. The living representatives, their habitat, range and variation are of the greatest importance in determining paleoecology.

Unless there is clear evidence of transportation it may be assumed that strand plants and upland plants will not be found in association and if such seems to be the case, additional study may reveal the errors of determination.

That all floras are dynamic and not static: that all their elements are more or less plastic in their reactions to the infinite complexity of their environment raises a certain amount of scepticism regarding the methods and results of what may be called paleoecology. This is especially true since so little is known regarding the precise relations between existing plants and their environment. At the same time there is no other method available and it must be considered to be a

legitimate method until negatived in human experience. If it be assumed untrue there is no limit to idle speculations as futile as those of medieval times.

The Wilcox flora as described in the present study comprises considerably more than 300 species—the exact number is without significance since it is so largely dependant on accidents of preservation and discovery, and since it is also considerably influenced by the evaluation of specific characters. The number might readily be increased to 400 if fragments of new forms were considered the basis for the description of species.

This flora is therefore one of the largest floras as yet known from a single geologic horizon in a single area, although it is considerably overshadowed numerically by the so-called Fort Union flora of the Rocky Mountain Province, which however covers a greater geographic area and a longer interval of time.

Compared with foreign Eocene floras of similar age it may be noted that Ettingshausen enumerated 72 genera and 200 species from the London Clay of the Island of Sheppey[7] and 116 genera and 274 species from Alum Bay on the Isle of Wight.[8] I mention these two English floras specifically since while never adequately described they are at least partly contemporaneous with that of the Wilcox, as I hope to show in the chapter on correlation, and they therefore offer various interesting details for comparison as will appear on subsequent pages.

The Wilcox flora comprises 128 genera in 59 families and 33 orders. The Thallophyta are represented by a few species of leaf-spot fungi, and if the student were to follow the fashion set by the older continental paleobotanists the so-called species of spot-fungi could be increased many fold, as I have only picked out for enumeration certain conspicuous or characteristic types. The Bryophyta, as is usually the case in fossil floras, are entirely unrepresented, although the sediments are often of a character to have preserved them in perfection if they had been present, and the assumption is logical that they were either confined to more northern latitudes at this time or were an exceedingly minor element in the flora. The Pterido-

[7] Ettingshausen, *Proc. Roy. Soc. Lond.*, Vol. 29, 1879, pp. 388–396.

[8] Ettingshausen, *Ibidem*, Vol. 30, 1880, pp. 228–236.

phyta which are such a preponderating element in all fossil floras up to the Middle Cretaceous are represented by a doubtfully determined Lycopod and six species of ferns.

Ferns are among the most abundant (in specific differentiation) vascular plants in the flora of tropical America, the island of Jamaica being especially celebrated for its fern flora. Grisebach enumerated 340 species of ferns in his "Flora of the British West Indies" published in 1864. In Urban's more recent work 182 species of the Polypodiaceæ alone are recorded from Porto Rico. The following five genera have been recognized in the Wilcox: *Aneimia, Lygodium, Asplenium, Pteris* and *Meniphylloides*—each represented by a single species except the genus *Asplenium* which has two species. While six species seems a small number of ferns in a subtropical flora like that of the Wilcox it is just twice as many as have been found in the contemporaneous deposits of Alum Bay on the Isle of Wight where the remains of an extensive flora is preserved in the pipe-clays. The explanation of this seeming disparity between the fern representation in the Lower Eocene and in modern floras is readily formulated and it will also indicate the reasons for thinking that the real Wilcox fern flora if it were available for study would be a rich and varied one, comparable at least with the existing fern flora of the lowlands of subtropical America.

The known Wilcox flora is almost entirely a coastal flora made up very largely of strand types. Very few elements in it can be legitimately considered as derived from inland areas by stream transportation, in fact their condition of preservation alone proves that they grew in the immediate vicinity of where they are now found as fossils. With a few striking exceptions the existing tropical and subtropical fern floras are floras of humid inland or upland habitats, for example the majority of the Jamaican ferns are found on the Blue Mountains. The most striking exception to this statement is the genus *Acrostichum* which strangely enough has not yet been positively recognized in the Wilcox flora although it was widespread along the shores of the Mississippi Gulf in the succeeding Middle Eocene (Claiborne) and Lower Oligocene (Vicksburg) floras, as abundant apparently as it is in the existing flora of tropical tidal marshes in both the Eastern and the Western Hemispheres. Another

fern type liable to be present in coastal thickets is the genus *Lygodium* with its scandent habit, and this genus is represented in the Wilcox flora by both sterile and fertile fronds. It is likewise common in the Claiborne and Vicksburg floras and in Tertiary floras generally. Besides *Lygodium,* the family Schizaeaceæ is represented by a species of *Aneimia* which must also be considered to have been a coastal type in the early Eocene as are some of its species at the present time, since very similar species of *Aneimia* are found at a very large number of Eocene coastal deposits both in this country and abroad.

The remaining four species of Wilcox ferns are all referable to the family Polypodiaceæ which is the dominant existing family of the fern phylum. The two species of *Asplenium* are types readily matched by existing Central American species. The *Pteris,* not certainly identified as a true species of this common cosmopolitan type, had stout coriaceous fronds and may have been transported since it occurs at only two localities in the Wilcox and at one of them it is in a fragmentary condition. This supposition receives some support from its presence in the basal Eocene of the Rocky Mountain province after the sea had withdrawn from that area and after there had been a large amount of volcanic activity and more or less uplift. The genus *Meniphylloides* is a unique type as yet peculiar to the Wilcox flora although it is closely related to the similarly unique genus *Meniphyllum* Ettingshausen and Gardner from the Middle Eocene (Lutetian) of England and both are closely related to and possibly the progenitors of the existing genus *Meniscium* which has at least one species that is close to the Wilcox form. *Meniphylloides* is only found at two localities near the top of the Wilcox and its probable habitat is not known. The remains are broken but are associated with a typical strand flora.

It will be seen that of the Wilcox ferns whose habitats can be surmised all are coastal types and when we recall that the mainland was relatively low throughout Wilcox time it is not surprising that the ferns are not more strongly represented. By a specialization of habitat in modern equatorial regions a considerable proportion of the flora becomes epiphytic, the smaller ferns being commonly so. None of the members of the extensive Wilcox flora can be regarded

as epiphytes with the possible exception of *Lycopodites? eolignitica* which is such a rare and poorly represented form that it is without significance. Apparently epiphytes were not conspicuous in the Wilcox coastal floras so that this possible source of supply for additional fern species is also eliminated.

The Gymnospermæ so conspicuous in Mesozoic floras are relatively unimportant in the Wilcox flora, a feature due to their general unimportance in Cenozoic floras and to their intolerance of the habitats and climatic conditions indicated by the *tout ensemble* of the Wilcox flora. All of the five Wilcox gymnosperms are referred to the relatively modern family Pinaceæ and none of the genera are especially close to Mesozoic types. The Cycadaceæ which might be expected to be represented by *Zamia*-like forms have not been found although the presence of typical *Williamsonia* fructifications in the Upper Cretaceous of the coastal plain indicates that the Cycad phylum had not been long extinct in this area.

The Angiospermæ, beyond all odds the dominant type in existing floras, was as clearly dominant in Wilcox time since to it belong over 94 per cent. of the known Wilcox flora. Of these numerous angiosperms only seven are referable to the Monocotyledonæ. It is true the number of monocotyledons might have been increased by describing the various sedge or grass-like fragments that are not uncommon at certain localities. None of these have, however, been dignified by names except a single form each of *Poacites* and *Cyperites* which were only retained since they were already in the literature. That only three species of palms have been recognized is remarkable since palms were well differentiated at this time and various genera such as *Phœnicites, Thrinax, Geonoma, Bactrites, Manicaria,* etc., are recognized in our later Tertiaries. In the contemporaneous deposits of Sheppey of the 30 monocotyledons enumerated by Ettingshausen (op. cit., p. 393) 22 species are palms. On the other hand the Alum Bay flora contemporaneous and not far distant from the Sheppey deposits furnished only 6 monocotyledons. This contrast indicates that the fruits accumulated at Sheppey in the delta of an Eocene river system contain interior forms not present in the coastal region represented by the Alum Bay clays and that inland from the Wilcox coast the display of monocotyledons suitable to the Wilcox environmental conditions flourished but failed of preservation.

Since the early Eocene floras of Europe are so much like those of southeastern North America an enumeration of the Sheppey palms is of considerable interest. They include the genera *Nipa, Œnocarpus, Areca, Iriartea, Livistonia, Sabal, Chamærops, Thrinax, Bactris, Asterocaryum,* and *Elæis.* Of these *Nipa* and *Sabal* are represented in the Wilcox flora while *Thrinax* and *Bactrites* are present in the embayment area in the Middle Eocene (Claiborne). The Order Palmales, or more properly Arecales, has a single existing family the Arecareæ (Palmæ) with about 150 genera and considerably over a thousand existing species about equally divided between the oriental and occidental tropics. There are no temperate outliers, although some species extend for considerable distances into the temperate zone as for example *Sabal adansonii* which ranges northward along the Atlantic Coast as far as North Carolina. The present distribution of the palms is a good illustration of modern continental floral diversities succeeding a Tertiary cosmopolitanism of floras and it shows further the part played by isolation in evolution, also indicated by the abundance of monotypic genera in the Orient where the tropical area is so much broken. Not a single species or genus is common to the two hemispheres and even the tribes are almost all either oriental or occidental.

Regarding the origin of the palms most students regard the Pandanaceæ (screw pines) as their probable ancestral stock and while the latter family is entirely oriental at the present time this was not true in the Tertiary, and it is perhaps significant that the existing genus *Phytelephas* which is regarded as intermediate between the Pandanaceæ and the Arecaceæ is exclusively American, and that genera now exclusively oriental like *Nipa* and *Phœnix* are represented in the American Tertiary (*Nipa* in the Wilcox and *Phœnix* in the Vicksburg). There is no warrant for asserting that palms are of occidental origin, at the same time their oriental origin is equally difficult of proof and what we know of their geologic history conclusively shows the inadequacy of the existing distribution in a discussion of their phylogeny.

The three Wilcox species of palms comprise a fan palm and two feather palms. The *Chamædorea* leaves represent a small palm whose numerous modern allies are confined to America, being richest

in species in the humid mountainous regions of Central America. It is not a coastal form and is not found in association with the typical Wilcox strand flora, occurring only in the basal Wilcox of Choctaw County, Mississippi, and at the base of the transgressing Upper Wilcox deposits in Saline County, Arkansas. Its rarity and occurrence in basal beds would seem to indicate that its area of growth was inland and only reached in these two cases by the landward migration of the strand line. The *Sabalites*, which I have compared with the existing *Sabal palmetto*, is common everywhere from the base to the top of the Wilcox. It is distinctly a coastal type, rather of the lagoons, bayous and estuaries than of the strand. This is indicated by the fragmentary nature of the remains at very many localities and the occurrence of innumerable complete specimens at other localities as for example at Oxford, Mississippi, where the presence of unios and the local unconformities indicate estuary conditions.

The Nipa palm found in the Upper Wilcox is clearly an inhabitant of muddy tidal shores so that it would naturally be expected in the laminated clays of the Upper Wilcox. Its single modern representative is tolerant of water of considerable salinity and is a member of the mangrove association of the Orient. It shows many points of affinity with the Pandanaceæ and has never before been found in the Western Hemisphere. Like so many forms which are strictly oriental in the existing flora such as *Cinnamomum, Artocarpus, Phœnix*, etc., it enjoyed a cosmopolitan range during at least the earlier half of the Tertiary period.

A somewhat full account of *Nipa* has been recently published by me[9] and need not be repeated in the present connection.

The single species of *Canna* of the Wilcox represents a strictly hygrophilous type which is confined to America in the existing flora. It is an inhabitant of estuary and river swamps near the coast, and that the Wilcox species inhabited a similar situation is indicated by its restricted occurrence and its association with *Sabalites* near the mouth of a Wilcox river, which on other grounds is known to have been present in Lafayette County, Mississippi.

The Dicotyledonæ of the Wilcox as might be expected are largely choripetalous forms since there are over 250 species of Choripetalæ

[9] Berry, E. W., *Am. Jour. Sci.* (IV.), Vol. 37, pp. 57–60, Fig. 1, 1914.

(Archichlamydeæ) and only 35 species of Gamopetalæ (Sympetalæ). At the same time the representation of Gamopetalæ is really much larger than might be expected thus early in the Eocene and many families often thought to be relatively more modern have been found to be represented.

The following orders of Choripetalæ are not represented in the Wilcox flora: Casuarinales, Piperales, Salicales, Balanopsidales, Leitneriales, Santalales, Sarraceniales and Opuntiales. The absence of the Balanopsidales, Sarraceniales and Opuntiales is not remarkable since they are all specialized types and the rather uniform habitats of the cacti and their relatively modern evolution both conspire to eliminate them from Eocene coastal floras. The presence of the primitive Casuarinales and Piperales might be expected especially since there is a well marked Piper-like form in the Upper Cretaceous of Alabama. The Salicales while prevailingly temperate forms are abundantly represented in the Upper Cretaceous floras of the embayment area and the Santalales have also been recorded from the American Upper Cretaceous and are present in the European Tertiary.

Those alliances of Gamopetalæ which are not present in the Wilcox to be enumerated presently are mainly the great modern and temperate zone groups. For example there are no Wilcox species of Ericales, Labiatæ, Convolvulaceæ, Bignoniaceæ. Scrophulariaceæ, Plantaginales, Valerianales or Campanulales, this proving not only the essential modernness of the evolution of the Compositæ[10] but firmly establishing the thesis that the Wilcox flora is a subtropical and not a temperate flora.

The following are the larger families in the Wilcox flora: The Lauraceæ with 30 species, Cæsalpiniaceæ with 26, Moraceæ with 23, Papilionaceæ with 22, Rhamnaceæ with 14, Sapindaceæ with 13, Sapotaceæ with 12, Myrtaceæ and Mimosaceæ each with 11, Combretaceæ and Anacardiaceæ each with 9, Juglandaceæ with 8, Celastraceæ with 7, and the Proteaceæ and Apocynaceæ each with 6.

The largest single genus is *Ficus* with 18 species, then comes *Cassia* with 12, *Sapindus* with 9, *Gleditsiophyllum* with 8, *Oreo-*

[10] The fruit described as *Carpolithus hyoseritiformis* is probably referable to the Compositæ.

daphne, Sophora and *Anacardites* each with 7, *Cinnamomum, Nectandra, Rhamnus, Myrcia* and *Bumelia* each with 6, and *Celastrus, Dillenites,* and *Apocynophyllum* each with 5. Ten species are referred to the form-genus *Carpolithus* and this number could readily be greatly increased if all the unidentified seeds were named and described.

The amentiferous families, in accordance with their Upper Cretaceous deployment and their undoubted primitive and not reduced character, are represented in the Wilcox flora by fourteen species, a number of which are individually abundant.

The Juglandales[11] are represented in the Wilcox by three species of *Juglans* only one of which, *Juglans Schimperi,* is at all common; by a doubtfully determined species of *Hicoria;* by three well-marked species of *Engelhardtia* and by an extinct type, *Paraengelhardtia,* of a habit similar to that of *Engelhardtia.*

The genus *Juglans* is one of the earliest of the still existing dicotyledonous genera to appear in the fossil record and it is continuously represented in fossil floras from the Mid-Cretaceous to the present. There are about 25 Eocene species of Walnut and they range during that period from the Gulf region to Alaska and Greenland, and are also present in the tropical forests of the Egyptian Fayum in the early Oligocene. The outlying existing species in the West Indies and under the equator in South America prove that in spite of the northward range of the Asiatic species in Manchuria and of some of the North American species into New England and southern Ontario, its progenitors were at least subtropical types, a fact corroborated by their foliar character since it is a well-known fact that compound leaves indicate tropical ancestry, and this is abundantly proven in the case of *Juglans* by its associates in the fossil floras where it has been found represented.

The genus *Engelhardtia*[12] is one of the most interesting Wilcox genera. In the first place the identification of its leaves is corroborated by two varieties of characteristic winged fruits.

The genus was described by Leschen in 1825 and contains about

[11] See Berry, E. W., "Notes on the Geological History of the Walnuts and Hickories," *Plant Woirld,* Vol. 15, 1912, pp. 225–240.

[12] See Berry, E. W., *Am. Jour. Sci.* (IV.), Vol. 31, 1911, pp. 491–496; *Plant World,* Vol. 15, 1912, pp. 234–238, Figs. 3, 4.

ten species of the southeastern Asiatic area. These range from the northwestern Himalayan region, where they extend a short distance north of the Tropic of Cancer, through farther India and Burma to Java and the Philippines. The pistillate flowers are small and are grouped in paniculate spikes. They develop into small drupe-like fruits, each of which is connate at the base to a large expanded trialate involucre.

A single little known species, rarely represented in even the larger herbaria, occurs in Central America and is the type and only species of the genus *Oreomunnea* of Oersted. This is much more restricted in its range than are its kin beyond the Pacific. *Oreomunnea* is very close to *Engelhardtia,* and for the purposes of the paleobotanist the two may be considered as identical since they represent the but slightly modified descendants of a common ancestry which was of cosmopolitan distribution during the early Tertiary. The present isolation of *Oreomunnea* furnishes a striking illustration of the enormous changes which have taken place in the flora of the world in the relatively short time, geologically speaking, that has elapsed since the dawn of the Tertiary.

The principle has frequently been enunciated that when closely related forms are found in the existing flora of the world, restricted in range and isolated from their nearest relatives, or when other existing genera are monotypic, it is quite safe to predict an interesting and extended geological history. *Engelhardtia* proves to be another illustration of this principle, for its peculiar three-winged fruits have been known in the fossil state for almost a century. They were long unrecognized, however, and the earlier students who described them compared them with the somewhat similar winged fruits of the genus *Carpinus* (Betulaceæ). With the botanical exploration of distant lands in the early part of the nineteenth century, specimens of *Engelhardtia* began to be represented in the larger European herbaria, and Baron Ettingshausen, that most sagacious of paleobotanists, as long ago as 1851 pointed out that certain supposed species of *Carpinus* were really fruits of *Engelhardtia.* He returned to the subject in 1858 without, however, actually changing the names of any of the supposed species of *Carpinus* nor does he seem to have been aware of the existence of a living species of *Engelhardtia* (*Oreomunnea*) in Central America.

Since Ettingshausen's announcement a dozen or more fossil species have been described. The oldest known European form occurs in the lower Oligocene (Sannoisian) of France and the species become increasingly abundant throughout southern Europe, especially toward the close of the Oligocene and the dawn of the Miocene, Saporta stating that the slabs from the leaf-beds at Armissan in southeastern France are thickly strewn with their peculiar fruits. Fossil forms continue in Europe throughout the Miocene and Pliocene and specimens of late Miocene or early Pliocene age are recorded from Spain, France, Italy, Croatia, and Hungary.

The Wilcox species are somewhat older than any of the known European forms

The existing *Engelhardtias* are upland forms and this may possibly have been their habitat in Wilcox times although their abundance at different localities along the Wilcox coast would seem to indicate that this was not the case.

The genus *Paraengelhardtia,* which is a unique type confined to a single locality in the Wilcox, is clearly allied to *Engelhardtia,* as I have shown in the systematic chapter. It seems probable that it represents a survival of the ancestral stock from which *Engelhardtia* was derived since its fruits are more primitive and indicate ancestral forms with smaller bracts comparable with the bracts of *Juglans* or *Hicoria* which in the course of time became accrescent and subsequently deeply trilobate. The primitive character of *Paraengelhardtia* and the presence of true *Engelhardtias* in the Wilcox so much earlier than their first occurrence in Europe suggests that America was the original home of the *Engelhardtia* stock, although this supposition cannot be verified or disproved until a Tertiary paleobotanical record for the continent of Asia is available.

The Myricales contains but two species of *Myrica* in the Wilcox flora. *Myrica* is a very old generic type with a large number of fossil species ranging from the Middle Cretaceous to the present. The existing species are relatively few in number and widely scattered geographically and represent survivors from a Tertiary cosmopolitan distribution. *Myrica*[13] is much less abundant in the Wilcox

[13] The allied and monotypic genus *Comptonia* which by some students is included in *Myrica* has an extended geologic history which has been discussed by Berry, *Amer. Nat.,* Vol. 40, 1906, pp. 485–520, pl. 1–4.

than in the European Tertiary, although it was present in the embayment area in the late Upper Cretaceous (Ripley formation of Tennessee). Its meager representation in the Wilcox time may be due to the more tropical climate conditions. The modern Myricas are temperate and subtropical and a number of the species are coastal forms of either swamps or sand dunes. *Myrica elæanoides* was evidently a coastal form and so was *Myrica wilcoxensis*. The latter is very similar to the existing *Myrica cerifera* which ranges from New Jersey to Texas and is also found on the Bermudas and Bahamas. It is most abundant and vigorous in the sandy swamps along the south Atlantic and Gulf coasts and its habitat may be compared with that of *Myrica wilcoxensis*. The latter seems to be the ancestral stock of a very similar species which occurs along the Middle Eocene (Claiborne) coast of the embayment.

The order Fagales, which includes such important timber trees of the temperate zone, is comprised by the two families Betulaceæ and Fagaceæ, together containing about 450 existing species, of which three fourths belong to the Fagaceæ. Only the latter family is represented in the Wilcox although the Betulaceæ are characteristically developed in the Upper Cretaceous of North America.

The family is unrepresented in the Wilcox flora probably because the climate was too warm and this reason may also account for the absence of true oaks since the Fagaceæ are represented in the Wilcox flora by only the genus *Dryophyllum* with four rather widespread and often common species.

The genus *Dryophyllum* is of worldwide distribution and consistently uniform characters in the various horizons of the late Cretaceous and early Eocene from the Senonian to the Ypresian stages. It especially characterizes the dawn of the Eocene and represents the ancestral stock from which the genera *Castanea, Castanopsis, Pasania* and *Quercus* took their origin, although this origin was in the late Cretaceous. As might be expected *Dryophyllum* has long since become extinct. The Wilcox species were apparently strand types as were also the numerous species enumerated by Debey, the describer of the genus, from the sandy shores of the Upper Cretaceous sea of Rhenish Prussia. *Dryophyllum* is abundant in the Montian of Belgium and in the littoral sands of Ostricourt and Belleu in

France. In the systematic chapter detailed comparisons are made between the Wilcox and the foreign species, which show a striking parallelism.

The Urticales includes the families Ulmaceæ, Moraceæ and Urticaceæ together containing about 1,600 existing species. The Urticaceæ are largely herbaceous forms and the Ulmaceæ are mostly extratropical.

The Ulmaceæ comprise thirteen genera and about 140 existing species, widely distributed in temperate and tropical regions. A single species of *Planera* described originally by Newberry from the Western Eocene is doubtfully identified from the Wilcox. The genus is monotypic in the existing flora and confined to wet swampy situations in the warm temperate region of southeastern North America. Its geologic history goes back to the Upper Cretaceous at which time species have been recognized along the Atlantic coast from North Carolina northward. Thus there is no reason why it should not have been present in the early Tertiary of the embayment unless it be argued that the climate was too warm.

The Moraceæ, by far the largest family of the order Urticales and the only one certainly represented in the Wilcox flora, contains between 900 and 1,000 existing species segregated among about 55 genera, of which the genus *Ficus* is by far the largest, including about 60 per cent. of the existing species of the family. The Moraceæ are distinctly tropical and warm temperate types and are most abundant in the oriental tropics, although the dominant genus *Ficus* is widespread and the family is also largely represented in the South American tropics.

There are at least 18 monotypic genera of which one is North American, four South American, four African, and nine Australian. No single tribe is confined to a single continental area and all show apparent anomalies of distribution due to our lack of knowledge of their geologic history. The genera *Ficus, Artocarpus* and *Artocarpidium* go back to the base of the Upper Cretaceous and numerous additional genera appear in the Eocene.

There are 23 species of Moraceæ in the Wilcox flora. The genus *Artocarpus* is represented by three well-marked species. In the existing flora the two score known species of *Artocarpus* are confined

to the southeastern Asiatic region[14] although some of them are cultivated in all tropical countries. The tribe *Euartocarpeæ* of which *Artocarpus* is the largest existing genus, have, however, five of their genera confined to Central and South America, one confined to tropical West Africa, two confined to the southeastern Asiatic region, one to Borneo and one ranging from Japan to Australia. While the geologic history of *Artocarpus* is only imperfectly known at least 15 different fossil species have been described. The oldest is a well-marked form based on characteristic leaves and parts of the fruit which show the typical surface features. It has been fully described by Nathorst[15] and comes from the Atane beds (Cenomanian) of West Greenland. Slightly younger is a less well-defined form recorded from the Emscherian of Westphalia, and the somewhat doubtful genus *Artocarpophyllum* of Dawson from the Upper Cretaceous of Vancouver Island. Another species is recorded from the Laramie formation and the genus is widely distributed in the basal Eocene of North America. It continues in the Mississippi Gulf region until the close of the Oligocene, the last recorded occurrence being in the Alum Bluff sands at Alum Bluff on the Apalachicola River. On the Pacific coast it is found in deposits in California and Oregon which are referred to the Miocene. In the European area it occurs in the Tongrian of France, the Tortonian of Baden, the Pontian of France and Italy and the Pliocene of Italy. It is present in both the Pliocene and Pleistocene of the island of Java.

Artocarpus is said to be represented by petrified wood in the Oligocene of the island of Antigua and it was evidently a member of the American flora from the Upper Cretaceous until late in the Tertiary, although like the genera *Cinnamomum, Nipa, Phœnix,* etc., it is not represented in post-Pleistocene American floras. An extinct genus related to *Artocarpus* and named *Artocarpoides* by Saporta, who described several species from the Paleocene of France, is represented by a single Wilcox species.

[14] It is found throughout Oceanica and was present in the Hawaiian and Marquesas when they were first visited by Europeans. It was introduced into the West Indies in 1793.

[15] Nathorst, *Kgl. Svenska Vetens-Akad. Handl.*, Bd. 24, No. 1, 1890, 10 pp., 1 pl.

The genus *Cecropia* with about 40 existing species confined to the tropics of South America has two species in the Aquitanian of Bohemia and the Midway and Wilcox form described as *Ficus* sp. is very probably a representative of this genus.

The genus *Pseudolmedia,* with five existing species in the American tropics, has a well marked species in the Wilcox flora. As far as I know it has not heretofore been recorded in the fossil state although it is probable that some of the very numerous fossil species of *Ficus* may represent *Pseudolmedia.*

The genus *Ficus* is represented by numerous species in the Wilcox flora no less than eighteen having been described and a number of these are individually abundant. They include the narrow lanceeolate forms of the *Ficus elastica* type, with close-set laterals, as well as open-veined lanceolate forms, and the shorter and broader palmately veined forms. None are lobate or have toothed margins. *Ficus* was evidently much more abundant and varied along the Wilcox coast than it is today throughout the West Indies and more nearly comparable in this respect with the display of figs in the East Indies or in tropical South America.

The number of fossil forms that have been referred to *Ficus* are very numerous, numbering perhaps 300 species. None are certainly known from the Lower Cretaceous, the genus *Ficophyllum*[16] being entirely doubtful. In the Upper Cretaceous, however, *Ficus* is very widespread and abundant, seemingly indicating a Lower Cretaceous ancestry as yet unknown. The Cenomanian stage has furnished 3 species in Greenland, 6 along the Atlantic Coast and 24 in the interior of North America, as well as 11 in Saxony, Bohemia and Moravia. The succeeding Turonian stage furnishes 4 species in Bohemia and the Tyrol, and several in North America (Tuscaloosa, Magothy, Black Creek, Eutaw formations). Later Upper Cretaceous horizons have abundant species of *Ficus* everywhere throughout North America and Europe as well as in Greenland, Australia and New Zealand, and this cosmopolitanism continues throughout the Tertiary, there being about 50 Eocene species, about 60 Oligocene species, 90 Miocene species and 20 Pliocene species. Africa is added to the record in the basal Oligocene, and Asia in the Miocene.

[16] See Berry, E. W., Md. Geol. Surv., Lower Cret., 1911, pp. 502–506.

The fossil records will have to be much more complete before the original center of radiation of the Moraceæ can be determined, the present brief sketch can be said to merely indicate that not only *Ficus*, but other genera like *Artocarpus* that are entirely oriental in the present, were normal elements in North America floras, from the time of the modernization of these floras at the beginning of the Upper Cretaceous onward. Along our east coast, they apparently became restricted in their range at the dawn of the Miocene and they apparently never after became as important in southeastern North America as they had been, or as they are in the recent flora of the Orient.

The order Proteales includes the single family Proteaceæ with about one thousand existing species. They include the prominent arborescent forms of Choripetalæ in the Southern Hemisphere, to which region all but the four genera *Roupala, Protea, Leucospermum* and *Helicia* are confined. They are usually considered as Australian types, in fact the majority of the genera and species are confined to that continent, nevertheless there are four genera in South America together containing over fifty existing species, and there are several genera peculiar to the African flora; and the genus *Helicia* is predominantly Asiatic.

The geologic history of the Proteaceæ is perhaps one of the most striking instances that paleobotany affords of the great difference in geographical distribution in former ages from what could possibly be inferred from a study of the present geographical distribution of the members of this family, although there are some significant features in the distribution of the recent forms that will be alluded to in a subsequent paragraph.

The discovery of fossil forms of Proteaceæ in the Tertiary deposits of Europe was the inspiration of a considerable literature[17] and was the occasion of a rather acrimonious controversy regarding their botanical affinity. This is well illustrated in the dissenting opinions expressed by the botanists Hooker and Bentham who both regarded fossil leaves as undeterminable. Starting with this apriori principle it is difficult to see how they could arrive at any other conclusion.

[17] See the writings of Unger, Heer, Ettingshausen, Schimper, Schenk and Saporta.

The most expeditious refutation of their opinion is furnished by the present distribution of some of the genera, *e. g.*, the genus *Roupala* has 36 species in tropical America, 2 in New Caledonia and 1 in Queensland: the genus *Embothrium* has four Andean species and one in Australia: the genus *Lomatia* has 3 species in Chile, 4 in Australia and 2 in Tasmania. It follows unless one is prepared to subscribe to the doctrine of special creation for each continent or to the independent evolution on separate continents of different species of the same genus, that during their geologic history these genera must have ranged over intervening areas, so that if the Cretaceous and Tertiary plants of the northern hemisphere with fruits and leaves of the Proteaceæ are not related to the genera that they resemble most, then forms with leaves and fruit resembling those of other families must be fossil Proteaceæ, which ought to seem absurd, even to an English botanist. As a matter of fact, while exception may justly be taken to some determinations of Unger, Ettingshausen and Heer, they in no wise affect the main body of facts and there is so much collateral evidence furnished for example by the geologic history of the Araucarian conifers, and the history of the Proteaceæ is so similar to that of the Myrtaceæ and Leguminosæ—the two other great families of the existing Australian flora, that the evidence seems conclusive.

Turning now to the fossil record those who follow the opinion of Hooker or Bentham will see how vast and substantial are the supposed illusions of the paleobotanists. In addition to the two extinct genera in the Wilcox flora I have fossil records of 32 genera of Proteaceæ, although this is artificially enlarged by the joint usage, according to taste, of names like *Dryandra* and *Dryandroides, Banksia and Banksites,* etc. A brief consideration of these genera with fossil representatives will prove valuable.[18]

The genus *Protea* Linné, from which the family takes its name, has about 60 existing species occupying disconnected areas in Central and South Africa. To it have been referred a middle Cretaceous species from Saxony; 3 Aquitanian species from Prussia, Bohemia and Greece; 1 species from the Burdigalian of Italy; 1 from

[18] This list is not complete but sufficiently so for the purpose of this discussion.

the Helvetian of Switzerland and one from the Messinian of Italy. Allied to *Protea* but possibly more generalized is the genus *Proteoides* of Heer. This has several Tertiary species and a considerable number of Upper Cretaceous species (15). There are two each in the Cenomanian of Bohemia and Lesina, two in the Atane beds of Greenland, three in the Dakota sandstone of North America, one in the Tuscaloosa formation of Alabama, one in the Middendorf beds of South Carolina, one in the Cretaceous of Australia, two in the Vancouver Island Cretaceous and one in the Senonian of Saxony.

The genus *Proteophyllum* Velenovsky[19] a still more generalized proteaceous type has seven species (Saporta, 1894) in the Albian (Vraconian) of Portugal and 8 species in the Perucer beds (Cenomanian) of Bohemia. Another generalized type is *Proteopsis* Velenovsky with a single species in the Cenomanian of Bohemia. The genus *Proteæphyllum* of Fontaine containing 2 species in the Patuxent fomation (Neocomian) of Virginia I regard as entirely worthless.[20] The genus *Conarrhenes* Labill with one existing species in Tasmania has a single species based on both foliage and fruit in the Miocene of Carniola according to a determination of Ettingshausen's which may well be viewed with suspicion. The genus *Conospermum* Smith with about 33 existing species in Australia has two fossil species in the Oligocene of Styria and one in the Miocene of Carniola, while the somewhat less definite genus *Conospermites* (Ettingshausen, 1867) has a fossil species in the Upper Cretaceous of Australia and one in the Cenomanian of Saxony and Bohemia.[21]

The genus *Helicia* Lour. is of especial interest since it is found farther north in the existing flora than any other member of the family. There are about 25 modern forms, mostly Indomalayan, but a few still survive in or have recently spread to Australia. The fossil record includes a species in the Oligocene of Styria and another in the Pliocene of Italy. The genus *Lambertia* Smith with 8 existing Australian species has a single fossil species in the Miocene of Carniola. The genus *Hakea* Schrad. with 100 recent Australian

[19] Velenovsky, Kvetena českého cenomanu, 1889, p. 18.

[20] See Berry, Md. Geol. Surv., Lower Cretaceous, 1911, pp. 494–499.

[21] I regard Fontaine's determination of a species in the Lower Cretaceous of Virginia as worthless.

species has eleven fossil species in the Oligocene of Europe; in France, the Tyrol, Saxony and Greece; and no less than 17 Miocene species in France, Italy, Switzerland, Baden, Hesse, Prussia, Bohemia, Austria, Styria, Croatia and Hungary.

The genus *Knightia* R. Brown with a modern species in Australia and 2 in New Caledonia has a fossil form in the Eocene of Australia and another in Graham Land[22] in beds regarded as Oligocene. The allied genus *Knightites* Saporta has two species in the Sannoisian of France.

The remarkable genus *Lomatia,* previously mentioned, has four existing species in Australia, 2 in Tasmania and 3 in Chile. As might be expected from their modern isolated occurrences there are over 30 fossil species based in some cases on associated leaves and fruits. The oldest of these are two (perhaps wrongly identified) species in the Dakota Sandstone. Eocene records include the Green River shales of North America, a Ypresian species from the south of England, an Italian species, five Australian and one Tasmanian species. There are about a dozen Oligocene species, some of which are very characteristic. They occur in the Tyrol, Saxony, Baltic Prussia and Styria, and the relatively large number of four are recorded by Dusén from Graham Land (Antarctica). There are also about a dozen Miocene species recorded from such separated areas as Colorado, Switzerland and Carniola. The wonderfully preserved leaves in the volcanic ash beds at Florissant, Colorado, from which seven forms have been described, the only known Miocene occurrence of *Lomatia* in North America, are alone sufficient to confound the sceptics.

The allied genus *Lomatites* Saporta has a Cenomanian species in Saxony and five or six Oligocene species in France. The genus *Stenocarpus* R. Brown, with 11 existing species in New Caledonia and 3 additional ranging from North Australia to New South Wales, has a single fossil species in the Oligocene of Saxony.

The genus *Persoonia* Smith has 60 existing species in Australia and one in New Zealand. The fossil record includes two widely distributed species in the Upper Cretaceous of North America; one

[22] Dusén, Wiss. Ergeb. Schwed. Südpolar. Exped., 1901–03, Bd. 3, Lief. 3, p. 7, Pl. 1, Figs. 7, 9, 11, 1908.

in the English Eocene; four in the Oligocene of Tyrol, Saxony, Styria and Greece; ten in the Miocene of France, Italy, Switzerland, Baden, Bohemia, Styria, Croatia, Carniola and Slavonia. A large number of these fossil forms of *Persoonia* are not especially convicing but certainly the three European species *Persoonia cuspidata, daphnes,* and *Myrtillus* of Ettingshausen[23] which have the leaves associated with characteristic fruits are above suspicion.

Bowerbank in his classic study of the pyritized fruits and seeds from the Island of Sheppey established a genus which he called *Petrophiloides* from its resemblance to the genus *Petrophila* R. Brown which has about 35 existing species in Australia, the majority of which are confined to West Australia. Bowerbank described several species one of which was shown by Starkie Gardner to be an *Alnus* fruit and others have been referred to *Sequoia.* Ettingshausen[24] in the study of the Sheppey fruits after careful comparisons retained three English Eocene species and the genus has also been recognized in the Sannoisian of Dalmatia and Styria.

The genus *Leucadendrites* was established by Saporta for a Sannoisian species of southeastern France from its resemblance to *Leucadendron* Herm., which has upwards of 70 existing species in South Africa.

The genus *Grevillea* R. Brown has 56 existing species confined to Australia. The fossil record includes a Cretaceous species in Australia; two Cenomanian species in Bohemia (*Grevilleophyllum* Velenovsky); three Eocene species in England, France and Italy; twelve Oligocene species mostly in southern France but also represented in Saxony, Tyrol, Bohemia, Styria and Greece; and twelve Miocene species in France, Switzerland, Bohemia and Croatia.

The genus *Embothrium* Forst., already alluded to, has four existing species in South America which range from Chile to the Straits of Magellan, and a fifth species in Australia. This widely separated occurrence is explained when the fossil record is combined with the occurrences referred to *Embothrites, Embothriopsis* and *Embothriophyllum.* To *Embothrium* are referred 8 Oligocene spe-

[23] Ettingshausen, *Sitz. K. Akad. Wiss.,* Wien, Bd. 7, 1851, pp. 718–719, Pl. 30, Figs. 6–14.

[24] Ettingshausen, *Proc. Roy. Soc. Lond.,* Vol. 29, 1879, p. 394.

cies of Styria and Greece and 4 Miocene species of Baden, Styria, Croatia and Hungary. To *Embothriopsis* Hollick a single species from the Long Island Middle Cretaceous is referred.

Embothriophyllum is used by Dusén for a single species from the supposed Oligocene of Graham Land. The genus *Embothrites* Unger has a doubtful species in the Dakota Sandstone; six Oligocene species in France, Tyrol, Styria, Carniola and Greece; and 3 Miocene species in Croatia and Bohemia.

The genus *Dryandra* R. Brown has about 50 existing species in Australia. The fossil forms have occasioned much discussion and have been referred back and forth between this genus and *Comptonia* and *Myrica*. The forms retained in *Dryandra* include a Cenomanian species in Bohemia and Moravia; an Eocene species in France; two Eocene species in Australia and an Oligocene species in Greece. The allied forms referred to the genus *Dryandroides* Unger include 5 Upper Cretaceous species in Europe and North America; an Eocene species in Tasmania; 4 Oligocene species in Italy, Tyrol, Saxony, Styria and Greece; and a Miocene species in Bohemia.

The allied genus *Banksia* Linné fil., also confined to Australia in the existing flora, has 7 Upper Cretaceous species—4 Australian and 3 in the North Temperate zone, ten Eocene species, of which 7 are Australian, 1 Alaskan (?) and 2 English; twelve Oligocene species widely distributed in Europe; 16 Miocene species equally widespread in Europe; and a Pliocene species in Italy. Three especially well marked species from the Wilcox have been referred to this genus.

The allied genus *Banksites* Saporta has a Cenomanian species in Bohemia and various Tertiary records from Europe hopelessly entangled in the literature with *Banksia, Dryandra* and *Dryandroides*. The genus *Roupala* Aublet (*Rhopala*), whose peculiarly isolated outliers in Queensland and New Caledonia have already been mentioned, is common in northern South America, extending northward to Guatemala. Fossil forms are recorded from the Cenomanian of Saxony, from the Eocene of Australia and from the Aquitanian of Switzerland. In addition Saporta described a *Rhopalospermites* from the lower Oligocene of France and a species of *Rhopalophyllum* has been described from the Upper Cretaceous of Australia and a second from the Miocene of Styria.

The geological history sketched in the preceding paragraphs is necessarily fragmentary, nevertheless I think the data are sufficient after excluding doubtful determinations to show that the family had its origin in the northern hemisphere, making its first appearance in the fossil record at the close of the Lower Cretaceous, becoming practically cosmopolitan during the Upper Cretaceous at which time it reached the Australian region from southeastern Asia. New Zealand must have already been segregated but not the land mass now represented by New Caledonia. During the early half of the Tertiary Africa and southern Europe were essentially a single floral province while in the Western Hemisphere the Proteaceæ ranged from the United States through South America and an unknown distance across Antarctica. Concomitant with the continent building and the consequent climatic changes of the Miocene the area of distribution commenced that shrinking which culminated during the Pleistocene, leaving the stranded remnants of the stock in their present widely separated quarters of the southern hemisphere. Not all the modern genera took part in this history since the local peculiarities of poor soil and rigorous climate combined with relative freedom from outside competition were the factors that stimulated a Tertiary evolution of forms in Australia in exactly the same manner as the peculiar Australian genera of Myrtaceæ and Leguminosæ were evolved.

The Wilcox species of Proteaceæ are six in number and are distributed in four genera, in addition to which a probable *Banksia* fruit is retained in *Carpolithus*. These genera are *Palæodendron, Proteoides, Knightiophyllum* and *Banksia*. The genus *Palæodendron,* not mentioned in the preceding paragraphs, was proposed by Saporta for small entire coriaceous leaves from the Sannoisian of southern France and is an entirely extinct type, sparingly represented in the Wilcox by a single species. The genus *Proteoides* was established by Heer for generalized proteaceous types which are well represented in the Upper Cretaceous floras of the embayment area as well as elsewhere. It is represented in the Wilcox by a single well-marked species confined to the Middle and Upper beds. The genus *Knightiophyllum* is proposed here for the first time for a well-marked long petioled, aquiline-toothed, coriaceous form of common occurrence at

Peryear. It is named from its resemblance to the genus *Knightia* R. Brown, a genus of few existing species confined to the Australian region but apparently represented in Europe during the Tertiary as has already been indicated.

The genus *Banksia,* with three Wilcox species, two of which are particularly well marked and a probable fruit, *Carpolithus proteoides,* is confined in the existing flora to the Australian region with about 50 species. The other genus of the tribe Banksieæ is *Dryandra* R Brown also with about 50 existing species confined to the Australian region. It is much like *Banksia* in its foliar characters. Both genera are found in abundance in the European Tertiary and undoubtedly enjoyed a more or less cosmopolitan range during the early Tertiary. Their ancestors probably entered the Australian region during the Upper Cretaceous before that country had become entirely separated from Asia, becoming adapted to the peculiar soils and climate of Australia, while the stock in the northern hemisphere appears to have been unable to stand the climatic changes and Tertiary competition and thus became extinct.

The Aristolochiales is placed by some students among the Gamopetalæ. It includes besides the Aristolochiaceæ, the two parasitic families, the Raffleisiaceæ and Hydneraceæ, altogether containing about 235 existing species, of which 205 belong to the Aristolochiaceæ, the only family of this order represented in the Wilcox flora. The genus *Aristolochia,* to which a typical fruit from the Wilcox is referred, is found in the American Upper Cretaceous and in both Europe and America during the Tertiary. There are about 180 existing species all perennial herbs or climbing vines and widely distributed in both tropical and temperate regions, about ten species being found within the United States.

The order Polygonales includes the single family Polygonaceæ with about 800 existing species segregated in about 30 genera, widely distributed. They embrace herbs, shrubs, vines and trees, with mostly cyclic flowers, and in their morphological features show some evidences of transition between the previous choripetalous alliances and the Chenopodiales. The geologic history of the family is practically unknown and it would seem that a large part of the specific variation, particularly of the temperate herbaceous forms, was rela-

tively modern. The family is represented in the Wilcox by the single genus *Coccolobis* with two species which appear to be the Eocene prototypes of the only two existing arborescent species of Polygonaceæ that reach the United States (the sea grape and the pigeon plum). The genus *Coccolobis* has about 120 existing species all confined to the American tropics and it would appear that it was of American origin. These species range from southern Florida to Mexico, Central America, Brazil and Peru and the majority are coastal forms. The two modern species which are so much like these two ancestral forms in the Wilcox, are strand types found from the Florida keys through the West Indies to the northern coasts of South America, and the conclusion is almost irresistible that the Wilcox forms enjoyed a similar range and an identical habitat.

The Chenopodiales (Centrospermæ of Engler) include ten families culminating in the Caryophyllaceæ, and containing about 3,500 existing species. They appear illy assorted and show a wide range in floral and other morphological characters. Perhaps a majority are modern types. The single family Nyctaginaceæ represents this order in the Wilcox.

The Nyctaginaceæ with about 150 existing species is predominantly American within the limits of the southern United States on the north and Chile and Argentina on the south. The genus *Pisonia* Plumier, the only genus thus far found in the Wilcox flora, is represented by three well-marked species. It has about 40 existing species chiefly in the American tropics and contains the only arborescent form of the family found within the United States. It has an extended geologic history, well-marked forms being found in the European and American Upper Cretaceous. The Wilcox species were undoubtedly strand types as are so many of the modern species, which inhabit sea beaches, the shores of salt water lagoons and marshes, the scrub of beach ridges and the jungle behind them. In the existing flora it is associated with *Pithecolobium, Reynosia, Metopium, Acacia, Bumelia, Cordia, Coccolobis, Ocotea, Fagara, Mimusops, Conocarpus, Cassia, Eugenia, Anona, Ficus,* etc., exactly as it was during Wilcox time. Species of *Pisonia* occur in the Upper Cretaceous of the Atlantic Coastal Plain (Black Creek formation) as well as in the Middle (Claiborne) and Upper (Jackson) Eocene.

The order Ranales appears to me to be a highly unnatural assemblage, which doubtless explains the prolonged discussion and wide range of opinion regarding its true status. As treated in Engler and Prantl it includes 16 families with over 4,000 existing species. While a distinct calyx and corolla are the prevailing habit this is combined with such primitive features as apocarpy and hypogyny, and by a well marked tendency to indefinite repitition and spiral arrangement of the floral members. I have removed the Lauraceæ which contain ¼ of the existing species to a place in the more evolved order Thymeleales.

The Ranales as a whole show no close filiation with previous alliances. They include forms that are more nearly Monocotyledons than Dicotyledons (Nymphæaceæ) and numerous botanists (*e. g.* Wieland, Arber, Hallier) see in them the logical zenith of evolution of the Mesozoic Cycadophytes and thus as representing the ancestral stock from which the Angiosperms were descended—apparently a most remarkable feat, except on paper, when any except floral features are taken into account.[25]

Considering as I do that the Ranalian alliance is a plexus containing unrelated elements, any extended consideration of their geologic history would be fruitless. Certain forms are well represented among the oldest known display of Angiosperms in the Middle Cretaceous. Only two Ranalian families are represented in the Wilcox flora and these two are both natural groups closely related and typically Ranalian. I refer to the families Magnoliaceæ and Anonaceæ.

The family Magnoliaceæ comprises about 70 existing species segregated into nine or ten genera, by far the largest of which is the genus *Magnolia* with about 21 species of eastern and southern Asia southern Mexico and the eastern United States. The family is mainly tropical and the bulk of the existing forms occur in southeastern Asia, the magnolias of that region being largely forms of tropical uplands.

There are numerous apparent anomalies in the distribution of the recent forms, thus none are native in Europe, although *Magnolia* per-

[25] For discussion of this theory see recent papers by Wieland, Arber and Parkin, and Hallier.

sisted in that region as late in geologic time as the early Pleistocene. Only one genus, *Drimys* Förster, occurs in South America or Australasia. There is a singular pairing of forms in southeastern Asia and southeastern North America. For example *Magnolia* has 14 species in the former region and seven in the latter: *Talauma* Jussieu has 3 species in farther India and one in the West Indies: *Liriodendron* Linné has a single species in each: *Schizandra* Michaux has species in each: *Illicium* Linné has five species in the former region and two in the latter. The general *Michelia* Linné (13 sp.) and *Kadsura* Jussieu (7 sp.) are confined to the former region and *Zygogynum* Baillon is confined to the island of New Caledonia. The leaves of all are entire and more or less elliptical with a coriaceous texture, often evergreen, and with a characteristic camptodrome venation. Of the seven species of *Magnolia* found within the limits of the United States, *Magnolia glauca* Linné ranges northward to Massachusetts and *Magnolia acuminata* Linné to New York and Ontario. About sixty fossil species have been referred to *Magnolia*. These are largely based upon leaves, although characteristic fruits, and in at least two cases, parts of flowers, have been found at various horizons. Magnolias are very abundant in both individuals and species in the Middle Cretaceous (Cenomanian-Turonian) especially in North America, where they are found along the Cretaceous Atlantic Coast from Greenland southward to Texas and in equal abundance about the borders of the advancing interior sea represented by the deposits known as the Dakota sandstone. They are much less common in Europe and the genus is either of American or Arctic origin.[26]

The Eocene records include 4 species of the Arctic region and 13 additional forms largely American, but some few European. The Oligocene, unrepresented in America by plant beds, has several European species toward its close. About eight Miocene species are recorded, of which the majority are American. The Pliocene, also practically unrepresented by plant beds in America, has furnished 5 or 6 European species and one is found in the early Pleistocene of that region. *Magnolia* seems to have been very abundant along the

[26] *Magnolia Delgadoi* Saporta, *Fl. Foss. Port.*, p. 194, Pl. 35, Fig. 5, 1894, recorded from the Albian of Portugal is almost certainly not a *Magnolia*.

shores of the extended Mediterranean sea of the Pliocene and to have subsequently been entirely exterminated in that region by the glaciation of the Pleistocene, while surviving in both North America and Asia by reason of the prevailing north and south trend of the of the mountain ranges. Some of the other genera of the Magnoliaceæ are represented by scattered fossil species but the record is too incomplete for generalizations. A survey of all the facts leads me to consider America as probably the original home of *Magnolia* and despite the massing of the existing forms in the eastern United States and their extension to Arctica in the Eocene, they probably originated in a warm-temperate or subtropical latitude, spread northward across Arctica to Eurasia, were cosmopolitan in the Tertiary, becoming restricted to the southeastern parts of Asia and North America by the aridity accompanying uplift, so well illustrated in the Eocene and later history of the Rocky Mountain and Great Plains province, and were finally killed off in Europe by the Pleistocene glaciation.

Lesquereux referred two forms from the Wilcox of northern Mississippi to *Magnolia* but these both prove to be species of *Terminalia* as Lesquereux had surmised in his preliminary studies. The genus *Magnolia* is, however, represented in the Wilcox by two large-leafed species, both of which are common to the basal Eocene of the Rocky Mountain Province. Neither show any close affinity with the antecedent Upper Cretaceous forms which are so common in the embayment area of Alabama and northeastward along the Atlantic Coastal Plain.

The family Anonaceæ contains about 700 existing species distributed among about 48 genera, only two of which are present in North America. The family is practically confined to the tropics, a single Australian species and the North American genus *Asimina* with 6 or 7 species being the only conspicuously extratropical forms. The area of maximum representation is southeastern Asia and the adjoining region of Malaysia, for while only 16 genera are confined to this region it contains over 350 species, and six additional genera (*Miliusa, Uvaria, Polyalthia, Oxymitra, Melodorum,* and *Poporvia*) with a total of over 250 species have the bulk of their species in this area. Only a single genus is confined to Australia and the bulk of the Aus-

tralian species are to be regarded as migrants from the preceding area. There are upwards of 100 species and 6 peculiar genera in tropical Africa; and America has about 200 species and 10 peculiar genera. These are all confined to the tropics except for a species of *Anona* which reaches the coast of peninsular Florida and for the genus *Asimina* with six or seven species of shrubs and small trees of the south Atlantic and Gulf States. One of these, *Asimina triloba* Dunal, is hardy as far north as New York and has the distinction of growing the farthest distance from the equator of any existing member of the family. The fossil record of the Anonaceæ is very incomplete, only the genera, *Anona* Linné and *Asimina* Adanson being known with certainty. Both of these genera are present in the Wilcox flora.

The genus *Anona* has from fifteen to twenty fossil species five of which are also represented by seeds. The oldest is a species described from the Dakota sandstone. There is a second species in the late Cretaceous or Early Eocene of the Rocky Mountain province. The flora of the Wilcox affords a glimpse into the true stage of evolution of Tertiary floras in that expanded belt of the American equatorial region which was the center of radiation of so many recent types. There were three exceedingly well marked species of *Anona* along the Wilcox coast and their leaves are very common at some localities although no seeds have as yet been discovered. I assume that these Wilcox forms had habits similar to those of the majority of the existing species, exemplified by our Florida *Anona glabra* Linné, or Pond Apple, which frequents shallow fresh water swamps, low shady hammocks, or stream borders near the coast. Other species occur in the low coppice association or on edges of brackish swamps on the Bahamas. The cultivated species, as for example the American *Anona reticulata* Linné which is planted in Guam often spreads naturally along the inner beaches, while attempts to introduce others of the most highly esteemed American species in the Orient have failed. From its prevalence among the existing species the habit of growing in wet shaded soils is evidently an old one, and since the Wilcox Anonas are associated with a strand flora, the assumption that they grew on the inner beaches or the shaded and more swampy edges of lagoons, possesses every degree of probability.

In the pipe-clays of Alum Bay which were contemporaneous with the Wilcox there are two species of *Anona,* and Engelhardt has described two species from the Eocene or Oligocene of Chili. The Oligocene record shows a species in France and a second in Saxony. In the Miocene there are two species each in England, Styria and Croatia and one each in Bohemia, Colorado and Transylvania. There is one each in the Pliocene of France and Italy, showing how modern was their extinction in the south of Europe.

The genus *Asimina* has only four or five recorded fossil species. These are all American except for a form from the Pliocene of Italy which has been referred to this genus, although I suspect that it represents *Anona,* since *Asimina* appears to have originated and been confined to the Western Hemisphere. The oldest known species is based on foliage which is found in the basal Eocene of the Rocky Mountains (Denver formation) and of the embayment (Midway Group). There is a single species based on a seed from the basal Wilcox and no other records except a form close to the modern from the late Miocene of New Jersey (Bridgeton sandstone) and the occurrence of the existing *Asimina triloba* Dunal in the interglacial beds of the Don valley in Ontario.

The order Papaverales (Rhoedales of Engler) includes six families—Papaveraceæ, Cruciferæ, Capparidaceæ, Resedaceæ, Tovariaceæ and Moringaceæ, together containing about 255 genera and 2,200 species. The Papaveraceæ and Cruciferæ are mostly herbaceous and widely distributed, largely in the North Temperate zone, and they are of relative recent evolution. The Resedaceæ is a small family largely confined to the Mediterranean region. The Capparidaceæ, Tovariaceæ and Moringaceæ are mainly tropical, the last two families consisting respectively of a single genus and two species of the American tropics and a single genus and three species, one Afric-Arabian and two East Indian.

The family Capparidaceæ with about 35 genera and 400 existing species is the only one of the order represented in the Wilcox flora. A majority of the existing species are herbaceous and they are found on all the continents in tropical and subtropical regions. Five subfamilies are recognized. Of these the Cleomoideæ and Capparidi-

odeæ are large and occur on all of the continents, with monotypic genera in North America (*Isomeris*), South America (*Stubelia Atamisquea, Belencita*), Africa (*Pteropetalum, Cladostemon*), and Australia (*Ræperia, Apophyllum*). The subfamily Dipterygioideæ has a single genus with only five or six species of Nubia, Arabia and the Punjab. The subfamily Roydsioldeæ includes about a dozen species, the genera *Roydsia* and *Stixis* being confined to India and the genus *Forchhammeria* being Mexican. The subfamily Emblingioideæ has only a single genus and species confined to Western Australia. No far-reaching conclusions regarding origin or past history can be deduced from our present knowledge of the geographical distribution of the Capparidaceæ and the fossil record is so imperfect that very little can be said regarding this history.

The following are the only fossil records known to me: F. von Müller has described somewhat uncertainly determined fruits from the Tertiary of Australia as the genera *Dieune* and *Plesiocapparis*. The latter has two species and is considered as probably a member of the section *Busbeckia* of the genus *Capparis*. Schenk has described the petrified wood of another form from the Tertiary of Egypt under the name *Capparidoxylon*. The genus *Capparis* has furnished a well-marked Wilcox species very close to the existing Antillean tree *Capparis domingensis* Sprengel. There are about one hundred existing species of *Capparis*, mostly tropical, and although found in the Eastern Hemisphere the majority occur in the American tropics, especially in Central and South America. The oldest known fossil forms are two species described by me as species of *Capparites* from the Upper Cretaceous of Alabama (Tuscaloosa formation). In addition to the Wilcox species previously mentioned, Engelhardt has described a Tertiary species from Bolivia. Many years ago Unger described a third species from the Middle Miocene of Styria but Schimper considers the latter to be a papilionaceous form. While the fossil record of *Capparis* is so meager such facts as are available would seem to indicate that it originated in the American Upper Cretaceous. Very many of the modern forms are shrubs or small trees of the strand flora and such is believed to have been the habitat of the Wilcox species.

The order Rosales includes about eighteen families[27] with over fourteen thousand existing species, the largest families being those of the Leguminosæ, and the Rosaceæ, Saxifragaceæ and Crassulaceæ. Some members of the alliance are close to the Ranales in their apocarpy, hypogyny and the indefinite repetition of certain floral members, and the order culminates in the relatively modern Papilionaceæ. Five families of Rosales are present in the Wilcox flora. Of these the three leguminous families are by far the most abundant.

The family Hamamelidaceæ consists of about nineteen genera and fifty species. Twelve of the genera are confined to the Asiatic region. One genus is doubtfully confined to Australia: Three genera are African: and three genera are common to Asia and eastern North America. The family is remarkable in containing no less than nine monotypic genera. A consideration of the existing distribution is not only of exceeding interest but also conclusive proof of an extended geologic history, which unfortunately has not yet been unravelled. Since the group is scarcely if at all represented in the existing flora of Australia or in its fossil flora, its present range over Asia would seem to have been accomplished after the land connection with Australia had been interrupted. As the only known Cretaceous fossil forms are from North America there is a probability that the group had its origin in the North American region. The fossil species are not numerous enough, however, for definite conclusions on this point.

The genus *Hamamelis* and its generalized fossil type *Hamamelites* Saporta have five species in the Dakota sandstone, one of which occurs in the Atlantic coast Upper Cretaceous (Middendorf beds of South Carolina) and another is doubtfully represented in the supposed Upper Cretaceous of Argentina (Kurtz). There are two Paleocene species in France and Belgium, and Conwentz has described characteristic flowers preserved in perfection in the Baltic Amber (Sannoisian) as *Hamamelidanthium*.

The genus *Parrotia,* with a single existing species of northern Persia and the Caucasus, has three species in the Dakota sandstone:

[27] The family Platanaceæ, which by the majority of students is referred to the Rosales, I regard as the sole survivor of an independent order, the Platanales, closely related to the Urticales.

one species in the Wilcox and Fort Union: two in the Oligocene of Europe: and two in the Miocene of Spitzbergen, Spain, France, Silesia, Austria, and Hungary. The distribution of *Parrotia* in the past as far as it is known confirms the evidence derived from *Hammamelis* for a North American origin for the family.

The third genus with a geological history is *Liquidambar,* in which upwards of twenty fossil species have been described. The oldest known forms occur in the Eocene at such widely separated points as Alaska, Oregon, Greenland and France. There are two species in the Oligocene of Asia and Europe. There are nine or ten Miocene species represented throughout Europe and North America (New Jersey to Oregon) and in eastern Asia. Three Pliocene species are represented in Spain, France, Italy, Germany, Austria, Styria and Slavonia. Typical fruits preserved in the Upper Pliocene of Germany show how late the genus flourished in central and southern Europe. Felix has described the petrified wood of *Liquidambaroxylon* from the Tertiary of Hungary. The existing *Liquidambar styraciflua* is found in the Pleistocene of West Virginia, North Carolina and Alabama and the eastern Asiatic species *L. formosana* occurs in the Pleistocene of Japan. The genus *Corylopsis* also occurs in the post-Miocene deposits of Japan.

The family *Rosaceæ* includes about 90 genera and over 1,300 existing species, widely distributed and mostly in temperate regions. Some of the genera like *Cratægus* seem to be undergoing saltation at the present time and hundreds of supposed species have been described in the past few years. The tribe Chrysobalanoideæ is confined to the tropics and the Neuradoideæ to the subtropics of Africa and southwestern Asia. All of the other tribes of Rosaceæ are widely distributed and their modern and fossil distribution is without especial significance for the present discussion.

The only genus represented in the Wilcox is *Chrysobalanus* with two species that are evidently the prototypes of the still existing forms. The latter are but two or three in number and as shrubs or small trees they inhabit the sandy shores in the maritime regions of Florida, tropical America and western tropical Africa.

The Leguminosæ as now segregated into 4 families constitutes

the largest alliance among the Choripetalæ (Archichlamydeæ), and next to the Compositæ the largest angiospermous group, with over 9,000 existing species segregated among about 450 genera.

There is a well-defined floral progression from the family Mimosaceæ with its actinomorphic flowers and numerous, usually free, stamens, through the Cæsalpiniaceæ, culminating in the numerically greatest group the Papilionaceæ with its strongly zygomorphic flowers and coalescent stamens, comparable with the like culmination in floral evolution of the Orchidaceæ among the Monocotyledonæ.

The Mimosaceæ, with about 30 genera and 1,400 existing species, are massed in the tropics of both hemispheres. None of the subfamilies are confined to a single continent but comparatively few genera occur in more than two continental areas and half the genera are restricted to a single continent. Asia and Australia each have two peculiar genera, Africa has four and America has seven. America also leads in number of species, about half the total of the family being present in the New World. Australia comes next with over 300, Africa next with upwards of 300 and Asia last with about 100. In the eastern United States there are only three genera and five species, none of which are arborescent. In the Gulf States the numbers have increased to 14 genera and 44 species.

The Cæsalpiniaceæ with about 90 genera and 1,000 species is also mainly tropical with a massing of forms in the American tropics where there are over 600 species and 37 peculiar genera, the subfamily Sclerolobieæ being entirely American and containing numerous monotypic genera. Asia and Africa each have about 150 species. There are, however, only 10 Asiatic genera as compared with 17 African. There are but three Australian genera and less than 100 species. In the eastern United States there are 5 genera and eleven species. Three of the genera, *Cercis, Gleditsia* and *Gymnocladus* are arborescent. In the southern states there are 11 genera and 44 species.

The Papilionaceæ have about 320 genera and 6,600 species. America leads in the number of peculiar genera having 82 while Asia leads in the number of species with about 1,700. Africa has 47 peculiar genera and about 1,600 species. Australia has 38 pecu-

liar genera and about 1,000 species. Asia has 33 peculiar genera, while Europe with 7 peculiar genera and about 700 species is less rich in both species and genera than any other continent. None of the subfamilies are confined to a single contintent but some of the tribes are, the Lipariinæ being South African and the Bossiæinæ being Australian, while 20 of the 27 genera and all but 63 of the 436 species of the subfamily Podolyrieæ are Australian. Two genera in this subfamily are American, 2 African, 1 Asiatic, 1 Mediterranean (Eurasia) and 1 common to North America and Asia.

In the eastern United States there are 46 genera and 194 species of Papilionaceæ, the genera *Cladrastis* and *Robinia* being arborescent. In the southern states there are 55 genera and 318 species. Sargent's "Manual of North American Trees," which includes many tropical forms of the Florida Keys, enumerates for the Leguminosæ as a whole only 34 arborescent species for North America in 17 genera.

In Grisebach's flora of the British West Indies the Leguminosæ outnumber all other families of flowering plants with 262 species. The same is true of Urban's flora of Porto Rico where they number 136 species.

The Leguminosæ found in the Wilcox deposits number over fifty species, many of which are individually abundant. They represent the families Mimosaceæ, Cæsalpiniaceæ and Papilionaceæ, the fourth family of the leguminous alliance, the Krameriaceæ, being a small herbaceous group of the New World of very late, probably of recent, evolution.

Of these fifty-odd Wilcox species eleven are referred to the Mimosaceæ, 26 to the Cæsalpiniaceæ and 20 to the Papilionaceæ. Definitely recognized genera are named in the usual way. Forms usually identified as species of *Acacia* (as for example most of those so named by Heer, Ettingshausen, Unger, etc.) which are referable to the Mimosaceæ but not to the genus *Acacia* as commonly understood are referred to the form-genus *Mimosites*. Forms not certainly identified as *Cæsalpinia* but referable to the Cæsalpiniaceæ are classed under the form-genus *Cæsalpinites* while a considerable number of *Gleditsia*-like forms of both leaves and pods are described in

the genus *Gleditsiophyllum,* a form-genus proposed by me in the first instance for an Upper Cretaceous form from North Carolina. There is a certain unavoidable duplication in the giving of specific names to unattached pods and leaflets since in some cases they may belong to the same botanical species. I have followed this method, however, in all instances where I was not sure of such a relationship.

The Mimosaceæ of the Wilcox are referred to four genera. The genus *Acacia* represented by a single indisputable species in which the leaves are reduced to phyllodes is of great interest since in the existing flora the 450 species are largely confined to the Australian region. The section Phyllodineæ to which the Wilcox species is referred has about 300 existing species which are confined to Australia and Oceanica although in Eocene times they were also present in Europe. It is a curious commentary on the modern character of the earlier Tertiary floras that the reduction of foliar organs and the habit of phyllody, often correlated with modern arid conditions, should have really been developed in these early floras.

The genus *Inga,* represented in the Wilcox by four well marked species, has upwards of 150 species in the existing flora, all of which are confined to the American tropical and subtropical regions. Its geological history is for the most part unknown although it appears to be represented in American Upper Cretaceous floras by *Inga cretacea* Lesquereux which occurs in the Dakota Sandstone and in the Tuscaloosa formation of Alabama. Ettingshausen has described a species from the Cenomanian of Saxony (*Inga Cottai*) and the European Miocene has furnished two or three species, while Engelhardt has described a Tertiary species from Bolivia.

In the genus *Pithecolobium,* which has two Wilcox species and belongs to the same tribe as *Inga* (Ingeæ), while the majority of the 100 or more existing species are American there are over a score in tropical Asia and a few in tropical Australia and Africa. With the exception of a Tertiary species from Bolivia I do not know of other fossil occurrences.

The genus *Mimosites,* with four Wilcox species, represents trees of the *Mimosa* type very abundant in recent species referred to several genera which are either American, Asian, Australian or African,

and abundantly represented in European Tertiary floras. Its Cretaceous ancestry is hidden among the species of leaflets referred to the form-genus *Leguminosites*. The genus *Mimosa* which is apparently most like the Wilcox *Mimosites*, has over 300 existing species and these are for the most part confined to the warmer parts of America, although they are represented in Asia, Africa and Australia.

Except for the family Lauraceæ the Cæsalpiniaceæ with 26 species is the largest family in the Wilcox flora and it is certainly a fact of considerable interest that the massing of the modern species in the American tropics should be foreshadowed by their numerical abundance on this continent as early as the Lower Eocene.

The Wilcox genera are five in number of which the largest is *Cassia* with twelve species. *Cassia* is the largest Wilcox genus except *Ficus*, and all of its species find their modern counterparts in existing species of tropical and subtropical America, many of which are mentioned by name in the systematic part of this work. Numerous as are the Wilcox species of *Cassia* there was apparently greater specific differentiation in contemporaneous European deposits since Ettingshausen records 15 species in the flora of Alum Bay (Ypresian of Isle of Wight). *Cassia* has between three and four hundred existing species found in the warmer temperate and tropical regions of all the continents and especially abundant in tropical America. Their place of origin is unknown since they make their appearance in the Upper Cretaceous almost simultaneously in New Zealand, Australia, Bohemia, Saxony, Greenland, the Atlantic Coastal Plain and the Dakota Group of the Rocky Mountain province. Upwards of one hundred fossil species are already known. Nor does the Eocene distribution shed any light on the early history of the genus since species occur in such widely separated regions as North America, Europe and Australia. There are numerous Oligocene and Miocene species, the Oligocene records being confined to Europe and Africa and the Miocene records being confined to Europe and North America. *Cassia* was abundant along the shores of the Pliocene Mediterranean of Europe and 4 species are recorded from South American beds which are thought to be of Pliocene age. Pleistocene species

are recorded from Maryland and the East Indies (Java) associated in the latter region with *Pithecanthropus erectus* Dubois. One fact is certain, the genus has been a part of the American flora since the dawn of the Upper Cretaceous, and several of the Wilcox species are the undoubted prototypes of existing forms of the American tropics.

The genus *Cercis*, with a single Wilcox species, makes its first recorded appearance in geological history in the Wilcox species, in the three species recorded from the Ft. Union deposits of the Rocky Mountain province and a fourth species found in the Ypresian of the Paris basin, so that its appearance was practically contemporaneous in France and Tennessee. It continues on both continents down to the present being even represented in the Pleistocene of both. The modern species number five or six and inhabit the warmer temperate regions of America, Europe and Asia.

There is one species of *Cæsalpinia* in the Wilcox and it is almost identical in character and habitat with *Cæsalpinia bahamensis* Lamarck of tropical America. The existing species number about two score of the tropics of both hemispheres. *Cæsalpinia* is recorded first from the Upper Cretaceous of the Atlantic Coastal Plain and it seems probable that it originated on this continent and reached Europe during the Eocene by way of the Arctic region, since it is common in the Oligocene, Miocene and Pliocene of the latter continent.

Four Wilcox species are referred to the form-genus *Cæsalpinites*. These represent true forms of *Cæsalpinia* or of allied genera in this family, one almost certainly representing the genus *Parkinsonia*, a small genus which occurs in the European Oligocene but which in the existing flora is confined to the warmer parts of North America and South Africa. Fossil forms referred to *Cæsalpinites* include about twenty of the European Oligocene and Miocene.

The genus *Gleditsiophyllum* makes its appearance in the Upper Cretaceous of the Carolina region. It is represented by eight species of leaves, leaflets and pods, often abundantly preserved, in the Wilcox deposits. Their relation to modern genera is uncertain, although they were evidently much like *Gleditsia*.

Two genera of Cæsalpiniaceæ which I confidently expected to find in the Wilcox and which must have been present during this time in southeastern North America are *Hymenæa* and *Bauhinia*. The former is confined to the American tropics in the existing flora where it has about eight species. It is represented by characteristic forms in the Upper Cretaceous of Alabama. The genus *Bauhinia* which has about 150 existing species of the tropics of both hemispheres has several especially characteristic forms in the Upper Cretaceous of southeastern North America (New Jersey, Maryland, Alabama).

The family Papilionaceæ which comprises over two thirds of the existing Leguminosæ undoubtedly represents the culmination of evolution in the alliance. The bulk of the family, especially the numerous herbaceous genera, are unquestionably of comparatively recent origin. In spite of this fact the family has twenty species in the Wilcox. These are distributed among six genera, of which *Dalbergites, Carpolithus* and *Leguminosites* are form-genera, while the other three are still existing. The largest genus is *Sophora* with seven species, one of which, evidently a strand type similar to and comparable in habitat with the cosmopolitan strand plant *Sophora tomentosa* Linné of the existing tropical flora, is very abundant in the Wilcox deposits. There are about 25 existing species of shrubs and small trees referred to this genus. They are scattered over the warmer parts of both hemispheres and are found on all tropical seashores. About a dozen fossil species are known. In addition to North America they are found in both Europe and Asia during the Eocene, a single form from Alum Bay (Ypresian) being contemporaneous with the Wilcox species and the others being later. While few species have been described the genus is widely distributed in the European Miocene where *Sophora europæa* Unger was a common coastal form of the Mediterranean region throughout the Miocene and into the Pliocene.

Four species, three based on leaflets and the fourth on a characteristic pod, represent the genus *Dalbergia* in the Wilcox flora. Two additional species whose generic relations are not so certain are referred to the genus *Dalbergites*. The existing species of *Dalbergia*

number about eighty forms found in the tropics of both hemispheres, and all show a strong generic similarity in their foliar characters. Over two-score fossil forms are known. The earliest of these occur in the Atlantic Coastal Plain and western Greenland so that there is a strong possibility that the genus was of American origin. If this theory was correct they must have undergone a rapid radiation since in the Eocene they are not only found in America and the Arctic but in Europe and Australia. The Alum Bay beds of the Isle of Wight (Ypresian) which I regard as contemporaneous, in part at least, with the Wilcox, contain according to Ettingshausen, six species of *Dalbergia*. European deposits furnish about a dozen Oligocene species and still more numerous Miocene species. *Dalbergia primæva* Unger, *D. retusæfolia* Heer, *D. hæringiana* Ettingshausen and *D. bella* Heer are widespread coastal forms of the European Tertiary, some of them ranging from the late Oligocene through the Miocene and into the Pliocene.

The genus *Canavalia* is represented in the Wilcox by a fine species undoubtedly ancestral to the existing *Canavalia obtusifolia* (Lamarck) De Candolle, a widely distributed tropical strand plant. A second species is less commonly represented and not as certainly identified. The genus contains about a dozen existing species of the tropics of both hemispheres but has not been heretofore found in the fossil state.

The Wilcox forms referred to *Leguminosites* cannot be dealt with satisfactorily since they represent pods and leaflets of this alliance whose generic relations are uncertain. The form-genus was proposed first by Bowerbank for the pyritized remains from the Island of Sheppey (London Clay), and two of his species are tentatively identified in the Wilcox. Subsequently many species have been described. They range in age from the Middle Cretaceous to the Pliocene. Saporta describes the oldest form in the Albian of Portugal. They are present in the Cretaceous of Australia, the Cenomanian of Saxony, the Atane and Patoot beds of Greenland, and the Atlantic Coastal Plain Cretaceous from Marthas Vineyard to Alabama. They are common in the Arctic Eocene, occurring also in Australia, America, Europe and Asia. Oligocene records in-

clude Europe and Antarctica, Miocene records are confined to America and Europe and Pliocene records include southern Europe and Japan.

While the foregoing analysis leaves a great many points in the history of the Leguminosæ unsolved it serves at least to show that the Wilcox forms are all represented and would find a congenial habitat in the present day American tropics and that thus early some of the main features of their present day development had been differentiated.

The most similar fossil display of these forms is to be found in the Ypresian flora of Alum Bay on the Isle of Wight, which unfortunately have never been described or figured, but of which Ettingshausen[28] published an analysis and enumeration in 1880. Another very similar display of forms is that described by Engelhardt from the Tertiary of Cerro de Potosi in Bolivia.[29] The exact age of the latter has never been determined although its resemblance to this part of the Wilcox flora suggests the possibility that it is Eocene instead of Pliocene, which later has been assumed to be its age. This may, however, simply be a reflection of the similarity between the Leguminosæ of the Embayment area in the Lower Eocene and those of subsequent periods in the American tropics.

The order Geraniales includes 21 families, with upwards of ten thousand existing species, of which nearly one half belong to the family Euphorbiaceæ. The other large families in the order of their size are the Rutaceæ, Meliaceæ, Malpighiaceæ, and Polygalaceæ each with over five hundred existing species, while the Geraniaceæ, Oxalidaceæ, and Burseraceæ each have over three hundred existing species. The alliance is mainly cyclic in the character of its floral members, starting with isocarpic forms and progressing in the direction of reduction in the number of carpels. The phylogenetic importance of the characters by which the Geraniales as an order is separated from the evidently allied Sapindales is not great and in some respects the order is apparently not a natural one. Six families of Geraniales have been recognized in the Wilcox flora. The

[28] Ettingshausen, *Proc. Roy. Soc. Lond.*, Vol. 30, 1880, pp. 228–236.

[29] Engelhardt, *Sitz. naturwiss. Gesell. Isis in Dresden*, 1887, Abh. 5, pp. 36–38, 7 Figs.; *Ibid.*, 1894, Abh. 1, pp. 3–13, Pl. 1.

first of these, the Rutaceæ, consists of about 111 genera and over 900 existing species widely distributed over the warm temperate and tropical regions of the earth. The fruits are capsules, samaras or drupes and the leaves which may be simple or compound are usually glandular punctate. While there are 34 genera with 127 species confined to America the family makes its greatest display in the Old World, Africa having 16 peculiar genera with 196 species and Australia 28 peculiar genera with 185 species. In addition to 6 genera with 7 species confined to the Asiatic Mainland there are 19 genera with 167 species found distributed from southeastern Asia through Malaysia greater or less distances, in some cases to New Zealand and Polynesia. The only truly cosmopolitan genus is *Fagara* with upwards of 150 existing species and represented in all tropical countries. The tribe Boronieæ with 18 genera and 158 species is confined to Australia and New Zealand; the Diosineæ with 11 genera and 181 species is confined to South Africa; and the Cusparieæ with 16 genera and 83 species is confined to tropical America. All of the other rather numerous tribes are represented in more than one continental region.

The family contains the remarkable number of 42 monotypic genera and while many of these may be regarded as of recent evolution, as for example a number of those of Australia, the isolated occurrences of many of the others indicates that they are of great age and once occupied intervening areas.

There are only twelve known fossil genera, or only about 10 per cent. of the existing genera, so that little can be said of the fossil history of the family. The oldest genus is *Citrophyllum* Berry represented by very characteristic leaves with alate petioles found in the Dakota sandstone of the Rocky Mountain province and from New Jersey to Alabama along the Atlantic coast in the Raritan, Magothy, Middendorf and Tuscaloosa formations. There is a second species of *Citrophyllum* in the Wilcox and a third in the overlying Claiborne. These forms are very similar to the leaves of recent members of the Aurantioideæ and undoubtedly represent ancestral forms. The genus *Dictamnus* Linné with a single existing species widely distributed in Eurasia, has furnished a fossil form in the Pliocene of France and a second in the Pleistocene of Japan. Unge

in 1850 described petrified wood from the Aquitanian of Greece as *Klippsteinia medullaris* referring it to the Aurantioideæ.

The genus *Amyris* (P. Browne) Linné has about a dozen existing species in the Antilles and Central America, two of which reach the coast of southern Florida. A fossil form is recorded by Unger from the late Miocene (Sarmatian) of Hungary. This determination is not conclusive however although Unger had both the leaves and fruit of *Protamyris berenices.* Unger also described the supposed ancestral genus *Protamyris* to which he referred four species from the Aquitanian of Kumi and the Miocene of Croatia. These are not especially convincing and both Ettingshausen and Schenk consider *Protamyris radobojana* Unger to represent a species of *Cedrela.*

The genus *Xanthoxylum* Linné with nine or ten existing species of eastern Asia and North America has been a favorite receptacle for fossil forms of Rutaceæ. About a score of species have been described, the oldest coming from the basal Eocene of New Mexico (Raton formation) while a second Eocene species is recorded from the Bartonian of France. Engelhardt has described two Eocene or Oligocene species from Chili. There are four Oligocene species, two in France and two in Prussia. There are about thirteen Miocene species, widely distributed and represented in California, Colorado, Spain, France, Switzerland, Baden, Bohemia, Croatia and Hungary. The two Pliocene species represent France and Asia Minor and one of the recent species is found in the Pleistocene of Japan. It seems probable that *Xanthoxylum* was derived from *Fagara* through a loss of the floral calyx and by adaptation to less tropical climatic conditions.

The genus *Fagara* Linné is substituted for *Xanthoxylum* by many recent systematists, although I prefer to consider it as the ancestral stock and in the older sense as including the 150 cosmopolitan tropical species while *Xanthoxylum* includes the extratropical forms of Asia and North America. Undoubtedly several if not all of the fossil forms described as species of *Xanthoxylum* are more properly referred to *Fagara* although none have heretofore been described under this name. The Tertiary flora of southeastern North America contains several very characteristic forms of this genus. The oldest

are three species from the Wilcox Group. There is another in the overlying Claiborne Group. The Vicksburg Group has furnished a very common form with several well-marked varieties some of the leaves of which show their glandular punctate character beautifully preserved. Still another form is found in the Apalachicola Group of Florida.

The genus *Ruta* Linné with upwards of 100 existing species mostly of Eurasia although present in Africa and South America, has furnished Menzel (1913) with characteristic capsules in the Aquitanian of Rhenish Prussia.

The genus *Phellodendron* Rupr. with two existing Asiatic species is represented in the Aquitanian of Rhenish Prussia by fruits (drupe). Engelhardt has described species of *Ticorea, Pilocarpus* and *Erythrochyton* from the early Tertiary of Chili.

The remaining genus with fossil representation is *Ptelea* Linné which has 7 or 8 existing species[30] confined to the United States and Mexico. The fossil forms are represented by both leaves and characteristic fruits. The oldest comes from the Arctic Eocene. There is a species in the Oligocene of Italy and six Miocene species, occuring in Colorado, France, Switzerland, Carniola and Hungary. A Pliocene species is recorded from Italy. Obviously the record will have to become much less fragmentary before any creditable conclusions can be drawn respecting the place of origin and geologic history of the Rutaceæ.

The family Simarubaceæ (often spelled Simaroubaceæ) includes about 28 genera and upwards of 150 existing species of shrubs or trees with pinnate leaves and drupaceous fruits, confined chiefly to the tropics and the warmer parts of the northern hemisphere. Only three of the existing species reach as far northward as the coast of southern Florida. The family is still represented on all the continents except Europe. Two genera with four species are confined to Asia; 3 genera with 4 species are confined to Australia; 4 genera with 6 species are confined to Africa and 9 genera with 71 species are confined to America. The most widespread species is the monotypic *Suriana maritima* Linné a cosmopolitan tropical strand plant occurring on the dunes, keys and coastal hammocks of southern Florida.

[30] Greene has recently described very many poorly established new species.

The only genus represented in the Wilcox is *Simaruba* Aublet, which has furnished a single species, *Simaruba eocenica* Berry, closely resembling the existing *Simaruba glauca* De Candolle which is found along tropical coasts from southern Florida to Brazil.

The only other genus with a geological history is *Ailanthus* Desf. which has 7 existing species of eastern Asia and the East Indies.[31] The fossil species number about fifteen. There are two in the Eocene of Wyoming and Oregon; eight in the Oligocene of France, Alsace, Styria and Prussia; and five in the Miocene of France, Switzerland, Baden, Italy and Colorado. While in the absence of collateral evidence that the Eocene occurrences in North America have any significance regarding the origin of the genus it is an interesting speculation that the genus originated in North America and subsequently reached Asia by way of the Eocene land connection across Behring straits. Certainly the genus lingered on this continent, as is evidenced by its presence at Florissant, as late as the Middle Miocene.

The family Meliaceæ contains about 42 genera and about 680 existing species of shrubs and trees with pinnate leaves. The vast majority are found within 30 degrees of the equator although they reach 40° north in eastern Asia and 40° south in New Zealand. Moreover the China berry (*Melia azedarach*) has been cultivated from time immemorial in all Mediterranean countries, and throughout the southeastern United States since its settlement, and is perfectly hardy. There are no temperate outliers however. While the Meliaceæ occupy a greater continuous area in South America where over 41 per cent. of the existing species occur, this large number of species (about 285) represent only 19 per cent. of the known genera. There are some remarkable similarities between the species of the American tropics and those of West Africa. Thus the two small genera *Swietenia* and *Carapa* are represented in both areas and *Carapa procera* is even said to be common to the two. Moreover the genus *Guarea* which has about 80 species in the American tropics has three in West Africa. The larger number of genera are found in the S. E. Asiatic region and the number of genera and their mutual

[31] *Alianthophyllum* Dawson with a single species is described from the Eocene (?) of British Columbia.

affinities decrease from Asia toward Africa and also through Polynesia. A number of genera (*Toona, Xylocarpus, Cipadessa, Melia*) extend from Africa through Asia to Malaysia. Two genera are peculiar to Australia (*Synoum, Owenia*) and two to Polynesia (*Vaveæ, Meliadelpga*). There are thirteen monotypic genera of which six are African and seven Asiatic. From the distribution of the existing species De Candolle[82] infers that southern Asia is the center of radiation of the family. I am inclined to think however that the reverse is probably true since the oldest known forms, except the entirely doubtful Cedrelospermites of Saporta from the Valanginian of Portugal, are American, and the widespread existing American representation of the family seems to comprise the specifically multiplied descendents of the original stock already represented in the Wilcox flora.

The Asiatic genera would represent immigrants into that area or forms evolved there. The Polynesian and Australian forms are much localized derivatives of the Indian stock and unless the peculiar species of New Caledonia could not reach that region except by a land connection it may be inferred that this Asiatic radiation was relatively recent.

The fossil species are unfortunately few in number. So far as I know the only fossil species of *Carapa* is that found in the Wilcox. Its occurrence in the early Eocene is at least a factor in explaining its present distribution in both the American and West African tropics. The fact that *Carapa procera* is common to these two areas may suggest that all of the African species are recent immigrants, but it is more probable that there are unrecognized specific differences in this form in the two areas and that the present disconnected distribution is an example of survivors from the early Tertiary radiation. Another genus with a modern distribution like Carapa is the genus *Moschoxylon* Jussieu (made a section of *Trichilia* Linné by Harms in Engler and Prantl) which has about 60 species of tropical America and West Africa. This has furnished two fossil species described by Engelhardt from the early Tertiary (Eocene or Oligocene) of Chili. The genus *Cedrela*, sometimes made the type of an

[82] De Candolle, C. de, "On the Geographical Distribution of the Meliaceæ," *Trans. Linn. Soc. Lond.*, 2 ser., Bot., Vol. 1, 1880, pp. 233–236, Pl. 30, 31.

independent family, the Cedrelaceæ, has four Wilcox species, Eocene prototypes of existing American species. This genus with 9 or 10 species is confined to America in the existing flora and is only known outside this area in two species from the Miocene of Croatia which Unger referred to *Cedrela* and an undescribed *Cedrela* recorded by Ettingshausen from the Ypresian of the south of England. Saporta has, however, recorded six species of *Cedrelospermum* from the Sannoisian of southeastern France. The fossil record of these three genera *Carapa, Moschoxylon* and *Cedrela,* brief as it is, shows clearly that the Meliaceæ are not a modern element in the flora of the American tropics but one that was already well differentiated in the early Tertiary.

The remaining fossil references to this family comprise *Meliaceæcarpum* based on capsules from the Aquitanian of Prussia which Menzel their describer compares with those of the genera *Dysoxylum* and *Guarea.* F. von Müller has described *Rhytidotheca* and *Pleioclinus,* two supposed meliaceous genera based on fruits, from the Pliocene of Australia.

The family Humiriaceæ is a small one, comprising only three genera and a score of species of shrubs and small trees all of which are confined to the American tropics except a single species found in tropical West Africa, a distribution suggesting a history comparable with that just suggested for *Carapa, Moschoxylon* and *Cedrela.* The only known fossil species is one from the Wilcox very close to the existing *Vantanea paniculata* Urban of northern South America.

The family Malpighiaceæ, confined to tropical and subtropical countries, contains about 55 genera and 650 existing species, many of which are scandent, including some of the finest lianas of the tropics with stems 2 dcm. in diameter. Others are shrubs and trees. The leaves are opposite and simple and the fruits drupaceous, capsular, or nutlike, and often winged. The only species that reaches the United States is *Byrsonima lucida* (Swartz) De Candolle, a small evergreen tree of the Florida keys.

The family is predominantly American in its distribution, over 67 per cent. of both genera and species being confined to the Western Hemisphere (37 genera and 440 species). The genera are all local in the sense that none occur in more than one continental area.

Of the two subfamilies into which the family is divided—the Pyramidotoræ and Planirotæ, the latter with two tribes Galphimieæ and Malphighieæ are entirely American. Of the three tribes into which the Pyramidotoræ is divided the Tricomarieæ are entirely American, the Hiræeæ have 3 genera and 23 species confined to Asia, 3 genera and 12 species confined to Africa, a genus with 12 species ranging from Malayasia to Australia, and 9 genera with 151 species confined to America. The remaining tribe, the Banisterieæ, has a monotypic genus in Asia, 2 genera and 15 species in Africa, a single genus with 7 species ranging from the East Indies to Australia and 11 genera with 247 species confined to America.

There are 21 monotypic genera distributed as follows: *Microsteira* confined to Madagascar: *Flabellaria* confined to Africa: *Caucanthus* confined to Arabia: *Brachylophon* confined to farther India: *Mezia, Diplopteris, Lophopteris, Clonodia, Coleostachys, Blepharandra, Lophanthera, Verrucalaria, Pterandra, Acmanthera, Diacidia,* and *Glandonia* confined to Brazil, Guiana and Venezuela: *Henleophytum* confined to Cuba: *Lasiocarpus* and *Echinopteris* confined to Mexico: and *Tricomaria* and *Mionandra* confined to Argentina.

Monotypic genera in general are susceptible of two interpretations, i. e., they represent either the last survivors of a long line as in the case of the *Ginkgo, Sassafras,* etc., or they represent relatively recent specializations. In the case of the foregoing monotypic genera it seems probable that the majority are the result of relatively recent evolution since there is nothing in their character or distribution to suggest any extended geologic history and none have been found in fossil floras.

The fossil record is most incomplete. No forms are known from the Upper Cretaceous for although Ettingshausen recorded a species of *Malpighiastrum* and one of *Banisteriophyllum* from the Upper Cretaceous of Australia, those identifications are open to the most serious question and I do not consider them of any weight in a discussion of this kind. The family is certainly represented in the lower Eocene by five species of *Malpighiastrum, Hiræa* and *Banisteria* in the Ypresian of the south of England and by five species of *Hiræa* and *Banisteria* in the Wilcox flora, based upon both leaves and characteristic fruits. There are also doubtful species of *Malpighias-*

trum and *Banisteriophyllum* described from the Eocene of Australia by Ettingshausen. Thus there is no direct geologic evidence of the place of origin of the family. The fact that it is so predominantly American at the present time and that only two genera have reached Australia from the East Indian region and that two of the American genera appear in the northward extension of the early Eocene flora of the American tropics during the Wilcox and are as ancient as any certain records of the family anywhere, renders the conclusion that the family originated in equatorial America an extremely probable one. With the exception of the Wilcox records enumerated above nearly all of the fossil records relate to Europe, and these may be briefly enumerated.

The genus *Malpighiastrum* of Unger has about 30 recorded species. These include the doubtful Upper Cretaceous and Eocene species previously mentioned as recorded by Ettingshausen from eastern Australia; 3 Ypresian species from the south of England; eight Oligocene species in France, Italy, Dalmatia, Styria and Transylvania; about 15 Miocene species in Italy, Prussia, Bohemia, Croatia and Transylvania; and two Pliocene species in Italy.

The genus *Heteropteris* Jussieu, with about 90 existing species ranging from Mexico and the Antilles to Bolivia and Brazil, has a late Oligocene species in Transylvania and two Miocene species in Styria and Croatia.

The genus *Hiræa* Jacq., with about 25 existing species ranging from Mexico and the Antilles to Peru, has furnished about ten fossil species, based for the most part on the winged fruits. There is a species in the Ypresian of southern England and a characteristic fruit in the Wilcox; four Oligocene species in the Tyrol, Styria and Transylvania; three Miocene species in Baden, Styria and Transylvania; and a Pliocene species in Brazil.

The genus *Tetrapteris* Cav., with about 60 existing species ranging from the West Indies and Mexico to southern Brazil and Bolivia, has furnished a fossil species in the Oligocene of Styria and three Miocene species in Bohemia, Styria and Croatia.

The genus *Stigmatophyllon* Jussieu, with about 45 existing species found in the Bahamas and Antilles and along the east coast of America from Mexico to Uruguay, has furnished Saporta with a

somewhat doubtful form from the Upper Oligocene of France. Similarly the genus *Byrsonima* L. C. Rich., with 90 existing species ranging from the Bahamas and Mexico to southern Brazil and Bolivia has been recorded by Massalongo from the early Pliocene of Italy, but the identification is extremely doubtful.

The genus *Banisteria* Linné contains about 70 existing species of climbing or scrambling shrubs ranging from the West Indies throughout tropical South America. It is represented by four species based upon both leaves and fruits in the Wilcox; there is a Ypresian species in the south of England; four Oligocene species in France, the Tyrol, Alsace and Styria; and four Miocene species in France, Switzerland and Croatia.

The genus *Banisteriophyllum* Ettingshausen with a single Upper Cretaceous and an Eocene species in eastern Australia I regard as of very doubtful affinities. Schenk also states that wood of a malpighiaceous type occurs among the silicified woods from the Oligocene of the Island of Antigua.

The family Euphorbiaceæ is sometimes made the type of a distinct order, the Euphorbiales, although the significance of the characters by which it is segregated from the Geraniales is not obvious. It is an exceedingly large alliance with about 220 genera and 4,000 existing species (Pax, 1890) of herbs, shrubs and trees, widely distributed throughout the tropical and temperate zones. The genus *Euphorbia* with over 700 species is perhaps the most widely distributed genus in the family. A very large number of the recent species, particularly those of xerophytic character so closely simulating the Cactaceæ, are of relatively recent evolution.

In such a multiplicity of existing genera and species any effort to trace the larger features of distribution would occupy more space than it is worth in the present connection. Four arborescent genera with five species reach the United States in the Florida region, and several additional are naturalized in that area. A considerable, but relatively insignificant, number are recorded during the Upper Cretaceous and Tertiary. The fossil records will, however, have to be greatly increased before they can be said to shed any definite light on the geological history of the family. Enough is now known,

however, to abrogate the statement made by Schenk[33] and quoted by Pax[34] that there is no certain evidence of the existence of the Euphorbiaceæ during the Tertiary. The following genera have been recorded as represented in the fossil state.

Euphorbia with a single species based upon a fruit described by Heer from the Swiss Miocene; *Euphorbioides* based on an inflorescence described by Wessel and Weber from the Aquitanian of Rhenish Prussia; the genus *Euphorbiophyllum* with several species to be noted presently; I have described a very characteristic species of *Manihotites* from the Upper Cretaceous of Georgia; the genus *Crotonophyllum* has several Upper Cretaceous and Eocene species; *Cluytia* is reported from the Eocene of the Isle of Wight and the Oligocene of Saxony and Rhenish Prussia; the following genera each with a single species were identified by Ettingshausen from the Miocene of Bohemia, *i. e., Adenopeltis, Baloghia, Omalanthus* and *Phyllanthus*. Conwenz has described a euphorbiaceous flower from the Baltic Amber (Sannoisian) as *Antidesma maximowiczii* and Felix has described petrified wood from the Tertiary of the U. S. of Columbia as *Euphorbioxylon*. *Hura*-like fruits are also recorded by Knowlton from the lower Eocene (Raton formation) of New Mexico. Engelhardt has recorded species of *Omphalea* Linné, *Tetraplandra Baillon* and *Mallotus* Lour., from the early Tertiary of Chili.

While difference of opinion regarding the determination of some of these records is justifiable I regard *Manihotites, Euphorbiophyllum, Crotonophyllum* and *Euphorbioxylon* as definite evidence of the existence of the Euphorbiaceæ during the Upper Cretaceous and Tertiary.

The Wilcox species are five in number and are referred to the genera *Crotonophyllum, Euphorbiophyllum* and *Drypetes*. The genus *Crotonophyllum* was proposed by Velenovsky for a well-marked species from the Cenomanian of Bohemia. I have described a second species from the Upper Cretaceous of South Carolina. Two species are recognized in the Wilcox and of these *Crotonophyllum*

[33] Schenk, "Palæophytologie," pp. 594–597, 1890.

[34] Pax, in Engler and Prantl's "Naturlichen Pflanzenfamilien," 1890.

eocenicum Berry may be successfully compared with a number of the six hundred existing species of *Croton* which is so abundantly represented in tropical America. Comparisons are especially close with *Croton eluteria* (Linné) Bennett which is found in the low coppice of the beach ridges throughout the Bahama islands.

The genus *Euphorbiophyllum* was proposed by Ettingshausen in 1853 for several species from the Sannoisian of the Tyrol. Altogether over a dozen species have been described by Ettingshausen, Saporta and Engelhardt. These have been compared with the existing, mostly tropical American, species of *Styloceras, Sapium, Stillingia, Adenopeltis, Exoecaria, Colliquaja,* etc. The oldest comes from the Cenomanian of Portugal and a second Upper Cretaceous species occurs in the Turonian of southern France. In the Eocene there is a species in West Greenland, a second on the Island of Sheppey (Ypresian) and a third in the Paris basin (Lutetian). Five Oligocene species have been described from the Sannoisian of the Tyrol, and a sixth from the Chattian of northern Bohemia. There are two Miocene species in Switzerland and two in Styria: a Pliocene species is described by Krasser from Brazil. A single small-leafed species of *Euphorbiophyllum* is of rare occurrence in the middle Wilcox.

The genus *Drypetes* Vahl. has about a dozen existing species confined to tropical and subtropical America. Three extend southward to northern Brazil and two range northward to the Florida keys. There are two well-marked species in the Wilcox flora—one an Eocene prototype of the existing *Drypetes keyensis* Urban, and the other of the existing *Drypetes lateriflora* (Swartz) Urban, both small trees of the coastal flora of southern peninsular Florida, the Bahamas, West Indies and Antilles. The genus, which has not previously been recorded in the fossil state, was probably of American origin and there is no evidence that it ever spread to the eastern hemisphere.

The order Sapindales, sometimes called the Celastrales, includes some twenty families, together containing about 3,200 species, the largest families in the order of their size being the Sapindaceæ which has more than twice as many species as any of the others; the Celastraceæ, Anacardiaceæ, Balsaminaceæ, and Ilicaceæ. As in the preceding order, the Sapindales start with isocarpic forms and pass to

those in which the carpels are reduced in number, and in the more evolved families the flowers have become zygomorphic. Since there are several distinct lines of development and the separation from the Geraniales is based on characters that seem trivial, it seems probable that the families comprising these two orders as at present understood represent a plexus of forms whose filiations are not yet understood.

The first family of the Sapindales that is represented in the Wilcox flora is the Anacardiaceæ, an exceedingly natural group. It contains about 58 existing genera and 435 species of shrubs and trees with round pithy branches, resinous and frequently toxic juice, alternate, simple, palmate or pinnate, exstipulate leaves, and drupaceous fruits with exalbuminous seeds. The Anacardiaceæ makes its greatest display in the tropics and subtropics of both hemispheres but in the existing flora is especially characteristic of the Malaysian region. *Rhus* is by far the largest genus and the only one of the family found in the extra tropical regions of both the northern and southern hemispheres. The present geographical distribution shows many anomalies throughout the family. Thus the genus *Campo-sperma* Thwaites has eight species in Madagascar, Ceylon, Sumatra, Borneo and the Malaccas and a single species in northern Brazil. The genus *Sorindeia* Thouars of tropical Africa and Madagascar is most closely allied to the genus *Mauria* Kunth of the Andes of South America. The genus *Calesium* Adanson has 13 species in tropical Africa and one in the East Indies. The Eurasian genus *Pistacia* Linné has a single species in Mexico. The genus *Thyrsodium* Bentham has 4 species in the Amazon region of South America and one in tropical West Africa. The subfamily Mangiferæ with about 80 species is entirely Malaysian except for a species of *Gluta* Linné in Madagascar and the genus *Anacardium* Linné which is confined to tropical South America, chiefly in Brazil. The subfamily Spondieæ is found in the tropics of all the continents, excepting Europe. The subfamily Rhoideæ is found on all the continents and shows a pairing of a considerable number of genera in equatorial Africa and America. The two remaining subfamilies, the Semecarpeæ and the Dobineeæ are restricted to the region extending from India to Australia. The

family contains twenty monotypic genera distributed as follows: Asia 5, Australia 3, Africa 6, Madagascar 3, North America 2, and South America 1.

The fossil records of the Anacardiaceæ are very incomplete although there seems to be no doubt that it was represented in both Europe and North America as far back as the Upper Cretaceous. As in the existing flora the most abundant genus in the fossil record is *Rhus* to which over one hundred species have been referred. Eight of these are Upper Cretaceous forms the oldest coming from North American strata correlated with the Cenomanian (Raritan, Dakota). The genus appears in Europe in the Turonian of Bohemia. There are over a dozen Eocene species of *Rhus*, widely scattered. Thus there are three in the Ypresian of Alum Bay, four in West Greenland and North American species in the Lance, Kenai, Ft. Union and Green River formations. The genus doubles its known species in the early Oligocene, being especially well represented in southern France, but also recorded from the Tyrol, the Baltic amber, Italy, Carniola and Styria.

In the Miocene *Rhus* seems to have been as abundant, as well differentiated, and as widely distributed as it is in the existing flora, for over sixty fossil species have already been described. The records embrace all European countries where Miocene plants have been found as well as Iceland and the following North American localities:—Maryland, Virginia, Colorado, Yellowstone Park, Idaho, Nevada, Oregon and California. Only a small number of Pliocene species are known and these are recorded in Spain, France, Italy, Germany and Slavonia.

Three Pleistocene species are recorded, 2 from Japan and one from China, all closely related to still existing species of that region. Engler[85] some years ago reviewed the geological records of *Rhus* and concluded that most of the then known fossil species belonged to the section Trichocarpæ (in the existing flora with over a score of species mostly confined to North America and eastern Asia), or the section Gerontogeæ (with 75 existing species mostly confined to South Africa). A few fossil forms he considered as representing the section Venenatæ, which has about 14 existing species in North

[85] Engler, A., *Bot. Jahrb.*, Bd. 1, 1881, pp. 413–419.

and South America. The other sections into which the genus is subdivided were not recognized among the fossil forms.

The allied genus *Cotinus* with two or three existing species in Eurasia and North America is probably represented by some of the fossil forms referred to *Rhus, e. g.,* Saporta considers *Rhus antilopum* Unger from the Aquitanian of Kumi as a species of *Cotinus.* This author has also described *Cotinus palæocotinus,* and Cockerell has described *Cotinus fraterna* from the Miocene of Florissant, Colorado.

The genus *Pistacia* with five existing Mediterannean species and one each in eastern Asia and Mexico has about fifteen known fossil species the oldest, of doubtful value, coming from the Raritan of Staten Island. A second Cretaceous species is found in the Laramie of Colorado. Europe enters the record with a Ypresian species from Alum Bay. There are three Oligocene species in France and seven Miocene species in France, Bohemia, Styria, Galicia, and Transylvania. There is a Pliocene species in Styria, an extinct Pleistocene species on the Island of Madeira, and the existing *Pistacia lentiscus* Linné in the Pleistocene of the Island of Santorin.

The genus *Anacardites* Saporta (*Anacardiophyllum*) has been used as a form-genus for fossil Anacardiaceæ of uncertain generic relationship. As used by Saporta it represented fossil forms resembling existing species of *Mangifera, Anaphrenium, Spondias, Comocladia, Holigarna,* etc., but not determinable with certainty. Heer has described a supposed species of *Anacardites* from the Atane beds of West Greenland. There are two species in the Sparnacian and one in the Ypresian of France and seven well marked species in the Wilcox. There are two or three Oligocene species in France and Germany and two or three Miocene species in France and Styria. Felix has described petrified wood from the Eocene of the Caucasus which he refers to *Anacardioxylon,* a type also represented in the Oligocene of Antigua in the American tropics (species compared with existing genus *Spondias*).

The floral genus *Heterocalyx* Saporta (*Trilobium* Saporta, *Elaphrium* Unger, *Getonia* Unger) which occurs at a number of horizons in the Oligocene of France, Croatia, and Styria is represented by a second species in the Wilcox. Saporta compared it with the South

American genus *Astronium* but Engler (op. cit.) considers it most like the Malayan genus *Parishia*.

The genus *Metopium*, not certainly recognized heretofore, has a well-marked species in the Wilcox. Several Tertiary woods are described by Unger as *Rhoidium* and Saporta has described a species of *Schinus* from the French Oligocene (Gargas) which is wrongly determined according to Schenk (p. 541).

The genus *Spondiæcarpum* has a species in the early Eocene of France and a second in the Aquitanian of Rhenish Prussia. Recently Fritel has described leaves from the Aquitanian of France which he calls *Semecarpites* that are very close to the existing genus *Semecarpus* which has about 40 species ranging from India to Australia.

The family Ilicaceæ (Aquifoliaceæ) is a relatively small one comprising only five genera and about 180 existing species. They are shrubs or trees with alternate, simple, entire or toothed, often coriaceous leaves. The flowers are small, dioecious and hypogynous. The fruit is a drupe with a thin fleshy sarcocarp enclosing as many crustaceous nutlets as there are carpels. The genus *Ilex* Linné to which all but seven of the existing species are referred is found in all tropical and temperate regions of the world except western North America, Australia, New Zealand and New Guinea. The remaining genera of the family are *Oncotheca* Baillon with a single species in New Caledonia, *Nemopanthes* Rafinesque with a single species in temperate North America, *Sphenostemon* Baillon with two species in New Caledonia, and *Byronia* Endlicher with three species, one in Tahiti, one in the Hawaiian Islands and one in Australia. This modern distribution is a certain indication that the family has an extended geologic history.

Over a hundred fossil species have been referred to the genus *Ilex*. At least thirteen species are recorded from the Upper Cretaceous. All but one from the Turonian of Bohemia are from the western hemisphere and include two in the Raritan formation, three in the Magothy formation, seven in the Dakota sandstone, one in the Atane and two in the Patoot beds of western Greenland.

There are about fourteen Eocene species including four in the Wilcox, one in the Ypresian of England, one in the Fort Union, four in the Green River beds, five in Greenland, one in Alaska and one in

Australia. There are over a score of Oligocene species including one from Chili that may even be of Eocene age. The lower Oligocene or Sannoisian has eleven species in France, Tyrol, Saxony and Prussia and includes three species of flowers described by Caspary from the Baltic amber. The Middle Oligocene or Tongrian has six species in France, Italy, Germany and Styria and there are seven species in the Upper Oligocene (Chattian) of France, Bohemia and Greece. Upwards of fifty species have been described from the Miocene of Europe and Asia, and from New Jersey, Colorado and California in this country. The most prolific Miocene area is that of France. About ten species are known from the Pliocene of Spain, France, Italy, Prussia and Asia Minor. One fossil and four recent species are found in the Pleistocene of Virginia, North Carolina, Alabama, Kentucky and the Island of Madeira. In addition to the fossil forms referred to *Ilex,* two Miocene species from Italy and Styria are referred to the genus *Nemopanthes* and four forms from the late Oligocene or the Miocene of Prussia, Styria, Croatia, Bohemia and Greece are referred to the genus *Prinos* Linné, which is usually considered a section of *Ilex.* The four species from the Wilcox that are referred to *Ilex* are represented in the collections by a small amount of mostly poor material and are without special significance.

The family Celastraceæ includes about 40 genera and upwards of 400 existing species of trees and shrubs with opposite or alternate, simple, persistent or deciduous leaves and capsular or drupaceous fruits. The three large genera *Euonymus, Celastrus* and *Gymnosporia* are practically cosmopolitan and several additional genera localized in the modern flora were cosmopolitan in the Tertiary.

The following 12 genera with over 100 species are confined to America: *Fraunhofera, Mortonia, Glossopetalum, Schaefferia, Goupia, Maytenus, Pachystima, Zinowiewia, Plenckia, Wimmeria, Gyminda, Rhacoma.* The genera *Glyptopetalum* and *Tripterygium* together with five species are confined to Asia. The genera *Hypsophila, Denhamia* and *Hedraianthera* together with seven species are confined to Australia. And the following ten genera with about 60 species are confined to Africa or Madagascar: *Putterlickia, Catha, Pterocelastrus, Polycardia, Ptelidium, Cassine, Elæodendron, Maurocenia, Schrebera* and *Lauridia.*

The family is definitely represented in the Cretaceous by at least five genera and is an important element in most Tertiary floras. The oldest known genus is the form-genus *Celastrophyllum* proposed by Gœppert. Five well-marked species occur in the Patapsco formation (Albian) of Virigina and Maryland. At the base of the Upper Cretaceous, particularly in North America, a large number of species occur. Upward of thirty have been described of which number two are recorded from New Zealand and two from the Cenomanian of Niederschœna in Saxony. There is a species in the Atane beds of Greenland and three in the Patoot beds. The remainder occur in the United States and have the following distribution: ten in the Raritan formation of New Jersey and Maryland, twelve in the Tuscaloosa formation of Alabama, two in the Magothy formation of New Jersey and Maryland, two in the Middendorf beds of South Carolina, seven in the Dakota sandstone, and two in the Black Creek formation of North Carolina. There are ten Eocene species—seven in the basal Eocene of Belgium, one in the Ypresian of England and two in the Claiborne group of the Mississippi embayment. There are five Miocene species in Italy, Bohemia and Styria; a Pliocene species in Italy; and four Tertiary species from the Island of Java. Another form-genus is *Celastrinites* Saporta which has four species in the Paleocene of France, one in the Denver formation of Colorado, another in the Livingston formation of Montana, and a seventh in the Miocene of Florissant, Colorado.

The genus *Celastrus* Linné is the largest fossil genus of the family and its history shows that while its present center of distribution is in the uplands of southeastern Asia and the East Indies the ancestral stock was cosmopolitan and very abundant in the Tertiary of America and Europe, with a strong probability that it originated in the former area at the dawn of the Upper Cretaceous or somewhat earlier. The oldest known species, *Celastrus arctica* Heer, is found in the Raritan and Magothy formations of New Jersey and Maryland and in the Patoot beds of Greenland. No less than thirty species of *Celastrus* have been described from the Eocene. These include six Ypresian species from England, five species in the Wilcox flora, one in the Denver, ten in the Fort Union, one in the Kenai of Alaska, three from Greenland and four from Australia.

There are also about thirty Oligocene species, all European, and including remains in the Baltic amber, in France, Switzerland, Germany, Austria-Hungary and Greece. There are at least a dozen species in the Chattian of Bohemia. Over fifty Miocene species have been described ranging throughout Europe, in eastern Asia, and in Virginia, Colorado, Idaho and Oregon in this country. About a dozen Pliocene species have been described from Spain, France, Italy and Sicily.

The genera *Cassine* Linné and *Pterocelastrus* Meissner both now confined to South Africa and Madagascar, each has a fossil species in the Miocene of Bohemia. The genus *Pachystima* Rafinesque, with two existing species in North America, has an Upper Cretaceous species in North Carolina and a Miocene species in Colorado.

The genus *Maytenus* Feuill, with about 70 existing species of the tropics and subtropics of South America has a well-marked species in the Wilcox flora. There are two species in the early Tertiary of Chili, one in the late Oligocene and three in the Miocene of southeastern Europe.

The monotypic genus *Gyminda* Sargent confined to Florida and the West Indies in the existing flora has a doubtfully determined fossil species in the Magothy formation of the Atlantic Costal Plain. The genus *Microtropis* Wall., with 9 or 10 existing species of the mountains of southeastern Asia from India to China and Japan, has a doubtfully determined form in the early Pliocene of Italy.

A well preserved flower in the Baltic amber is described by Conwentz as *Celastrinanthium Hauchecornei*.

The genus *Elæodendron* Jacquin, with about 25 existing species confined to South Africa, has a considerable geologic history. Four Upper Cretaceous species have been described—one from Australia, one from the Dakota sandstone, and two from the Magothy formation of the Atlantic coast. There are six Eocene species showing that the genus was represented in New Zealand (?), Australia (?), Alaska, the Ypresian of England and the Fort Union of the Rocky Mountain region. There are five Oligocene species in the Tyrol, Bohemia and Transylvania; nine Miocene species in France, Switzerland, Italy, Prussia, Bohemia and Styria; and four Pliocene species in Italy.

The remaining genus known in the fossil state, *Euonymus* Linné, has about sixty existing species widely distributed throughout the northern hemisphere but most numerous in the Asiatic tropics and in China and Japan. Upwards of thirty fossil species are known, being based upon both fruits and leaves. There are four well-marked Eocene species all of which are confined to North America where they are represented in West Greenland, in the Fort Union and Green River beds of the Rocky Mountain region, and in the Wilcox of the Mississippi embayment. The species of the latter region is a very abundant and characteristic form. There are four or five Oligocene species of *Euonymus* recorded from Bavaria, the Tyrol and Bohemia. The twelve Miocene species occur in France, Prussia, Bohemia, Styria, Croatia, and Hungary. There are four Pliocene species in Germany, Italy, and Slavonia; and two still existing species occur in the Pleistocene of France.

From this very brief survey of the fossil history of the Celastraceæ it is seen that there is a probability, similar to that shown by so many other families of Dicotyledonæ, that the ancestral stock originated in the western hemisphere.

The family Sapindaceæ consists of about 118 genera and over one thousand existing species of trees or shrubs with alternate, pinnate, exstipulate, persistent, or deciduous leaves and drupaceous or capsular fruits with crustaceous mostly solitary seeds. About one third of the genera are lianas. Most of the family is confined to tropical and subtropical regions and about 23 per cent. of the genera (27) and 34 per cent. of the species (345) are confined to America. There are more genera (30) confined to the African region but only about one fifth as many species (75).

The genera *Cardiospermum*, *Schmidelia* (*Allophylus*) and *Sapindus* are found in all tropical countries. The genus *Paullinia* with over 120 existing species while mostly American is present in Africa and Madagascar. The genus *Dodonæa* with over 40 species in Australia has one or two forms found in all tropical countries and a single species in the Hawaiian Islands and Madagascar. *Harpullia* is common to Asia, Africa and Australia. There are two genera with about fifteen species confined to Australia, four genera with 66 species ranging from Asia to Australia, 10 genera with 22 species

confined to the East Indies, two genera with 20 species confined to Polynesia and 6 genera with 35 species ranging from Malaysia or the East Indies to Australia. It is quite obvious from these few facts regarding the existing distribution that the family is an ancient one and that there has been an extensive evolution of both generic and specific types in relatively modern times in the American tropics on the one hand and in the Malaysian region on the other.

The fossil record while much less complete than might be wished includes at least 13 genera of which six are extinct, and about 160 species, by far the largest number being referred to the still existing genus *Sapindus* which appears well differentiated and widely distributed at the dawn of the Upper Cretaceous. There are about ten Upper Cretaceous species of which all but four occur in pre-Senonian strata. Thus there are two in the Perucer beds of Moravia and Bohemia, one at Niederschœna in Saxony—all Cenomanian. Two in the Atane and one in the Patoot beds of West Greenland. Two in the Dakota group; two in the Tuscaloosa formation of Alabama, one in the Middendorf beds of South Carolina, one in the Woodbine formation of Texas; two each in the Raritan and Magothy formations of the Middle Atlantic states, one in the Montana group and two in the Laramie. I have given this Upper Cretaceous distribution in some detail because of the special interest attached to the deployment of the Upper Cretaceous Dicotyledonæ. It should be noted that seven of these Upper Cretaceous forms are North American. There are over thirty Eocene species of *Sapindus* of which two thirds are North American. The genus is very abundantly represented in both individuals and species in the coastal floras of the Wilcox group from which I have described no less than 9 species. There are four species in the overlying Claiborne group. Species of *Sapindus* are equally common in the Rocky Mountain province in the Denver, Fort Union and Green River beds. There is an Eocene species in Greenland and one each in New Zealand, Australia, Tasmania and Chili. There are four undescribed species in the Ypresian of England and a fifth in beds of the same age in Hungary. There is an Upper Eocene species in France and a second in Oregon.

There are six or more Oligocene species well distributed in Eu-

rope to which continent their discovery has thus far been confined. There are over 30 Miocene species found throughout southern Europe, in eastern Asia, and in North America (Colorado, Oregon and Yellowstone Park). The eight or ten Pliocene species are confined to southern Europe.

In addition to the genus *Sapindus* there are several form-genera derived from the same root. Thus *Sapindophyllum* has been applied to two species from the Albian of Portugal (?). To it are also referred a Cenomanian and a Chattian species from Bohemia and a Tertiary species from Japan. The term *Sapindoides* has been used by Perkins for *Sapindus*-like fruits preserved in the early Tertiary lignites of Brandon, Vermont, from which eight species have been described. In some respects the most interesting genus is *Sapindopsis* Fontaine represented by three abundant and well preserved species in the Patapsco formation (Albian) of Maryland and Virginia one of which is also present in the Fuson formation of the Black Hills, and which I have shown[36] to be very probably ancestral forms of the genus *Matayba* Aublet (Cupanieæ) which has upwards of two score existing species in the tropical and subtropical regions of America. This well-marked type suggests the interesting question of how early in the Mesozoic the ancestors of many modern genera may have been present in equatorial America.

The genus *Paullinia* Linné which has about 122 existing species mostly confined to the American tropics but sparingly represented in Africa and Madagascar, has an Oligocene species in Prussia and two early Miocene species in southeastern France and Bohemia.

The genus *Thouinia* Poit, which in the modern flora has about 15 species confined to the West Indies and Mexico, is represented by an early Tertiary, probably Eocene species, in Chili. The genus *Nephelium* Linné with over a score of existing species in southeastern Asia is recorded by Unger from the Aquitanian of Greece and by Geyler from the Tertiary of Borneo.

The genus *Kœlreuteria* Laxm. is represented by two Chinese species in the existing flora. In the fossil state it is recorded from the Tertiary of the Island of Sachalin, from Spitzbergen and from

[36] Berry, Md. Geol. Surv., Lower Cretaceous, pp. 467–474, Pl. 83–88, 1911.

Switzerland and Baden. Felix has described a genus, *Schmideliopsis,* based on fossil wood from the Oligocene of the Island of Antigua, very close to the existing genus *Schmidelia* Linné which has upwards of a hundred existing species in all tropical countries.

The modern Cupanieæ are represented in paleobotanical literature not only by *Cupania* but by species of *Cupanites* and *Cupanoides.* The latter generic term was proposed by Bowerbank for cupaniaceous fruits and seeds of which he described several characteristic species from the Ypresian of the Island of Sheppey. Similar forms have also been recognized in the Miocene of Carniola and in the Pliocene of Italy. The genus *Cupania* Linné has about 35 existing species confined to the American tropics. Several Ypresian species from the south of England have been referred to it by Ettingshausen and it has also been recorded from the Miocene of the Island of Sachalin. The greater number of *Cupania*-like forms have, however, been referred to the genus *Cupanites* Schimper. Nine or ten species have been described and with the exception of extremely doubtful forms from the Upper Cretaceous of New Zealand and the Eocene of Australia, the oldest authentic occurrences are the two species of the Wilcox flora. There is a third species in the overlying Claiborne group of the Mississippi embayment. The oldest European form is one from the late Oligocene of Styria. In the Miocene, species are recorded from Germany, Bohemia, Austria, Croatia and Hungary.

The genus *Dodonœa* Linné often made the type of a distinct family, the Dodonæaceæ, has about fifty existing species of which four fifths are Australian. *Dodonœa viscosa* Linné is cosmopolitan in the tropics and there are one or two additional species in the American tropics as well as one in the Hawaiian Islands and another in Madagascar. The genus (including *Dodonœites*) was evidently widespread in former times and upwards of a score of fossil species, based on both leaves and fruits, have been described. The oldest known forms are two species in the Ypresian of the north of England and the two contemporaneous species in the Wilcox, which are represented by both leaves and characteristic fruits. There are five Oligocene species in France, Tyrol, Bohemia and Styria; and ten

Miocene species in Prussia, Baden, Switzerland, Bohemia and Croatia. A well-marked species occurs in the Claiborne (Lutetian) ranging along the Claiborne coast from northeastern Georgia to central Louisiana.

It is impossible from the known facts to discuss the place of origin of the family, but it is obvious that certain genera were evolved toward the close of the Lower Cretaceous in equatorial America and have inhabited that or adjacent areas throughout the long stretch of time down to the present.

The order Rhamnales includes about 1,000 existing species of shrubs, trees and vines about equally divided between the families Rhamnaceæ and Vitaceæ. It closely parallels the Sapindales in its floral development but is distinguished by the mostly tetracyclic flowers with opposite stamens and often lacking a corolla. The leaves are simple and typically alternate. Of the two families only the Rhamnaceæ is represented in the Wilcox flora.

The family Rhamnaceæ (Frangulaceæ) includes 47 genera and about 500 species of shrubs and trees mostly of the tropics but with several genera extending for considerable distances into the temperate zone, the genus *Rhamnus* in particular being mostly extratropical in the northern hemisphere. The genera *Zizyphus, Adelia* and *Gouania* are found in all tropical countries. Almost half of the genera are common to more than one continental area. America has the greatest number of peculiar genera (15) with about 85 species. Two monotypic genera are confined to Asia, five genera including the large genus *Phylica* Linné together with about 70 species are confined to Africa and five genera including the two large genera *Spyridium* Fenzl. and *Cryptandra* Smith in all with about 70 species are confined to Australia.

Ten or eleven genera, of which five are present in the Wilcox flora, are found fossil, the three largest being *Rhamnus, Paliurus* and *Zizyphus*. The genus *Rhamnus* Linné which is cosmopolitan in the northern warm temperate and subtropical zones has about seventy existing species. There are considerably over one hundred fossil species, mostly well characterized with simple often entire leaves with ascending secondaries and closely spaced fine percurrent

nervilles. There are a dozen or more species described from the Upper Cretaceous, the genus appearing in the Cenomanian in both Europe (Niederschœna, Saxony) and America (Raritan formation). There are six species in the Dakota sandstone, two in the Magothy formation, one in the Atane and two in the Patoot beds of Greenland. The genus is represented in the Montana group and the Laramie formation of the Western Interior and in the Senonian of Westphalia. There are about thirty Eocene species, the majority being North American. Species of *Rhamnus* are very common in the Raton and Denver formations along the Front Range of the Rocky Mountains and from the base to the top of the Wilcox. There are four species in the Raton, eight in the Denver and six in the Wilcox. The genus is also well represented in the later Eocene along the Pacific coast and in western Greenland. In Europe only a single species is recorded from the Paleocene. The Ypresian which is synchronous with the Wilcox has three species in the south of England.

There are eleven or twelve Oligocene species in France, Prussia, Tyrol, Italy, Dalmatia, Styria, and Greece and a single undescribed species in the Apalachicola group of Florida. There are over two score Miocene species, *Rhamnus* being especially abundant in the Miocene of Switzerland, Italy, Bohemia, Prussia and Styria. It is also present at this time in Iceland, Spitzbergen, Manchuria and Sachalin Island. In this country there are species in British Columbia and in Colorado.

There are about thirteen Pliocene species, no less than nine being recorded from Italy and there is one known from the Island of Java. There is an extinct species in the Pleistocene of Hungary and a recent species in the Pleistocene of the Island of Madeira. In addition to the species referred to *Rhamnus* the form-genus *Rhamnites* Forbes founded on three species from the Eocene of the Isle of Mull has two American Upper Cretaceous species found in the Raritan, Tuscaloosa, Magothy and Dakota formations. There is a species in the Fort Union and another in the Wilcox. The genus *Rhamnacinium* of Felix is based on petrified wood. It contains five or six species found in the Eocene of the Caucasus, Texas, Saskatchewan and the Miocene of Yellowstone Park.

The genus *Paliurus* Jussieu with only two existing species ranging from southern Europe through southern Asia to China and Japan was cosmopolitan in former times. Upwards of 40 fossil species have been described. At least twelve are known from the Upper Cretaceous, all confined to the North American region. There are two each in the Raritan, Magothy and Laramie, five in the Dakota and one each in the Eutaw formation of Georgia, in West Greenland and Vancouver Island. There are ten Eocene species also confined to North America. Two of these are found in the Fort Union and there are three each in the Denver, in western Greenland and in the Wilcox. The leaves are not common in the Wilcox but the characteristic peltate fruits are not uncommon. The oldest European forms are two species in the Oligocene of France and there is a well marked species in the Oligocene (Vicksburg group) of Louisiana. The thirteen Miocene species are found in Asia (Siberia, Sachalin), Europe (Switzerland, Baden, Germany, Bohemia, Italy, Styria and France), and North America (Colorado and Oregon). The presence of numerous species of *Paliurus* in the Upper Cretaceous and Eocene of North America and their absence on other continents before the Oligocene renders it very probable that the genus originated in the western hemisphere.

The genus *Zizyphus* Jussieu with about forty existing species largely shrubs, often prostrate or scrambling, and rarely small trees, is mostly Indo-Malayan in its distribution but is represented by a few species in the tropics of eastern Asia, America, Africa and Australia. There are over fifty known fossil species and as in the genus *Paliurus* the ten Upper Cretaceous species are confined to North America. They are found in the Raritan and Magothy formations of New Jersey and Maryland, the Eutaw formation in Georgia, the Tuscaloosa formation in Alabama, the Woodbine formation in Texas, the Dakota sandstone of the West, the Patoot beds of Greenland and the Upper Cretaceous of Alaska. There are about twenty Eocene species including the two common and characteristic species of the Wilcox and one in the overlying Claiborne of the embayment region, five in the Denver, three in the Fort Union, two in the Green River, one in Alaska and one in West Greenland. There are two

Paleocene species in France and Belgium, four Ypresian species in the south of England and a Lutetian species in France. There are eight Oligocene species very common in deposits of this age throughout Europe. Over twenty species have been recorded from the Miocene of Colorado and California in this country, from France, Switzerland, Germany, Italy, Austria-Hungary, and Russia in Europe, and from Japan and Java in Asia. There are three or four Pliocene species in Europe. While the evidence is not so clear as in the case of *Paliurus* there is a possibility that *Zizyphus* too is of occidental origin.

The genus *Reynosia* Grisebach with only two existing coastal species ranging from the Florida keys through the West Indies has two characteristic species based on leaves in the Wilcox flora and a third species based on the petrified wood in the overlying Claiborne deposits of Texas.

The genus *Berchemia* Neck. has about a dozen existing species, ten of which are confined to eastern and southeasern Asia. There is one in eastern extratropical North America and one in eastern Africa. This distribution could not have been brought about except by the agency of a cosmopolitan Tertiary range. While the specific differentiation of *Berchemia* is limited to five or six fossil forms these are very common and wide ranging. The earliest occurrences are North American and include the Raton, Denver and Fort Union formations of the Rocky Mountain province. The genus makes its apearance in Europe during the Oligocene and is common throughout that region in the Miocene, becoming restricted to southern Europe (France, Italy, Sicily and Slavonia) during the Pliocene.

A species of *Hoveniphyllum* supposed to represent the existing genus *Hovenia* Thunberg with a single existing species in southeastern Asia, is present in the Plio-Pleistocene of Japan. The genus *Colubrina* Brongniart with 15 existing species in tropical America and one in southeastern Asia is recorded from the Miocene of Bohemia.

The genus *Pomaderris* Labill with about 24 existing species confined to Australia and New Zealand has two species in the Eocene of the former region and three species (*Pomaderrites* Ettingshausen) in the Miocene of Prussia, Bohemia and Styria.

The genus *Ceanothus* Linné with about forty existing speci confined to North America has included numerous fossil species su sequently referred to *Paliurus* or *Zizyphus*. There are four r corded from the Upper Cretaceous of Greenland, New Jersey, Va couver Island and Westphalia; two Eocene species recorded fro Greenland and British Columbia; a Miocene species in Prussi Switzerland and Italy; and a Pleistocene species in Kentucky.

The next order, the Malvales, includes nine families and abo 1,800 existing species. The Tiliaceæ, Sterculiaceæ and Bombacac are the only ones represented in the Wilcox flora. The large modern family, the Malvaceæ with over 800 species, many of whi are herbaceous and range from 65° North latitude (Russia) to 4 South latitude (New Zealand), is not represented in the Wilco The order displays somewhat uneven or but little understood phyl genetic characters but is evidently allied to the succeeding order, t Parietales, through the family Elæocarpaceæ. These inequalities evolution are shown among other ways by the complete syncarpy the Tiliaceæ associated with an indefinite number of stamens and the complex arrangement of the stamens in the Sterculiaceæ, asso ated with more or less incomplete union of the carpels. Both t leaves, flowers and fruits exhibit a wide range of variation throug out the order.

The family Tiliaceæ, represented in the Wilcox flora by a sing not very common form of *Grewiopsis*, has about 35 genera and 3 existing species mostly of tropical lands and showing two centers differentiation and distribution—one surrounding the Indian Oce and the other in northern South America. The geological history confined to the four genera *Tilia* (or *Tiliæphyllum*), *Grewia*, *Grew opsis* and *Apeibopsis*.[37] The genus *Tilia* Linné with 18 or 20 wide distributed existing species in the north temperate zone (absent western North America and central Asia) has furnished about fossil species based upon both leaves and fruits, the oldest know being from the North American Eocene. There are no conclusi Oligocene records except two French species, but about fifteen Mi

[37] The genus *Luhea* has been described from the Eocene of Sézan (Langeron) and from the Oligocene of Ménat (Laurent), both Fren localities.

cene species are found in North America, Europe, Asia and the Arctic regions. There are five Pliocene species recorded from Europe and Japan and six Pleistocene species in Ontario, New Jersey, France, Germany, Holland and Denmark. The genus has apparently had its existing range since Miocene time.

The genus *Grewia* Linné has about 90 existing species ranging from Arabia to China and Japan and through Malaysia to Australia, and from Abyssinia to South Africa. About fifteen fossil forms have been described, the oldest known, five Eocene species, coming from western North America. There are two Oligocene species in Europe and about six Miocene species in Oregon, Spitzbergen and throughout Europe. The larger number of *Grewia*-like fossil forms are, however, referred to the genus *Grewiopsis* of Saporta. Six of these are from the Upper Cretaceous and all are confined to North America, a very significant fact since several of them are especially well marked. They are found in the Magothy formation of the east coast, the Tuscaloosa formation of the south coast, and the Dakota Montana and Laramie formations of the western interior. There are about six Eocene species in the Denver, Lance and Fort Union: one in the Wilcox and one in the Claiborne of the Mississippi embayment region, six in the Paleocene of France and one in the Ypresian of England. There is also a Miocene (?) species recorded from Yellowstone Park. While it is quite possible that some of the fossil records ascribed to the genus *Populus* are those of *Grewia* or its ancestral stock, it seems clear that the latter genus or its immediate ancestors were common in the Upper Cretaceous and Eocene of North America.

The fourth fossil genus of Tiliaceæ is *Apeiobopsis* Heer[38] named from its affinity with the existing genus *Apeiba* Aublet which has five or six species confined to tropical South America. *Apeibopsis* includes not only leaves but very characteristic fruits. To it are referred somewhat doubtfully determined leaves from the Upper Cretaceous Dakota sandstone and Atane beds. There are about fourteen Tertiary species including a basal Eocene form from Wyoming, two Ypresian forms from England, a species from West Greenland

[38] To it should probably be referred the Arctic forms described by Heer as *Nordenskioldia*.

three species in the lignites of Brandon, Vermont, two Oligocene species from Italy and five Miocene species from France, Switzerland and Bohemia.

The family Bombacaceæ[39] with 20 genera and about 120 existing species is confined to the tropics and principally to the American tropics. The only known fossil forms are those of the genus *Bombax* or the allied *Bombaciphyllum* and *Bombacites*. *Bombax* Linné has about fifty existing species, all large tropical trees, and almost confined to America. There is a single species in Africa, about six in southern Asia and one in Austalia. The fossil species number over twenty, the oldest known[40] being a common form in the Perucer beds (Cenomanian) of Bohemia and Moravia. There are three species in the Ypresian of southern England and two well-marked forms in the Wilcox flora. There are five additional Eocene forms of which three are from Chili and two from eastern Australia. There are five Oligocene species recorded from France, Saxony, Bohemia and Carniola. The genus is represented in the early Oligocene (Sannoisian) of southeastern France not only by the foliage but by beautifully preserved flowers so that there is little ground for questioning the correctness of the identifications. There are five Miocene species in Bohemia, Croatia and Styria.

The family Sterculiaceæ includes about 5 genera and 800 existing species of mostly tropical shrubs and trees with prevailingly large, simple or digitately lobed or divided leaves; the flowers are sometimes apetalous and differ from those of the Malvaceæ in their 2-celled extrorse anthers. Syncarpy is more or less complete.

The Sterculiaceæ of the existing flora are found on all the continents except Europe. The genera *Sterculia, Helicteres, Melochia, Buettneria* and *Hermannia* have species in both the eastern and western hemispheres. The geologic history of the family extends back to the base of the Upper Cretaceous but is confined to a relatively few number of genera. The most abundant of these is the genus *Sterculia* Linné, which in the existing flora has about one

[39] Ettingshausen, "Ueber die Nervation der Bombaceeen," *Densk. k. Akad. Wiss. Wien. Math. Nat. Cl.*, Band 14, 1858, pp. 49–62, Pl. I.–XI.

[40] An Albian species of Bombax described by Fontaine is entirely valueless.

hundred species of large leafed trees. They are divided into three tribes named from the habit of the leaves the Digitatæ, Lobatæ and Integrifoliæ. The first of these range from farther India to Australia with only one or two American species. The second is most abundant in the American tropics but is also found in Asia and Africa and shows many parallelisms between the American and Asiatic forms. It is most abundantly represented in the past history of the genus. The third and largest modern tribe, the Integrifoliæ, has five or six American species and the balance are found in Asia and Africa.

The fossil forms (sometimes referred to *Sterculiphyllum*) number more than fifty species. Upwards of a score are known from the Upper Cretaceous. These are mostly American and are referable to the tribe Lobatæ which may well have originated in the western hemisphere. There is a species each in the Credneria sandstone of Saxony and the Perucer beds of Bohemia (both Cenomanian) and a third in the Turonian of the latter country. The balance are North American and include species in the Raritan formation, the Cheyenne sandstone of southern Kansas, and in British Columbia, a species in the Patoot beds of West Greenland and six species in the Magothy formation of the Atlantic Coastal Plain and eight species in the Dakota sandstone of the western interior. There are less than a dozen Eocene species, the majority being confined to the lower Eocene. Thus there are three species in the Paleocene of France and another in the Ypresian of England as well as one or two in the Denver and Raton formations of the Rocky Mountain front range. The single large Wilcox species is entirely typical and shows the usual variability in lobation and size. It appears to be filiated with *Sterculia Snowii* Lesquereux from the American Upper Cretaceous, and may be exactly matched by several existing species. There is a small leafed species in the middle Eocene (Claiborne group) of the embayment which exactly matches the typical *Sterculia labrusca* Unger from the European Tertiary and the existing *Sterculia diversifolia* Don. It is closely parallelled by two American Upper Cretaceous species—*S. minima* Berry and *S. mucronata* Lesquereux. There are upward of ten Oligocene species widely scattered over Europe and about 15 Miocene species, mostly

European, with a single species on the east coast of Asia (Sachalin) and two species in Colorado, one of them especially well marked. There are several Pliocene species in southern Europe.

Two somewhat different species of sterculiaceous capsular fruits from the Wilcox are referred to a new genus, *Sterculiocarpus.* The larger of these, *S. eocenicus,* seems referable to the subfamily Buettnerieæ, while the smaller, *S. sezannelloides,* is referable to the Lasiopetaleæ or Helictereæ. Both are very similar to the fruits from the Paleocene of Sézanne referred to the genus *Sezannella.* The latter genus with two species was described by Viguier from casts of wonderfully preserved flowers as well as fruits from the celebrated Travertins of Sézanne and referred with great certainty to the Lasiopetaleæ.

The tribe Dombeyeæ with seven genera and about 75 existing species is almost entirely confined to Africa and the adjoining islands, five or six species of the genus *Melhania* Forsk, only, ranging from Arabia to farther India. This tribe is represented in fossil floras by the genus *Dombeyopsis* Unger named from its supposed affinity with the modern genus *Dombeya* Cav. which has 40 African, mostly Madagascar, species. About 30 species have been referred to *Dombeyopsis.* They are liable to be confused with *Luhea, Grewia* and other forms of the allied family Tiliaceæ. There are three species in the Laramie Cretaceous, two in the Denver formation, twelve (according to Massalongo) in the Upper Eocene of Monte Bolca in Italy, five in the European Oligocene and six in the Miocene of Iceland, France, Switzerland, Prussia, Silesia and Styria. A Pliocene species is recorded from central France. Fossil wood described as *Dombeyoxylon* is recorded by Schenk from the late Tertiary near Cairo, Egypt.

The Buettnerieæ are represented by a doubtful species described from the Miocene of Colorado, and probably by some of the fossil forms referred to other genera, *e. g.,* some of the palmately veined *Ficus*-like forms such as *Ficus occidentalis* and *Ficus Schimperi* both of which are present in the Wilcox flora. Flowers of *Buettneria* were reported from Sézanne by Solms-Laubach but this probably refers to the subsequently described genus *Sezannella,* mentioned in a preceding paragraph.

The Helictereæ are represented by a doubtful species of *Helicteres* Linné described from the Pliocene of Italy and by forms referred to the existing genus *Pterospermum* Schreb. or to the extinct genus *Pterospermites* Heer. Over 30 species have been described. There are nine or ten in the Upper Cretaceous all of which are North American, and their combined range extends from New York to western Alabama, throughout the Rocky Mountain and Great Plains province and in the Atane beds of Greenland. There are about a dozen Eocene species all North American except a single species in the Paleocene of France. The American forms extend northward to West Greenland and Alaska. There are two or three species in the European Oligocene and ten Miocene species throughout Europe and in western North America (Yellowstone Park, California, mouth of the Mackenzie River). A single Pliocene species is recorded from France. It seems probable that this type originated in the western hemisphere since it is so abundantly represented in that region during the Upper Cretaceous and Eocene. The modern species of *Pterospermum* are, however, confined to eastern tropical Asia.

The order Parietales includes thirty families together with over four thousand existing species, the largest families being the Guttiferæ (775), Flacourtiaceæ (530), Begoniaceæ (425), Violaceæ (400) and Dipterocarpaceæ (330). None of these families are present in the Wilcox flora, where the order is represented by the two families, the Dilleniaceæ and Ternstrœmaceæ. The Parietales are prevailingly syncarpous and show affinities with the Ranalian plexus through the Dilleniaceæ which were formerly referred to that order. The alliance as a whole is a complex one including several divergent lines of development with, on the whole, a gradual increase in floral complexity.

The family Dilleniaceæ contains 14 genera and about 275 existing species found on all the continents, the genus *Tetracera* being cosmopolitan in the tropics. The genera *Empedoclea, Curatella, Doliocarpus* and *Davilla* together with 50 species are confined to the American tropics: *Hibbertia* and *Pachynema* together with 75 species are Australian, there are five genera with about 25 species confined to the Asiatic tropics; the genus *Saurauia* (or *Saurauja*),

with about 60 species, is common to Asia and South America; and the genus *Dillenia* with about 25 species ranges from Asia to Australia; so that on the whole the family is prevailingly oriental in the existing flora.

The fossil record is unfortunately most incomplete, illustrating however a wider range of the genera in the past in response to milder climatic conditions in both the north and the south temperate zones during the Tertiary, and also the fact that several of the modern American genera have been American throughout their known geologic history. Thus *Empedoclea* with two existing South American species, sometimes made a subgenus of *Tetracera*, has a fossil form in the eary Tertiary of Chili. The genus *Doliocarpus* with about 20 recent species also in the South American tropics has two fossil forms in the early Tertiary of Chili. The genus *Davilla* with 25 modern species in tropical America is doubtfully represented in the Wilcox flora by *Calycites davillaformis* Berry.

The genus *Saurauja* with 60 modern species in South America and Asia has a species in the Paleocene of France, another in the Ypresian of the south of England and a third in the Miocene of Croatia.

The genus *Dillenia* with 25 existing species confined to Asia and Australia is represented by a form in the Paleocene of Belgium and by some of the Wilcox species referred to the form-genus *Dillenites.* The genus *Tetracera* with 40 recent species found in all tropical lands, has two fossil species in the early Tertiary of Chili, another in the Pliocene of Java and is represented in the Wilcox flora by some of the species of *Dillenites.* I have recognized five well-marked species of *Dillenites* in the Wilcox and these appear to represent modern forms of both *Dillenia* and *Tetracera.*

Conwenz described three species of *Hibbertia,* a large Australian genus, in the Baltic amber (Sannoisian) but Schenk considered that they did not belong to either this genus or even the family.

The family Ternstroemaceæ (Theaceæ) contains about 16 genera and 175 existing species mostly tropical but extending into the north temperate zone in North America and eastern Asia (*Thea, Gordonia* and *Stewartia*). The following seven out of the sixteen genera are confined to a single area: *Bennetia* Martius with five species inhabits

the South American strand, *Asteropeia* Dub. is confined to Madagascar, *Thea* Linné with sixteen species is confined to southern and eastern Asia, *Mountnorrisia* Szysz. with two species is a native of the East Indies, the three monotypic genera *Visnea* Linné, *Tremanthera* Müller and *Pelliciera* Tr. and Planch. are confined respectively to the Canary Islands, New Guinea and Central America. The remaining nine genera, all relatively small, are all found in more than one region. Thus *Archytæa* Martius has two species in northern South America and a third in the East Indies; *Gordonia* Ell. has two North American species and fourteen scattered from India to Malaysia; *Hæmocharis* Salisb. has nine American and five Asian species; *Stewartia* Linné with five species is found in North America and Japan; *Taonabo* Aublet has 20 species in South America and eight in Asia; *Adinandra* Jack. has 19 African species and one in Asia; *Eurya* Thunb. with 36 species and many varieties is confined to tropical America and the East Indies.

This remarkable existing distribution and the pairing of America and Asia as well as the fact that it requires five subfamilies for the reception of only sixteen genera are sure indications that the family has an extended geologic history and that many of the genera were once cosmopolitan. Unfortunately most of this history is unknown.

The genus *Stewartia* is represented in the Baltic amber by a fine flower (*Stewartia kowalowskii* Caspary) and by leaf remains from the Plio-Pleistocene of Japan (Mogi). *Gordonia* has a species in the Pleistocene of Java. The genus *Eurya* Thunberg, now American and East Indian, has a species in the Oligocene of France (*Freziera* Swartz). Fossil wood described by Felix and named *Ternstrœmiacinium* occurs in the Eocene of the Caucasus. *Visnea* Linné, now confined to the Canaries, has a typical fruit in the Aquitanian of Rhenish Prussia. The genus *Ternstrœmia* Nuttall (antedated by *Taonabo* Aublet) has several fossil species, the oldest (*Ternstrœmiphyllum*) coming from the Perucer beds (Cenomanian) of Bohemia. It has two species in the Ypresian of the Isle of Wight, one in the Miocene of Bohemia and another in the Miocene of Croatia. I have described four well-marked species of *Ternstrœmites* from the Wilcox group and similar forms are present in the overlying Claiborne group (Lutetian). Finally the very abundant species in

the North American Cretaceous described as *Celastrophyllum* and already referred to in the discussion of the Celastraceæ are very probably, in part at least, referable to this family, so that enough is known of the geologic history of the group to confirm at least the statement made in a preceding paragraph that it must have had a long and extended history.

The family Lauraceæ with in the neighborhood of 1,000 existing species distributed among forty to fifty genera is often placed next to the family Anonaceæ among the Ranales (*e. g.*, in Engler and Prantl's "Naturlichen Pflanzenfamilien"). It may be noted, however, that the spiral arrangement of floral organs characteristic of the order Ranales is replaced by a cyclic ararngement and hypogny is also replaced by epigyny, so that I follow various students in referring the Lauraceæ to the order Thymeleales, the other large family of which, the Thymelæaceæ (not known in Wilcox flora), has about 400 existing species, chiefly of temperate Australia and the Cape region of Africa.

The geographical distribution of the Lauraceæ cannot be disposed of in a similar simple statement since there are not only many anomalies in the distribution of the existing species but we know so considerable a part of the geologic history that our difficulties seem increased thereby rather than diminished. For example the existing species of the family are divided into eight tribes, no one of which except the monotypic Eusideroxyleæ of Borneo is restricted to a single continental region.

The largest of these tribes is the Cinnamomeæ with upwards of 500 species endemic on all the continents but Europe, and chiefly Asiatic and American. The four genera *Persea, Phœbe, Notaphœbe* and *Mespilodaphne* are found in both hemispheres; *Cinnamomum* and *Machilus* are oriental; while *Oreodaphne, Strychnodaphne, Nectandra, Pleurothrium, Umbellularia, Dicypellium* and *Synandrodaphne* are occidental; the first three being large genera and the last four being monotypic.

The tribe Litseæ, with six genera and about 200 species, is represented on all the continents except Europe and Africa. Only 9 of these two hundred species are found in the occident and yet among these is the monotypic North American genus *Sassafras,* and

the genus *Sassafridium* confined to the American tropics. All of the other genera are found on more than one continent.

The tribes Apollonieæ, Cryptocaryeæ and Cassytheæ are found on all the continents but Europe. The Laureæ are Eurasiatic and the Acrodiclidieæ are confined to Central and South America, except the genus *Endiandra* which with 16 species occurs in the East Indies and Australia.

The problem of correctly identifying leaves of the various genera of this family is beset with almost unsurmountable difficulties, not the least of which is due to the wide differences in usage among students of the recent forms where the whole plant is available for study. Long-continued paleobotanical practice has been to refer most fossil leaves that lacked the more apparent characters of *Cinnamomum* or *Sassafras, Persea* or *Malapœnna,* etc., to the comprehensive genus *Laurus* given at a time when *Laurus* was used in a comprehensive sense, and sometimes still more generalized by paleobotanists as *Laurophyllum* for lauraceous leaves of uncertain generic affinity and not necessarily close to the existing species of *Laurus,* in fact they are in general not true species of *Laurus.* I have departed from this practise of describing new species of *Laurus* for a variety of reasons foremost among which is the very great affinities between the Wilcox flora and the existing flora of the American tropics, the evidence from the foliage of a large number of genera being coroborated by fruits or seeds or wood anatomy. I have used this similarity with a great deal, perhaps too much, confidence and the result has been that the following stand out as the more important lauraceous types in the Wilcox flora:

Nearly all are seemingly members of the subfamily Persoideæ and under this subfamily of the tribe Cinnamomeæ as segregated in Engler and Prantl's "Naturlichen Pflanzenfamilien."

First the genus *Cinnamomum,* usually readily recognized and certainly represented in our Eocene floras.

Second the genus *Persea,* represented by the larger and wider forms with the typical venation of this genus.

Third the genus *Nectandra,* so abundant and characteristic of the existing flora of tropical and subtropical America, represented by several species very close to modern forms.

Fourth, I have failed to follow the latest usage in recognizing t genus *Ocotea* as such, since for obvious reasons it seems better recognize the genera *Mespilodaphne* and *Oreodaphne* of Nees rath than to regard them as subgenera of *Ocotea*. The third subgenus *Ocotea,—Strychnodaphne,* I have failed to recognize in the Eoce flora of this area.

The only apparent oddity in distribution shown by the Wilc Lauraceæ in comparison with recent floras of tropical America is t abundance of *Cinnamomum,* and this simply adds confirmation to t well-known fact of the cosmopolitanism of this genus in the ea Tertiary. Grisebach records only 28 species of Lauraceae in l flora of the British West Indies. Most of these are not coastal for although many have a wide range from lowlands to mountains. regards the Lauraceæ those of the Wilcox, which number 30 differe forms, are more closely comparable with the more abundant mode representation of this family in northern South America. This ceives more or less confirmation from a study of the balance of t Wilcox flora. It would seem from a consideration of all of t facts that the early Eocene floras of the Mississippi embayment a much more like those existing at the present time along the Caribbe sea in Central America and northern South America than they a like those of the West Indies. I do not mean by this that the W cox flora has not many points of resemblance to the lowland flora the West Indies and that of the Florida keys. They contain ve many common types, but with the following difference. The Miss sippi embayment Eocene floras represent a maximum northward e tension of a flora like that which now inhabits northern South Am ica. At the end of the Oligocene with the southward migrati of the temperate Miocene fauna as far as Florida, this flora retir to the South American mainland and the present flora of the W Indies, Florida keys, Bahamas and Bermuda represent a later nort ward migration from that area, a migration in which some of t Wilcox types were left behind.

The existing species of *Cinnamomum*[41] number about fifty. Th are confined to the Oriental tropics except for their extension in the warmer more humid part of the temperate zone in Japan, a

[41] Staub, "Die Geschichte des genus Cinnamomum," Budapest, 1905.

they have their chief center of differentiation in the elevated region of Burma, Siam, Cochin-China and Malaysia, although they are cultivated in all tropical countries and outside the tropics in Europe, Africa and North America. Their fruits are eaten by birds which seed them freely so that they commonly escape from cultivation. Thus *Cinnamomum Camphora* (Linné) Nees and Eberm. is naturalized throughout peninsular Florida, and the commercial *Cinnamomum zeylanicum* Breyn., is readily naturalized in the same manner from the Oriental camphor plantations.

While the data for constructing the geologic history of *Cinnamomum* are far from complete there are more known fossil than recent species and these show, as in the case with so many plant groups, surprising extensions of range during the Upper Cretaceous and Tertiary. The original home of the genus is unknown for it appears in the early part of the Upper Cretaceous at about the same time in New Zealand, Australia, central Europe, Greenland, North and South America. The European and North American records appear to be slightly older than the balance and would indicate that the Asiatic region may have been the original home of the genus which spread northeastward across the Behring region to America and northwestward into the European region, the latter largely an archipelago at that time.

The Eocene records include all of the continents except Antarctica and South America. The Oligocene records are chiefly European and African, although the genus is still represented in the Florida Oligocene. During the Miocene *Cinnamomum* was abundant in Europe and present in Asia but appears to have become extinct in North America, at least there are no conclusive North American records. A number of fruits from the Brandon (Vermont) lignites have been referred to *Cinnamomum* but these lignites are in my opinion pre-Miocene in age. The Pliocene records are entirely European and East Indian. The genus appears to have lingered as a common type in Mediterranean Europe until the changing climates that ushered in the Pleistocene glaciation caused its extinction, any connected distribution with its present Oriental home across southwestern Asia having already been interrupted by the orogenic movements and the development of arid conditions in southwest Asia.

There are six well-marked types of *Cinnamomum* leaves described from the Wilcox group, some of them being abundant and generally distributed, and all but two appear to be new to science. In addition buds and flowers that suggest this genus are described under the form-genus *Laurophyllum.*

There are two species of *Persea* in the Wilcox flora. Disregarding the fossil forms referred to *Laurus* in a comprehensive sense there are about fifty known fossil species of *Persea* which is about the number of the existing species. All six of the Upper Cretaceous forms are American where they are widely distributed. By Eocene times they had reached Europe and South America and they are cosmopolitan in the northern hemisphere throughout the Tertiary, being especially abundant in the Pliocene of the Mediterranean region. It would seem as if their Cretaceous origin was occidental, that they spread over the northern hemisphere during the Tertiary and became restricted to southeastern Asia, the Canary Islands and America during the Pleistocene.

The genus *Ocotea* of Aublet with over 200 existing species is, it seems to me, composite, and I regard Nee's three genera *Mespilodaphne, Oreodaphne* and *Strychnodaphne* as distinct. The modern species of *Mespilodaphne* are confined to South Africa and tropical America. The fossil record is almost entirely merged in the forms referred to *Laurus.* I have recognized four well-marked species in the Wilcox flora. They are abundant types and some range from the base to the top of the deposits, and along the Wilcox coast from Mississippi around the head of the embayment and westward to western Texas.

The genus *Oreodaphne* has been recognized in the American Upper Cretaceous and throughout the European Tertiary. At the present time its numerous species are confined to the American tropics. In the Wilcox it has seven well-marked species, which are abundant individually, some ranging from Mississippi to Texas and from the base to the top of the Wilcox. The genus is probably of American origin and it has been a member of the flora of the American tropics from the Upper Cretaceous to the present.

The genus *Nectandra* with about seventy existing species confined to tropical and subtropical America probably has its geologic

history entangled with the fossil forms referred to Laurus. It occurs in the American Upper Cretaceous and the European and South American Tertiary. There are at least five characteristic Wilcox species some of which were abundant along the Wilcox coasts and some range from the base to the top of the deposits. Like *Oreodaphne* this genus appears to have been of American origin, becoming cosmopolitan in the Tertiary and restricted to its original home during the Pleistocene, where it is still a vigorous and much differentiated type.

The tribes Eusideroxyleæ, Litseeæ, Apollonieæ, Acrodiclidieæ, Laureæ and Cassytheæ do not appear to be represented in the Wilcox flora although the Litseeæ are represented in the Upper Cretaceous of the Mississippi embayment area and the Laureæ are common in the American Upper Cretaceous.

The tribe Cryptocaryeæ, now largely American, is represented in the Wilcox by a single well-marked species of *Cryptocarya*. The existing species of *Cryptocarya* number about 40 of which ¼ are South American and the balance Oriental. Only two or three fossil species are known. These come from the Tertiary of Australia and the Pleistocene of Java.

The form genus *Laurus* which serves to render insecure the discussion of the geologic history of the preceding genera includes a very large number of fossil forms of which no less than 25 are Cretaceous, the oldest being from the Albian of France and Portugal. Species of *Laurus* are abundant throughout North America in the Cenomanian, ranging northward to Greenland and also occurring in Europe and Australia. They have over a score of species in the Eocene and with a similar wide range. The 30 or more Oligocene species are confined to Europe. Over 30 Miocene species are confined to Europe and America and the score of Pliocene species are Mediterranean and largely Italian.

I will mention only one other genus since it definitely shows a past history that is probably typical of a large number of genera of Lauraceæ. The genus *Sassafras*.[42] monotypic and confined to North America in the existing flora, belongs to a large tribe—the Litseeæ, which today is chiefly Oriental, ranging from Asia through Malaysia

[42] See Berry, *Bot. Gaz.*, Vol. 34, 1902, pp. 426–450, tf. 1–4, Pl. 18.

to Australia. *Sassafras* has well-marked foliar characters of both form and venation that render it readily recognizable in the fossil state. Upwards of two score fossil forms have been described. The oldest of these are three well marked species in the Patapsco formation (Albian) of the Middle Atlantic slope in Maryland and Virginia. A species is recorded from this horizon in Portugal but the identification is very doubtful as is that of a Cenomanian species described from Bohemia, which latter probably represents the genus *Sterculia*. In America on the other hand the genus is widespread and well differentiated at the base of the Upper Cretaceous, ranging from Greenland along the coast and in the interior to South America and with about a dozen known species. By Eocene times *Sassafras* had reached Europe[43] probably by way of the Arctic regions, where it has been found throughout the Oligocene and Miocene. In the Pliocene the European forms had retreated southward but remained common in Italy, France and Spain. The glaciation of the Pleistocene caused their extinction on that continent, the single existing species surviving today in the original home of the genus.

The order Myrtales as developed in the Wilcox flora contains 11 species of Myrtaceæ, 9 species of Combretaceæ, 1 species of Trapaceæ and 1 species of Melastomaceæ, as against over 7,000 species in the existing flora.

The family Myrtaceæ has over 3,100 existing species separated by taxonomists into 2 subfamilies. The first of these the Myrtoideæ with 32 genera and about 2,400 species comprises mostly tropical forms of which over 75 per cent. are confined to the western hemisphere. There are over 200 in Asia, one of which extends into southern Europe, about 75 in Africa, about 200 in Australia, and about 60 in Oceanica. Nineteen of the genera are confined to America and these include the only monotypic genera in the subfamily, three in number, as well as large and greatly differentiated genera like *Myrcia* with upwards of 450 species. The two other large genera, *Myrtus* with 178 species and *Eugenia* with about 1,300 species, are the only two genera found on all the continents and in these two genera America furnishes 135 species of *Myrtus* and 850 species of *Eugenia*, or over 60 per cent. The second subfamily, the Leptosper-

[43] A very doubtful form is recorded from Australia.

moideæ, comprises the Leptospermæ with 28 genera and about 700 species and the Chamæleucieæ with 12 genera and about 165 species. Both of these tribes are even more strikingly Australian than the Myrtoideæ are American. The Chamnæleucieæ are entirely Australian and mainly confined to western Australia. The Leptospermæ have a single monotypic genus in Chili and the distribution of the other members of this tribe suggest the probability that it should be placed in some other alliance, since with the exception of *Metrosideros*, which is represented in Africa, and the genus *Baeckea* which reaches the Asiatic mainland, all of the genera are confined to Australia or the surrounding islands southeast of Asia.

In a recent paper Andrews[44] has presented some interesting statistics of distribution and an ingenious theory of the history of the family. He considers that the original stock was arborescent or shrubby with entire, simple, opposite, penniveined leaves with dots and intra-marginal acrodrome veins; with the calyx lobes and petals imbricate, probably in fives: flowers regular, solitary or in cymes; stamens indefinite, numerous, free, with versatile, 2-celled anthers; ovary inferior with two or more cells; style simple; fruit inferior, crowned with persistent limb of calyx, indehiscent, succulent or fleshy (rarely dry); albumen none; cotyledons thick and fleshy, with a short radicle.

From the character of Cretaceous climates this or some other theoretical prototype flourished in a mesophytic environment. Among modern groups the nearest approach to this theoretical stock is furnished by the Myrtoideæ which are fleshy fruited, most numerous in species, and widely spread in the equatorial regions, with over 75 per cent., however, confined to America. The existing Myrtaceæ with capsular fruits representing the extreme of specialization in the family are Australian while the Chamælaucieæ standing in an intermediate position between the two preceding groups are almost wholly confined to western Australia.

These are the facts of modern distribution. Their interpretation may be various. Andrews (op. cit.) from a study of the present distribution, geologic climates and the geological history of the Austra-

[44] Andrews, E. C., "The Development of the Natural Order Myrtaceæ," *Proc. Linn. Soc. N. S. Wales*, Vol. 38, Pt. 3, 1913, pp. 529–568.

lian region, concludes that the Leptospermoideæ originated from the Myrteæ, and that the Cretaceous forms were widespread which latter was undoubtedly the case. That before the separation of Australia from the Asiatic mainland fleshy-fruited forms found themselves in a region of warm moist climate but relatively poor soil and that it was this edaphic factor that was the principal stimulus to the differentiation of the Leptospermoideæ, which with the exception of the genus *Metrosideros* show adaptations to poor soil and temperate or dry climates and this exception explains the relatively wide distribution of *Metrosideros* from Asia to the Fiji Islands. The *Eucalyptus* forms according to the view of this student were derived from *Metrosideros* after the separation of New Caledonia from Australia and the latter continent from Asia. To support this latter point Andrews is obliged to consider all of the Cretaceous identifications of *Eucalyptus* and all of the Tertiary identifications outside of Australia as equally misleading. With regard to the presence of *Eucalyptus* in North America I think this contention to be not unlikely, for although in accordance with paleobotanical usage, I have identified numerous forms of *Eucalyptus* in the North American Upper Cretaceous, I have long thought that these leaves represented ancestral forms of *Eugenia* or *Myrcia,* but have hesitated suggesting any change in nomenclature from the havoc it would play with stratigraphic paleobotany.

The supposed American Cretaceous fruits of *Eucalyptus* have long since been shown to be referable to *Dammara*-like forms and in my studies of the Tertiary flora I have scrupulously refrained from referring any of the numerous myrtaceous leaves to the genus *Eucalyptus.* Regarding the possible occurrence of *Eucalyptus* in Europe I am not sure that the identifications of Heer, Unger and Ettingshausen are erroneous. Certain remains considered as *Eucalyptus* fruits seem very convincing from the published figures and there is not the slightest doubt that the other great modern Australian alliance—the Proteaceæ—was represented in both Europe and America during the Cretaceous and Tertiary. There is one additional argument against the Cretaceous radiation and the paleobotanical determination of *Eucalyptus* and that is the great persistence of the peculiar juvenile, opposite, cordate, sessile and horizontal leaves which

must represent an ancestral character of long standing before the evolution of the falcate leaves of the genus with twisted leafstalks and other xerophytic features.[45]

I have dwelt at some length on this question because of its phylogenetic importance and the possible bearing of the Wilcox flora on this point. In considering the morphology of the existing species, *Eugenia* has many claims to be considered the most primitive although *Myrcia* is almost equally old and is certainly closely related to *Eugenia*. Among the numerous Cretaceous fossils from North America now referred to *Eucalyptus* there is not a single one that does not exhibit characteristic features of *Eugenia* or *Myrcia*, especially the latter, a fact greatly impressed on me in handling a large amount of recent material during my study of the Wilcox forms.

In the Wilcox flora there are six well-marked species of *Myrcia* and four nearly equally well marked species of *Eugenia* as well as a single species of *Calyptranthes*. The latter genus appears also to be represented in recent collections from the Isthmus of Panama. Without pursuing the subject beyond the known facts, confessedly meager, and noting the presence in the Wilcox flora of numerous Combretaceæ and a representative of the great tropical family *Melastomaceæ*, largely American in the existing flora, both of which are families closely related morphologically to the Myrtaceæ, it would seem that the known facts, as well as the law of probabilities, suggest America as the original home of the family. That it reached Europe either by way of Asia or the North Atlantic plateau early in the Upper Cretaceous and became cosmopolitan before the close of the Cretaceous. During the late Tertiary this ancestral stock, which largely coincided with the existing subfamily Myrtoideæ, was forced to withdraw from temperate North America to the American tropics, where it had originated and to which it has since been so largely confined. The types peculiar to the Australian region represent the relics of the Cretaceous radiation with numerous new types evolved on that continent as Andrews has suggested. This is exactly the

[45] See Deane, H., "Observations on the Tertiary Flora of Australia," *Proc. Linn. Soc. N. S. Wales*, Vol. 15, 1900, pp. 463-475; Cambage, R. H., "Development and Distribution of the Genus *Eucalyptus*," Presidential Address, *Jour. Proc. Roy. Soc. N. S. Wales*, 1913.

reverse of the hypothesis proposed by Deane (op. cit.) but one t accords far better with the facts not only of geologic history, with those of existing distribution.

As is pointed out in the systematic part of this work all of Wilcox forms are coastal types closely related to existing Ameri species of similar habitat. About 150 fossil forms have been ferred to the Myrtaceæ, one third at least having been described species of *Eucalyptus*. At least half of these occur in the Cretace of all parts of the world, but particularly throughout the north hemisphere. They are especially well represented in North Am ica and the possibility that they are ancestral forms of *Myrcia Eugenia* has already been pointed out. A similar widespread dis bution but less specific variation characterizes the Eocene forms t have been referred to *Eucalyptus*. The Oligocene records are European and the Miocene records include both Europe and A

The genus *Myrtus* has about 24 fossil species, all European, majority being almost equally divided between the Oligocene and Miocene. The oldest forms are early Eocene but the form-ge *Myrtophyllum* Heer has several Upper Cretaceous species in rope, America and Australia, as well as Tertiary species in Eur Asia and South America.

The genus *Myrcia* DC. so well represented in the Wilcox fl has species in the European Oligocene, four species in the early T tiary of Chili and one in the Pliocene of Brazil.

The genus *Eugenia,* also prominent in the Wilcox flora, has oldest known species in the Dakota sandstone. It is represente Europe throughout the Tertiary from the lower Eocene to Pliocene.

The genus *Callistemon* R. Brown has been identified in both Upper Cretaceous and Tertiary of Europe and no less than 25 cies have been referred to the genus *Callistemophyllum* Etti hausen. These include Upper Cretaceous forms in America Europe, Eocene forms in Greenland and Australia, and numer Oligocene and Miocene species in Europe.

Leptospermum, Leptospermites and *Leptospermocarpum* have b identified from the Upper Cretaceous and Tertiary of Europe: *T*

tania-like fruits have been described as *Tristanites* by Saporta from the lower Miocene of France: the genus *Psidium* Linné, with about 100 modern species in the West Indies and Mexico, is represented in Chili by an early Tertiary species: and finally the genus *Metrosideros* has been identified in the Atane beds of Greenland and in both the Oligocene and Miocene of Europe.

The family Combretaceæ (Terminaliaceæ) embraces about 16 genera and 285 existing species of shrubs or trees and tropical vines, with simple, entire, coriaceous, persistent, exstipulate, alternate or opposite leaves. The inflorescence is racemose or capitate and the flowers are regular, perfect or polygamous, often apetalous. The stamens are two or three times as numerous as the petals and the one-celled ovary develops into a drupaceous or berry-like indehiscent fruit, often crowned with the accrescent calyx and containing a solitary seed without endosperm.

The existing species are all tropical or subtropical, ranging from 34° north to 35° south latitude, and a relative large number are littoral or strand types. The various continental areas have the following peculiar species: America 75, Africa 85, Madagascar 36, Asia 57, Australia 23. About ten or a dozen species are found in more than one area, there being a remarkable identity between the American tropics and those of West Africa, the genera *Cacoucia, Conocarpus* and *Laguncularia* having identical species in both regions.

The geologic history of the family is most incomplete, but it is exceedingly prominent in the Wilcox flora where it is represented not only by characteristic leaves but by flowers and fruits. No species are certainly known from horizons as old as the Upper Cretaceous although a species of *Termanaliphyllum* has been described from the Perucer beds (Cenomanian) of Bohemia and a species of *Conocarpites* from the Tuscaloosa formation of Alabama. So far as I know there are no authentic occurrences as old as those of the Wilcox. In this flora there are three well-marked species of *Combretum,* a genus with about 130 existing species found in all tropics except Australia and Polynesia. Over thirty of these are endemic in South America and their abundance in the Wilcox as well as the

occurrence of a species in the early Tertiary of Chili strongly sugge that the genus is of American origin. This statement as well as t determination of the Wilcox species receives confirmation in the markably preserved flower from these beds described as *Combreta thites*. *Combretum* has been recorded from the Miocene of Switz land and Germany, and from the Pliocene of Italy. It occurs in t Claiborne group of the Mississippi embayment and Felix has scribed petrified wood from the supposed Eocene of the Caucas which he calls *Combretacinium*.

The genus *Conocarpus* Gærtner, a member of the tropical ma grove association, has a well marked species in the Wilcox flo which is supposed to be descended from the *Conocarpites* describ from the Tuscaloosa formation in this same general region. Anoth species very close to the modern form of the American tropics curs in the Claiborne group. *Conocarpus* fruits have also been scribed recently from the Aquitanian of Rhenish Prussia.

The genus *Laguncularia* Gærtner, monotypic in the mangro association of America and the west coast of tropical Africa, is re resented by both leaves and fruits in the Wilcox flora. The o other genus of Combretaceæ with known fossil representation *Terminalia* Linné It is a large genus in the existing flora with o one hundred species almost equally divided between America, As Africa and Australia, several of the species being very wide-rangi littoral types. There are three Wilcox species, based on both lea and fruit. One of the species makes its appearance in the under ing Midway group of the Western Gulf region, possibly representi the beginning of its extension northward along the coast in the e bayment region from tropical America.

Five Oligocene species of *Terminalia* have been described fr Europe, the determinations resting on both leaves and fruits, and t occurences ranging from the Sannoisian to the Chattian and g graphically from southeastern France to Greece. There are sev well distributed Miocene species in Europe, as well as Plioc species in both Spain and Italy along the shores of the Plioc Mediterranean Sea. A supposed Pliocene species is also record from Bolivia.

While future discoveries will have to greatly amplify the fossil record before the history of the family in past times can be traced with any degree of surety, the remarkable display of these forms in the Mississippi embayment region, evidently derived from the American tropics, gives a large amount of probability to the theory that the family originated in the American tropics during the Upper Cretaceous.

The genus *Trapa* Linné, formerly included in the family Onograceæ, is now made the type and only genus of the family Hydrocaryaceæ (Trapaceæ, Dumort, 1827). There are three existing species, all aquatics, and all confined to the old world except for the naturalization of *Trapa natans* Linné, in New England and New York. The latter species is found irregularly scattered throughout central and southern Europe, its area of distribution being a contracting one as shown by its occurrence in post-glacial deposits at very many localities beyond its present range in Russia, Finland, Sweden and Denmark. The two other existing species are *Trapa bicornis* Linné of China and *Trapa bispinosa* Roxburg of southeastern and southern Asia (said also to occur in Africa).

The genus has an extended geological history. Rosettes supposed to represent the floating leaves (*Trapa ? microphylla* Lesq., and *Trapa ? cuneata* Knowlt.) are widespread in the Rocky Mountain province in beds of late Cretaceous and early Tertiary age. The oldest recognizable fruits are a large bi-cornute form from the Eocene of Canada and Alaska and *Trapa wilcoxensis* Berry found in the Wilcox flora. An Oligocene species (*Trapa Credneri*) Schenk has been described from Saxony, and no less than seven species have been described from the Miocene—two occurring in Idaho (Payette formation), one in Japan and the balance in Europe, where two species continue into the Pliocene. A species from the late Pliocene of America is found in southern Alabama. The existing *Trapa natans* has been recorded from the preglacial beds of England and Saxony and from very many interglacial and postglacial deposits in Portugal, Italy, Netherlands, Germany, Sweden, Russia and Denmark, Gunnar Andersson in a recent paper (1910) mentioning 18 localities in West Prussia, 6 in Denmark, 17 in Sweden and 29 in Finland.

The family Melastomaceæ is a relatively large one with ab 150 genera and over three thousand existing species. It is alm strictly tropical although some members range southward to south latitude. It is a typically American family, seven of the fift tribes into which the family is divided being confined to tropi America, and about 2,500 of the existing species being also ende in this region. While the geologic history of this vast assemblage forms is practically unknown, there is no evidence to disprove theory that it, like the allied families Combretaceæ and Myrtac had its origin in that most prolific region—the American tropics.

The few fossil forms that have been found, including lea flowers and calices, have been referred to the form-genus *Melas mites* first proposed by Unger. A doubtfully determined spec which probably belongs to the Lauraceæ, had been recorded from Upper Cretaceous of Westphalia. The only known Eocene spe is the well-marked form present in the Wilcox flora. Four Oligoc species have been described from Bohemia, Styria and Egypt; f Miocene species from Switzerland, Prussia and Croatia; and Pliocene species from Italy.

The order Umbellales (Umbellifloræ of Engler) includes three families—the Araliaceæ, Umbelliferæ and Cornaceæ, toget with upwards of 3,000 existing species of which more than tw thirds belong to the Umbelliferæ. The three families are clos related and stand somewhat apart from the rest of the choripetal orders. While undoubtedly there has been great specific variation very modern times especially among the herbaceous forms of Umb liferæ, some members of the alliance go back as far as undoub dicotyledons have been found, and this fact is one of the strong arguments for considering its relationships to the Gamopetalæ to less close than some botanists have suggested, a suggestion ba primarily on a consideration of the floral structures apart from morphological features of the whole plants As regards floral e lution the Umbellales clearly mark its highest expression among Choripetalæ and parallel the Gamopetalæ. The flowers are epi nous, with cyclic stamens, reduced carpels, and often reduced sep The Araliaceæ and Cornaceæ are both positively and the Umb liferæ doubtfully represented in the Wilcox flora.

The family Araliaceæ contains about 52 genera and 500 existing species, chiefly inhabitants of the tropics, the notable exceptions to this statement being in North America and eastern Asia. The modern center of development is in the Asia-Australia region, no less than 33 genera being confined to Asia, Malaysia, Australia or Polynesia. Africa has three peculiar genera with about 30 species; America has five peculiar genera with about 100 species. The genus *Schefflera* is cosmopolitan. *Hedera* and *Polycias* occur in Eurasia and Africa. Two genera are common to Asia and America and *Aralia* adds Australia to a similar distribution. *Pseudotenax* with about six species is peculiar to western South America and New Zealand.

The fossil record is not nearly as complete as it should be to afford a secure basis for generalizations. A number of genera are found, however, in the oldest deposits in which undoubted dicotyledons are known. The largest genus is *Aralia,* commonly used by paleobotanists as a form-genus for generically unidentified species of Araliaceæ, rather than for forms falling within a strict modern definition of *Aralia.* No less than fifty species of *Aralia* have been described from the Cretaceous. Two of these come from horizons as old as the Albian of Portugal. In beds of similar age in eastern America (Maryland and Virginia) there are two well-marked species referred to *Araliæphyllum* and clearly ancestral to the numerous species of *Aralia* so common in the Upper Cretaceous of the latter region. Very similar, in some cases identical, forms are found in the Cretaceous on both sides of the Atlantic. There are fifteen species in the Perucer beds (Cenomanian) of Bohemia and Moravia and a like number in the Dakota sandstone of the western United States, while along the east coast there are nine species in the Raritan formation, eight in the Magothy formation, and one each in the Black Creek formation of North Carolina, the Eutaw formation of Georgia, the Tuscaloosa formation of Alabama and the Woodbine sand of Texas. In Greenland there are two species in the Atane beds and a third in those of Patoot. In the younger Cretaceous there are two species in Bohemia, two in Westphalia and one in Colorado. Australia has a species and ten supposed varieties of *Aralia* in the Upper

Cretaceous beds of that country. In addition to the foregoing display, the allied genus *Araliopsis* (Berry 1911) has a number of well-marked species in the Raritan, Magothy and Dakota formations, so that it must be conceded that the araliaceous stock was well differentiated and cosmopolitan before the close of the Cretaceous.[46]

There are over a score of Eocene species of *Aralia,* they being especially common in the Fort Union of the western United States, the Paleocene of Belgium, and the Eocene of Australia. The three Wilcox species are not common—two of them are common Fort Union species and the third was described originally from western Greenland. In addition there are species in the Denver formation, the Green River formation, in Oregon, New Zealand, Italy, and the south of England.

There are upwards of twenty Oligocene species, especially in the Sannoisian of southeastern France from which 14 species have been described. All of the other Oligocene records are also European.

There are also about twenty Miocene species distributed over North America, Europe and Asia. Some of the California species, *e. g., Aralia Whitneyi,* are clearly ancestral to existing Asiatic east-coast forms. A fruit (*Araliæcarpum*) is described from the Miocene of Prussia. There are in addition between 15 and 20 fossil species of *Aralia* more or less doubtfully connected with other genera of the family, *e. g.,* there is a species of *Arthrophyllum* doubtfully identified from the upper Oligocene of France; a species of *Cephalopanax* (?) is recorded from the lower Miocene of France; several forms of *Sciadophyllum* (?) occur in Greenland, Bohemia and France; and *Paratropia* (?) is recorded from the Paleocene, Oligocene and Miocene of France and the Miocene of Bohemia.

There are two species of *Oreopanax* in the Wilcox flora, one of them exceedingly well marked and clearly referable to the section Digitatæ of *Oreopanax.* The latter genus has about eighty existing species with simple, lobate and digitate leafed sections confined to tropical America but present in the Paleocene, Tongrian and Aquitanian of France. The modern Asiatic genus *Acanthopanax* Decaisne and Planchon has Oligocene species in France and Germany, and a Miocene species in Japan.

[46] See Berry, "Aralia in American Paleobotany," *Bot. Gaz.,* Vol. 36, 1903, pp. 421–428.

The genus *Panax* Linné with about six existing species in Asia and North America has furnished a number of fossil forms based on numerous characteristic fruits as well as leaves. It is represented from Greenland to Alabama along the west coast of the Atlantic and in the Perucer beds of Bohemia (*Araliphyllum*). It has five species in the Oligocene of Europe and six Miocene species in Europe and Colorado. The genus *Cussonia* Thunberg with about 25 African species in the existing flora is doubtfully recorded from the Albian of Portugal. It is present in the Perucer beds of Bohemia (*Cussoniphyllum*) and in the Oligocene of France and Greece.

The genus *Hedera* Linné with only three existing species of Europe, Asia and Africa has numerous and well-defined fossil forms.[47] No less than fifteen have been described from the Upper Cretaceous of both America and Europe. There are about seven Eocene species in Greenland, Alaska, the Fort Union of the western United States, and in the Paleocene of Belgium and France. The genus remains common during the Tertiary in Europe and is present in America as late as the Upper Miocene lake of Florissant, Colorado, The anecestor of the existing *Hedera helix* Linné occurs in the Pliocene of central France and the modern form itself is found in the Pleistocene of England, France and Italy. A species of *Polyacias* occurs in the Pleistocene of Java associated with *Pithecanthropus erectus*.

The family Umbelliferæ with 170 genera and upwards of two thousand existing species is distinctly an extratropical family with numerous boreal forms. The majority are herbaceous and of relatively modern origin. It is very sparingly and doubtfully represented in the fossil state and the only Wilcox form that suggests such an affinity is the fruit described as *Carpolithus prangosoides* which greatly resembles those of the existing genus *Prangos* Lindley.

The third family of the Umbellales, the Cornaceæ, is a relatively small one, with only sixteen genera and about 100 existing species, mostly of the temperate zone. The fossil forms are confined to the two genera *Cornus* and *Nyssa*. *Cornus* has about 40 existing species of herbs and small trees mostly confined to the north temperate zone

[47] The forms from the Potomac group of Maryland and Virginia described by Fontaine as species of *Hederæphyllum* are entirely worthless.

in Eurasia and North America but represented in Mexico, and with a single species in Peru. Over fifty fossil species have been described. There are at least twelve in the Upper Cretaceous, all confined to North America and ranging from Greenland to Alabama. There are about a dozen Eocene species in America, Europe, and the Arctic, one of these is sparingly represented in the Wilcox flora. Oligocene records are few in number but over 25 Miocene species have been described, the genus being particularly abundant at this time throughout central Europe but also represented in both North America and Asia. About five Pliocene species are recorded from Spain, France, Italy and Japan and the genus has afforded Pleistocene material in New Jersey, Holland, England, etc.

The genus *Nyssa* Linné (including also *Nyssidium* Heer and *Nyssites* Geyler and Kink.) comprises about seven existing species ranging from shrubs to large trees, natives of southeastern North America and eastern and central Asia. It has furnished over fifty fossil forms, the majority being based on the characteristic costate stones. The oldest known forms are from near the base of the Upper Cretaceous (Dakota, Tuscaloosa) of North America. By Eocene time *Nyssa* had reached Alaska, Greenland and Europe. There are two characteristic species in the Wilcox, both based on stones, and a third occurs in the overlying deposits of the Claiborne group. In the lignite deposit of Brandon, Vermont, of uncertain but probably early Tertiary age, no less than eighteen so-called species of stones have been described, and while doubtless the specific differentiation is overrefined, it emphasizes the abundance of *Nyssa* in New England at that time. *Nyssa* is abundant in the European Oligocene and there are Miocene species in New Jersey, Virginia, Europe, and Asia; and a Pliocene species occurs in Alabama. Some of the modern species are common in the Pleistocene of this country from New Jersey southward.

While much remains to be learned regarding the history of the Cornaceæ it seems clear that the two genera *Cornus* and *Nyssa* which have yielded fossil forms are both types that appear to have originated in North America during the Cretaceous.

No family of the Choripetalæ has succeeded in maintaining a

world-wide distribution as have several families of Monocotyledonæ and Gamopetalæ. No distinctly boreal group has been developed as among the Gamopetalæ (Ericales). Certain great families characterize the north temperate region and these are all herbaceous forms believed to be of relatively recent origin, e. g., Polygonaceæ, Caryophyllaceæ, Cruciferæ, Saxifragaceæ, Onagraceæ and Umbelliferæ. While aquatic forms are common this habit does not characterize whole families as among the Monocotyledonæ. The Choripetalæ predominate in the American tropics and many of the families present in the Wilcox flora have been shown to have probably originated in that region.

The second grand division of the Dicotyledonæ, the Gamopetalæ (Sympetalæ), constitute a rather well-defined group, presumably derived from the Choripetalæ, and characterized by a complete cyclic arrangement of the floral parts, a usually gamopetalous corolla, ovules with a small nucellus and usually a single integument. It contains nine or ten orders and upwards of 50,000 existing species. The majority of the orders appear to be more compact and natural groups than the corresponding alliances among the Choripetalæ. The Ericales, Primulales and Ebenales are pentacyclic and isocarpous, while the Gentianales, Polemoniales, Personales, Plantaginales, Rubiales, Valerianales and Campanales are tetracyclic and anisocarpic, the last three orders being epigynous.

The alliance predominates in herbaceous forms and several of the families are distinctly boreal. While the Compositæ, Labiatæ and Plantaginaceæ are of world-wide distribution there are no notable continental pairings such as is usually the result of an extended geologic history. These and many other facts suggest that the Gamopetalæ as a whole, especially the more evolved, herbaceous, extratropical families, are of relatively modern origin whose major specific differentiation was concomitant with the occupation of the temperate zones after the retreat of the Pleistocene ice-sheets.

From the viewpoint of floral structures the so-called Compositæ are clearly the culmination of the evolution of floral structures. This is shown not only by their gamopetaly, epigyny, connivent anthers, and the formation of seedlike fruits with a pappus, but by the complex flowerhead, the prevalence of diclinism, the dimorphism of the

corollas and other special features. This theorem is corroborated by the in general modernness of the alliance.

Six of the Gamopetalous orders are represented in the Wilcox flora. The first of these, the Primulales, in its fullest development in existing floras includes the three families Myrsinaceæ, Primulaceæ and Plumbaginaceæ. They are structurally much alike with a single cycle of stamens opposite the petals, and a unilocular ovary with a free central placenta. This community of floral organization can only be attributed to convergence and not to filiation since the Myrsinaceæ are old forms which in modern floras are predominantly tropical and American while the Primulaceæ are chiefly north temperate and boreal herbs of relatively recent evolution: and the Plumbaginaceæ are very modern halophytic herbs and undershrubs of salt beaches and steppes, the majority being found in the Mediterranean and Caspian regions.

The Myrsinaceæ, the only family represented in the Wilcox flora, is characterized by alternate, simple, coriaceous, punctate, exstipulate leaves; perfect, regular flowers; and single seeded drupaceous fruits.

The family contains about thirty genera and 530 species of shrubs or trees, largely tropical and predominantly American. Thus eleven genera containing upward of 200 species are peculiar to America while there are only four genera with less than a dozen species peculiar to Asia, and three genera with about 100 species peculiar to Africa.

The genus *Myrsine* Linné is found on all the continents except Europe and in Polynesia. Its distribution is extratropical in the African region. *Euardisia* Pax is found in all tropics. *Maesa* Forskal is found in all oriental tropical countries as is also the monotypic genus *Ægiceras* Gærtner, a member of the coastal mangrove association. The genus *Cybianthus* Martius, largely South American, has species in the Philippines and in New Grenada. There is little that is significant in the recent distribution of the family and the fossil record is very incomplete.

Over seventy-five fossil forms have been referred to *Myrsine*. The oldest are the seven or eight forms recorded from the Upper Cretaceous. All of the older of these (Cenomanian) are from North America and only one from the Turonian of Bohemia occurs in the

European Upper Cretaceous. The American forms are not varied specifically but are wide ranging and common, extending from the Atane beds of Greenland along the Atlantic coast to the Tuscaloosa formation of western Alabama as well as in the Dakota sandstone of the western interior.

The recorded Eocene species of *Myrsine* number seven or eight and include an Australian form, one in the early Eocene of Alum Bay, three in the upper Eocene of France, and two in western Alaska. *Myrsine* is exceedingly varied and abundant during the Oligocene throughout southern Europe, over thirty species having been described, of which eleven occur in the basal Oligocene of southeastern France (Sannoisian). There are upwards of thirty Miocene species throughout Europe, one in Colorado being the only known American occurrence. Several species linger in the Pliocene of southern Europe in France and Italy and one species is present in the Pliocene of Brazil. In addition to the forms referred to *Myrsine* several forms from the European Tertiary have been referred to the form-genus *Myrsinites.* Ettingshausen recorded a species of *Pleiomerites* from the Miocene of Bohemia; and the genus *Maesa* Forskal, which has about 40 modern species in Asia, Africa, Australia and Polynesia, is represented in the Oligocene of Transylvania and Egypt and in the Miocene of Styria.

The genus *Ardisia* Swartz (including *Ardisiophyllum* Geyler) has furnished about a dozen fossil species, the oldest of which, a very doubtfully determined form, comes from the Turonian of Bohemia. There is an Eocene or Oligocene species in Chili, three Oligocene species in Bohemia and one in Transylvania. There are four Miocene species in France, Bohemia and Styria; and Pliocene species in Italy and Borneo.

The genus *Icacorea* Aublet is the only member of the Myrsinaceæ found in the Wilcox flora. The genus has numerous existing species confined to South America. The fossil record is meager but includes two or three species of the European Oligocene. The Wilcox species is thus considerably older than any European occurrence. It represents a form which is very close to the modern *Icacorea paniculata* Sudworth, a shrub or slender tree of the Florida keys, Baha-

mas, Cuba and the east coast of southern Mexico. In addition to the foregoing records at least four kinds of flowers have been described from the Baltic amber (Sannoisian). These are *Berendtia* Gœppert (2 species), *Myrsinopsis* Conwenz, and *Senaelia* Gœppert.

While the geologic history of the family is thus so incomplete it is not without significance in this case as in the case of so many families previously discussed, that a predominantly American family in the existing flora has its oldest known fossil occurrences in the basal Upper Cretaceous of North America.

The order Ebenales includes the families Sapotaceæ, Ebenaceæ, Styracaceæ and Symplocaceæ, together with upward of one thousand existing species, the larger families being the Sapotaceæ and Ebenaceæ, both of which are represented in the Wilcox flora, while the other two families are sparingly represented in the European Tertiary. There is considerable range in floral structures from indefiniteness in the number of stamens and carpels and polypetaly, to a 4 to 8 cyclic arrangement, which leads floral morphologists to consider the order as among the most primitive of the Gamopetalæ.

The family Sapotaceæ comprises trees or shrubs with a milky juice and with alternate, simple, entire, mostly coriaceous, petiolate, exstipulate leaves. It contains about thirty-two genera and nearly four hundred existing species of all tropical countries. About half of the existing species are American. There are eleven genera confined to America, seven to Africa, three to Australia, two to New Caledonia, two to Asia and Malaysia, two to Malaysia and one to Asia. The three large genera, *Sideroxylon, Chrysophyllum* and *Mimusops,* are represented in all tropical countries. There are four genera and twelve species represented in the Wilcox flora. The largest of these genera is *Bumelia* Swartz with six well-marked Wilcox species. *Bumelia* with about a score of species is confined to America in the existing flora, ranging from the southern United States through the West Indies and Central America to Brazil. It has numerous fossil species, the oldest coming from the Upper Cretaceous (Dakota sandstone) of the western interior. In addition to the six Wilcox species, which are prototypes of still existing forms, there are two Eocene species (Ypresian) in southern England.

There are about a dozen Oligocene species, ten of which are widespread in Europe, one is found in the Apalachicola group of western Florida and two forms, representing both leaves and fruit, are found in the Vicksburg group of Louisiana and Texas. There are seven or eight Miocene species widespread in Europe and one is recorded from the late Miocene of Colorado.

The genus *Chrysophyllum* Linné with about sixty existing species found in all tropical countries, but the majority American, has a supposed species in the Upper Cretaceous of Saxony (Niederschœna); a well marked species in the Wilcox flora; three Oligocene and six Miocene species in Europe.

The genus *Mimusops* Linné with about 40 existing species in all tropics has three well-marked Wilcox species and a fourth in the overlying Claiborne deposits. To it has been referred a species from the Upper Cretaceous of Saxony (Niederschœna) and it is undoubtedly represented in the Upper Cretaceous of the embayment region as well as elsewhere by the leaves that have been referred to the form-genus *Sapotacites.*

The genus *Sideroxylon* Linné, with about eighty existing species in the oriental tropics and about fifteen in the American tropics, has two species in the Wilcox flora which are the oldest thus far discovered. To this genus have been referred four Oligocene and one or two Miocene species from Europe.

Isonandra Wright a small modern genus of the Malayan region is represented in the Tertiary of Borneo by *Isonandrophyllum* Geyler; the genus *Achras* Linné (*Sapota* Plumier), now monotypic in the West Indies, has three species in the European Miocene; *Labatia* Swartz, with six existing species in the American tropics, has been doubtfully determined in the Miocene of Prussia and Italy; and Felix has described two forms of petrified wood which he refers to this family under the name *Sapotoxylon,* one species from Germany and the other from an unknown locality and horizon.

A large number of fossil forms of Sapotaceæ have been referred to the form-genus *Sapotacites* proposed by Ettingshausen (also *Sapotophyllum*). There are at least ten Upper Cretaceous forms widespread in North America and represented in Europe in the Perucer

beds of Bohemia and the *Credneria* stage of southern Saxony (Cenomanian). Three of these Upper Cretaceous forms are from the Tuscaloosa formation of Alabama and undoubtedly represent the ancestors of some of the Wilcox forms. There are about ten recorded species of *Sapotacites* in the Eocene of Australia, France and southern England. There are about a score of species in both the Oligocene and Miocene, most of which are European. There is, however, an undescribed species in the Apalachicola group of western Florida. In the Pliocene there are species in southern Europe and on the island of Java.

Notwithstanding the incompleteness of the record it is obvious that the family became well differentiated during the Upper Cretaceous and while it would not be safe to assign its place of origin to the American region, it is probable that at least several of the genera, such as *Bumelia* for example, originated in this region.

The family Ebenaceæ includes about eight genera and upwards of three hundred existing shrubs and trees, of which over half are referred to the genus *Diospyros* Linné. The family is mainly tropical as are most of the species of *Diospyros,* but the latter is represented in the north temperate zone in eastern North America, eastern Asia, and the Mediterranean region. The three modern monotypic genera, *Tetraclis, Brachynema* and *Rhapidanthe* are confined respectively to Madagascar, Brazil and West Africa and none have been found fossil. The genus *Royena* is mostly South African; *Euclea* is entirely confined to Africa: *Maba,* a large genus, ranges from Africa eastward to Polynesia; and *Macreightia* is common to tropical Africa and America.

Diospyros with about 180 existing species is cosmopolitan. Between 90 and 100 fossil forms have been described. In that grand display of dicotyledonous genera which during the mid-Cretaceous replaced the old Mesozoic flora of ferns, cycads, and conifers and which appeared with such apparent suddenness at a number of points in the northern hemisphere, we find unmistakable evidence of the abundance and wide distribution of species of *Diospyros.* No less than seventeen different forms have been described from the rocks of this age, and the localities where they have been found are scattered

from Australia to Bohemia, Greenland, and Vancouver Island. A large majority of these species are American, and they seem to have been especially at home along the Cretaceous coast of the Atlantic and along the border of the Mediterranean sea which extended northwestward from the Gulf of Mexico over much of our present Great Plains area. One of these species, well named *Diospyros primæva* by Professor Heer in 1866, is especially widespread and abundant, being found not only in Iowa, Kansas, and Nebraska in the west but also from Texas eastward through Alabama and northward in South Carolina, North Carolina, Maryland, New Jersey, Long Island and Greenland, or from latitude 33° to latitude 71° north. That these early persimmons were not very different from those of today is shown by their similar foliage. This resemblance is also shown by the fossilized remains of the calices of various species. One of these calices from another early Cretaceous species, recently described by the writer, is *Diospyros vera,* found in what is known in the Potomac River valley as the Raritan formation. Apparently the habit of accrescence had not been fully formed but the calyx was persistent then as now and entirely like a modern calyx in appearance. It was four-parted as it usually is in existing persimmons, but other fossil forms had a five-parted calyx like a good many present day tropical species.

In the Eocene epoch, which succeeded the Cretaceous, the records of the fossil occurrences of *Diospyros* show that it was truly cosmopolitan. These records include about 20 species in Siberia, Alaska and Greenland on the north; Canada, various localities in Europe, as well as Colorado, Montana, Wyoming, Nevada, Oregon, Washington, and other western states. Unfortunately, we have no Eocene or later Tertiary records along the Atlanic coast of North America outside the embayment region since the preserved deposits are all of marine origin and contain no fossil plants. There is little doubt, however, that *Diospyros* continued to be an abundant element in the aborescence flora of this area.

There are two well-marked species of *Diospyros* in the Wilcox flora, one of which continues in this region through the Claiborne.

A large calyx is present in the Claiborne or Vicksburg of southwes
ern Texas.

There are about 24 Oligocene species, *Diospyros* being especial common throughout southern Europe. There is an American sp cies of this age in the Apalachicola group of western Florida. Tl luxuriant forests of the Miocene have furnished about twenty sp cies of *Diospyros,* the known distribution at this time includes Eur pean localities from Spain to Hungary and American records Oregon, California, Yellowstone Park and Colorado. There a seven Pliocene species in southern Europe and in Java.

The allied genus *Royena* Linné has furnished splendidly pr served fruits from the oasis Chargeh in Egypt (Upper Cretaceous as well as four Oligocene and two Miocene species in Europe. seems never to have been cosmopolitan like *Diospyros,* since it h never been recognized in the western hemisphere. The fossil hi tory of the genus *Euclea* Linné was evidently similar to that of *Ro ena, i. e.,* it makes its appearance in the basal Oligocene of Euro where it is represented throughout the Oligocene and Miocene, b coming confined to Africa in Plio-Pleistocene times.

The genus *Macreightia* DC. has nine or ten existing species, o occurring in tropical Africa and the balance being American. *Ma reightia* is represented by both leaves and flowers in fossil floras a it has been a favorite receptacle for tripartite calices, not alwa of assured botanical identity. The oldest form is one in the Germa Oligocene and there are five or six species in the European Miocen It has not been definitely recognized in North America, althoug some of the Wilcox material is not unlike some European materi referred to *Macreightia.* Felix has recognized wood of this fami (*Ebenoxylon*) in the Oligocene of the Island of Antigua.

The order Gentianales (Contortæ of Engler) includes six fam lies with between four and five thousand existing species, the large family being the Asclepiadaceæ with upwards of two thousand sp cies. The families are complexly interrelated among themselve and with the next two orders, about the only constant characte being the opposite leaves and the generally twisted corolla in æstiva tion. The Asclepiadaceæ, not found in the Wilcox, shares with th

Apocynaceæ in the development of a latex-system and in other specializations, and the elaborate contrivances for entomophily in the former family reach a degree of complexity almost comparable with that of the Orchidaceæ. The Loganiaceæ, also not represented in the Wilcox flora, are lianas characteristic of South America and Asia and regarded by Engler as relatively primitive and possibly the ancestral stock of the Gentianales and Rubiales. The order as a whole is numerically massed in the tropics by reason of the many tropical genera of the two largest families—the Asclepiadaceæ and Apocynaceæ, which together contain three fourths of the existing species of the order.

The family Oleaceæ, sometimes considered as an order, the Oleales, contains 21 genera and about 400 existing species. There are three small genera peculiar to Asia and four peculiar to America, the remaining fourteen genera being found in more than one continental area. The three largest genera *Fraxinus* (40), *Mayepea* (50) and *Jasminum* (160) are all cosmopolitan. Eight of the twenty-one genera have been found fossil and it is evident that the family has an extended history, although there are no known Cretaceous records worthy of credence. Nor is the record well enough known to warrant generalizations. It is obvious from the early Eocene occurrence of leaves of *Fraxinus* associated with characteristic fruits, that the family must have been evolved before the close of the Upper Cretaceous but none of the genera have any well-marked or abundant known representation until Tertiary times.

The genus *Fraxinus* Linné has two species in the Wilcox flora, a characteristic samara, and foliage identical with that described by Heer from western Greenland as *Fraxinus Johnstrupi.* The latter furnishes an interesting instance of the extended distribution of members of the Eocene flora, at the same time illustrating the northward radiation of floras during the Eocene. Upward of ten additional Eocene species are known all of which are American and ranging from Tennessee to Alaska and Greenland. The Oligocene marks the appearance of the genus in Europe from which time to the present the genus has been represented throughout the warmer parts of the north temperate zone, at least four of the existing species making their appearance in the Pleistocene.

The second genus represented in the Wilcox flora is *Osman*
Lour. It has about ten existing species of eastern North Ame
eastern Asia and Polynesia. The Wilcox species is exceedi
close to *Osmanthus americanus* B and H of the Atlantic and (
coasts from North Carolina southward. A second fossil speci
found in the Miocene of Florissant, Colorado.

The old world genus *Phillyrea* Linné is found fossil in Eur
the genus *Notelæa* Vent., which has six existing Australian sp
and an isolated remnant of its former distribution in Madeira and
Canary Islands, is represented in the Eocene, Oligocene and Mio
of Europe; the genus *Olea* Linné with over thirty existing sp
about equally divided between Africa, Asia, and Australia and P
nesia, has about twenty fossil forms (including *Oleophyllum* (
wenz and *Oleæcarpum* Menzel) in Europe where they range in
from the basal Eocene through the Oligocene, Miocene and Plio
to the Pleistocene. The genus is not known in American f
floras but there is a supposed species in the early Tertiary
Australia.

The genus *Ligustrum* Linné with about 35 existing specie
southeastern Asia and the East Indies has three species in the
gocene and Miocene of Europe.[48] Saporta has described repres
atives of the genus *Syringa* Linné from the Sannoisian of sc
eastern France, the occurrence of the latter genus being base
floral remains.

The family Apocynaceæ comprises 133 genera and between
and eleven hundred existing species of perennial herbs, vines, sh
and trees, mostly with a milky acrid juice and simple exstip
leaves. The fruit is usually a pair of follicles or drupes and
seeds are often comatose. The family is almost equally divided
two subfamilies, the Plumeroideæ having 68 genera and about
species and the Echitoideæ having 65 genera and about 500 sp
The genera *Plumeria* Linné with about 40 species, and *Rauw*
Linné with about 45 species, are cosmopolitan, mostly tropical;
24 genera with about 300 species occur in more than one contin
area. America with 36 peculiar genera containing about 325 sp

[48] A species of *Ligustrum* recorded by Hollick from the Upper Creta
of Long Island is probably a *Pisonia*.

heads the list, followed by Africa with 28 peculiar genera containing about 130 species, and Asia with 20 peculiar genera containing about 75 species. Australia has few endemic genera or species, but numerous genera range from Asia or Africa to the Australian region and several genera are peculiar to Malaysia and to Polynesia. In the present state of our knowledge the distribution does not furnish material for generalization.

The fossil record, although including the representatives of at least a dozen genera, is too incomplete to shed much light on the history of the family or its existing distribution. The largest fossil genus is the form genus *Apocynophyllum* proposed by Heer and embracing fossil forms resembling *Thevetia, Cerbera, Apocynum* and other existing genera of the family. Five species are recorded from the Upper Cretaceous, coming from the Dakota sandstone, Australia, Westphalia and Saxony. There are over a score of Eocene species widely distributed. There are five species in the Wilcox flora some of which are exceedingly well marked and common. There are also five species in the Ypresian of southern England. Other Eocene records include Greenland, Australia, New Zealand and Chili. The score or more of known Oligocene species are confined to European localities. The Miocene species number about 25, all confined to Europe except a form recorded from Italy.

Fossil forms have been sparingly referred to the following genera: *Allamanda, Hæmadictyon* and *Thevetia* have been recognized by Engelhardt in the early Tertiary of Chili: *Alyxia, Alstonia, Cerbera* and *Tabernæmontana* have been recognized in the European Tertiary by various students: the genus *Neritinium* Unger has four or five species in the European Miocene: the genus *Plumeria* has four Miocene species in Europe and a Pliocene species in Brazil. The genus *Echitonium* Unger has over a dozen fossil species. There are five in the Eocene including a well marked form in the Wilcox flora; two in the Oligocene and five in the Miocene of Europe.

The genus *Nerium* Linné has only three or four existing species of shrubs or trees in the warmer parts of Eurasia. However the commonly cultivated *Nerium oleander* Linné of the Levant grows to a relatively large size and is extensively naturalized in Florida and the West Indies. It is used for hedges in Bermuda. Saporta re-

corded an Upper Cretaceous species, *Nerium Röhlii,* from the C panian of Westphalia but this is almost certainly a member of Myrtaceæ and not a *Nerium.* Undoubted species do occur in Eocene of Europe, including the remains of a characteristic fl from the Paris basin. There are several Oligocene and Miocene cies in Europe and the existing *Nerium oleander* or its imme ancestor occurs in the Pliocene of southern Europe in France Spain. The Wilcox species *Apocynophyllum tabellarum* is very gestive of *Nerium* but the genus is not certainly known in western hemisphere.

It may be noted that with the exception of the not cert identified species of *Apocynophyllum* the family is not represent the abundant known Upper Cretaceous floras of the world, w might mean that it originated somewhere in the southern hemisp

The order Polemoniales or Tubifloræ[49] contains the four fam Convolvulaceæ, Polemoniaceæ, Hydrophyllaceæ and Borragina The first three are characteristically American, the Convolvul being chiefly tropical, while the largest family, the Borraginace typically developed in the north temperate zone.

The family Borraginaceæ, the only one of the order known in Wilcox flora, contains about 85 genera and 1,600 existing speci mostly widely distributed north temperate herbs and shrubs trees in tropical countries, characterized by alternate, exstipu mostly entire leaves. The known fossil forms are few in nu and of slight significance and comprise for the most part Ter remains described as species of *Borraginites* and *Heliotropites.* family is represented in the Wilcox by two species of *Cordia,* a g containing about 230 existing species of shrubs and trees of the mer regions of both hemispheres, especially the western. There species in the Upper Cretaceous of the Misssissippi embayment (Tuscaloosa formation) and a Miocene species in Europe. E Tertiary forms are recorded from Chili by Engelhardt and Tasmania by Ettingshausen. The slight evidence available indi that the genus originated in the American tropics and that the of the family is of late Tertiary origin.

[49] Not the Tubifloræ of Engler which includes the orders Polemon and Personales, here regarded as distinct.

The order Personales or Labiatifloræ includes sixteen families distinguished from the Polemoniales by the zygomorphism of the flowers. The specific differentiation is great and the lines of descent are confusing. The largest families are the Labiatæ with over 3,000 existing species, the Scrophulariaceæ with about 2,500, the Acanthaceæ with about 2,000, and the Solanaceæ with about 1,800. Two of the sixteen families, the Verbenaceæ and Solanaceæ, are represented in the Wilcox flora.

The family Verbenaceæ includes about 73 genera and 1,300 existing species of widely distributed herbs, shrubs, or in tropical countries trees. The family is largely tropical or subtropical and is notably represented in the South American region. The fossil record is most incomplete. The largely old world genus *Clerodendron* Linné is unmistakably present in both the Eocene and Oligocene of Europe, and Ettingshausen has referred, somewhat doubtfully determined forms from the European Miocene to the American genus *Petræ* Linné and to the cosmopolitan genus *Vitex* Linné. The genus *Citharexylon* Linné has about twenty existing species ranging from the Florida keys and lower California through the American tropics to Bolivia and Brazil. A single species found in the middle and upper Wilcox is extremely close to the existing *Citharexylon villosum* Jacquin, a small coastal tree of the Florida keys, Bahamas and Antilles. With the exception of one or two doubtfully determined forms in the Miocene of southeastern Europe it is the only known fossil form.

The genus *Avicennia* Linné sometimes made the type of a distinct family, the Avicenniaceæ or Black-mangrove family, includes from three to thirty existing species according to the varying interpretation of different students. They are found on all tropical tidal shores. Two species have been recognized in the Wilcox flora, one based on leaves and the second on a not conclusively identified capsule.

The family Solanaceæ includes about seventy genera and about 1600 existing species, widely distributed and largely tropical, but extending into the temperate zone, notably in the western hemisphere. They comprise herbs, shrubs, vines, or in tropical countries often trees, with opposite, stipulate, toothed, lobed or dissected leaves.

Their fossil history is almost entirely unknown. The single Wilcox representative of the family is a flower described as *Solanites*, genus founded on the somewhat younger remains of a similar flower found in the Sannoisian of France, and comparable with the existing South American genus *Saracha* Ruiz & Pavon, as well as with *Witheringia, Solanum*, etc.

The last order of Gamopetalæ positively recognized in the Wilcox flora is the Rubiales which includes over 5,000 existing species segregated into five families, over four fifths being referred to the family Rubiaceæ—the only one represented in the Wilcox.

The Rubiaceæ includes about 355 genera and over 4,500 existing species of herbs, shrubs and trees; with simple, opposite or verticillate, mostly stipulate, leaves. They are widely distributed and largely tropical. While the Wilcox representation is confined to single species each of *Exostema, Psychotria* and *Guettarda*, great interest must attach to the fossil record of so highly organized a family which is my justification for introducing the following brief sketch of our knowledge of it.

No less than twenty-five genera have been recognized in the fossil state. With the exception of the very doubtful determination of species referred to *Rubiæphyllum* from the Turonian of Bohemia and doubtless representing a species of Ericaceæ, the family is unknown in the Upper Cretaceous. It is however represented in the early Eocene both in America and Europe. The Wilcox forms represent a species of *Exostema* Rich., close to the existing *Exostema caribaeum* R. & S. which ranges from the Florida keys to Central America. The genus comprises about twenty existing species of shrubs and small trees confined to the tropics and subtropics of America. The second Wilcox species is referred to *Guettarda* Endlicher, genus of about fifty species mostly confined to the American tropics but including one or two cosmopolitan tropical maritime species. The Wilcox form is very close to the existing *Guettarda elliptica* Swartz, a small tree of the Florida keys, Bahamas and West Indies. The third Wilcox species is *Psychotria grandifolia* described originally by Engelhardt from the early Tertiary of Chili. The genus *Psychotria* Linné comprises about 350 existing species of shrubs and small trees in tropical America. Asia and the East Indies, two thirds

of its species being American. The fossil form is compared with *Psychotria grandis* Swartz of the American tropics.

The genus *Coussarea* Aublet with about 40 existing species in the Brazilian region has been identified by Engelhardt from the early Tertiary of Chili. The genus *Hoffmannia* Swartz with about a score of existing American herbs or shrubs, mostly confined to Central America, has a fossil species in the early Tertiary of Chili. Likewise the genera *Sabicea* Aublet and *Gouatteria* Martius each have a single species in the Tertiary of Chili.

The Baltic amber (Sannoisian) has yielded a flower referred to *Sendelia* and a leafy twig referred to *Enantioblastos*. The genus *Galium*, comprising over 250 widely distributed existing herbaceous forms, has been doubtfully identified from the Eocene of Greenland. Its fruits are also not uncommon in Pleistocene deposits. The genus *Randia* Houst., embracing about one hundred existing species of shrubs or trees in all tropics, is identified by a fruit in the Aquitanian of Rhenish Prussia.

The genus *Rubiacites* so named by Webber from its resemblance to the existing forms of *Rubia* Linné has furnished three species of leaves and flowers in the Aquitanian of Prussia and Switzerland. The genus *Gardenia* Ellis, containing about sixty species of shrubs or rarely trees of the eastern hemisphere, is represented by characteristic fruits in the Sparnacian of France, the Aquitanian of Germany and England, the Miocene of Baden and Italy, and the Pliocene of Italy. The genus *Posoqueria* Aublet, which includes five or six existing South American shrubs or trees, is represented according to Unger by both leaves and fruits in the Miocene of Croatia. The genus *Ixora* Linné with one hundred existing species of shrubs and small trees in all tropics is likewise recorded from the Miocene of Croatia, as is also *Pavetta* Linné, a genus with about seventy existing species of shrubs or small trees of the Oriental tropics, which has furnished both leaves, flowers and fruits from the celebrated plant and insect beds of Radoboj in Croatia.

The genus *Coprosoma* Forst., with 40 existing species in Australia, New Zealand and Oceanica, was recorded by Ettingshausen from the Tertiary of Tasmania.

The genus *Nauclea* Linné, which has about thirty existing species

of shrubs and trees in tropical Asia and Oceanica, was identified by Unger in the European Miocene and petrified wood of this type (*Naucleoxylon*) was described by Crié from the Pliocene of Java.

The genus *Morinda* Linné has about thirty existing species in all tropics, especially in the Orient and the Pacific islands. A fossil species has been recorded from the Oligocene of Italy and five additional species based on leaves have been described from the Miocene of Croatia.

A fruit from the Tertiary lignites of Brandon, Vermont, has been described by Perkins as *Rubioides* and another from the Aquitanian of Rhenish Prussia by Menzel under the name *Rubiaceæcarpum.* Geyler has identified the old world genus *Grumilea* Gærtner in the Tertiary of Borneo, and finally the genus *Cinchonidium* proposed by Unger for fossil fruits and leaves which were very similar to those of the existing South American genus *Cinchona* Linné, has furnished a number of species. There are four or five in the Eocene, including the Fort Union of the western United States and the Ypresian of England; five in the late Oligocene of southeastern Europe; about eight Miocene species, one coming from the Esmeralda formation of Nevada and the balance being European.

The family is thus seen to have been well represented in fossil floras throughout the Tertiary, but the small proportion of the existing genera with fossil representatives and the incompleteness of the record of those with fossil representatives renders untrustworthy any generalizations that might be made from the present facts.

Under *Incertæ sedis* are grouped fourteen species of the Wilcox flora. These include two forms referred to *Calycites;* two to *Antholithus* and ten to *Carpolithus*. It would be quite useless to attempt any botanical discussion or comparison of these uncertain forms, such remarks as they suggest being more suitably confined to the discussion of the individual species.

Johns Hopkins University,
April 25, 1914.

SOLAR MAGNETIC PHENOMENA.[1]

By GEORGE E. HALE.

(Read April 24, 1914.)

The discovery by Stark of the electrical analogue of the Zeeman effect establishes a new point of view for the solar physicist. It is now known that an electric field, like a magnetic field, may cause the spectral lines of a light-source placed within it to break up into several components. Furthermore, these components, when observed at right angles to the lines of force, are plane polarized in both cases. Thus there are important points of resemblance between the Zeeman and Stark effects, and it becomes necessary to review the evidence on which the proof of the existence of solar magnetism is based. Is it possible that electric fields, rather than magnetic fields, are responsible for the observed spectroscopic phenomena?

Fortunately, as a brief consideration of the observations will show, this evidence is not open to the charge of ambiguity. The phenomena described in my papers on the magnetic fields of sun-spots and the general magnetic field of the sun are unmistakably those of the Zeeman effect. They are clearly ascribable, in their broad features, to magnetic rather than to electric fields, and if the latter exercise a secondary influence, it is not easily recognizable.

Here an important opportunity for further research is presented. The separation of electrons in sun-spots should give rise to electric fields, which may be sufficiently intense to produce an appreciable Stark effect. Other regions of the solar atmosphere where the conditions are most favorable for the production of electric fields are also open to investigation. But our knowledge of the spectroscopic

[1] Abstract. The complete details of the paper, which will be published in a series of articles in the *Astrophysical Journal*, include the results of investigations on the radial and tangential spot field; the rate of change of field-strength with level, both for spots and the general field; the relationship between field-strength and spot area; the complex fields of spot groups; the phenomena of bipolar spots, etc.

phenomena of all of these regions indicates that special methods of research will be required. It is true that the components of the hydrogen lines are much more widely separated by Stark's electric fields than by any magnetic fields yet produced. But electric fields sufficiently intense to produce such separation do not appear to exist in the sun.[2] Furthermore, when the observations can be made along the lines of force, it is easier to detect a magnetic field giving incomplete resolution of a line than an electric field causing equal overlapping of its constituent parts. This is because of the right-handed and left-handed polarization of the components: a characteristic feature which distinguishes the Zeeman effect from all other spectroscopic phenomena. The use of a quarter-wave plate in conjunction with a Nicol prism permits either component to be extinguished at will. Thus line displacements may be produced which are measurable with such precision as to disclose the existence of a magnetic field of only a few gausses. In fact, it might even be feasible, with special appliances, to detect the earth's field in this way. The absence of circular polarization prevents the observation of such displacements in the Stark effect, but the use of suitable apparatus may ultimately bring to light solar electric fields much weaker than those near the cathode of an ordinary vacuum tube. In any event, it will become possible to set an upper limit to the intensity of the electric fields existing in various parts of the sun.

Let us now review the evidence indicating the presence of magnetic fields in sun-spots, after recalling the hypothesis which led to the application of the tests for the Zeeman effect on Mount Wilson in 1908.' This hypothesis, based on the forms and motions of the dark hydrogen ($H\alpha$) flocculi revealed a few weeks earlier with our five-foot spectroheliograph, holds that sun-spots are vortex phenomena. The electrons emitted at high solar temperatures, if whirled in a vortex, must produce a magnetic field, assuming the positive and negative electrons to be unequal in number. The recent work of Harker justifies the view that negative electrons would flow from the hot vapors surrounding the vortex toward the cooler

[2] Unless the widening of lines in the chromosphere, especially that associated with eruptive phenomena, where strong electric fields may be present, should prove to be due in part to their influence.

vapors within it, thus providing the separation called for in the hypothesis. As for the existence of the vortex, in a form different from that first assumed, it is abundantly confirmed by the discovery of Evershed, and the subsequent observations of Evershed and St. John on the motion of vapors in the solar atmosphere surrounding spots.

Assume the axis of the vortex to coincide approximately with a solar radius. Then, if the spot were central on the sun, the lines of force at its center would lie in the line of sight. Such an iron line as λ 6302.709, which is resolved by a magnetic field into three components, should then appear in the spot as a doublet, the central component being absent when observed along the lines of force. The two outer components should be circularly polarized in opposite directions, and it should be possible to extinguish either one at will with the aid of a Nicol prism and quarter-wave plate. Furthermore, two spot vortices rotating in opposite directions should show the opposite components of the line, with the same adjustment of the polarizing apparatus.

This test was successfully applied, and has since been repeated on many sun-spots. Under the most favorable conditions, either component can be completely extinguished. In general, however, the observations cannot be made exactly along the lines of force, and under such circumstances the elliptically polarized components are not completely cut off. Moreover, such a line as λ 6302.709 usually appears as a triplet, the relative intensities of the central and side components varying, as would be expected from the Zeeman effect, with the angle between the lines of force and the line of sight.

Speaking generally, this angle should increase as the spot approaches the sun's limb. We should therefore be able in this case to distinguish the phenomena of plane polarization, since in the laboratory the three components are plane polarized when observed normal to the lines of force. As the central component is polarized in a plane at right angles to the plane of polarization of the side components, it should be possible to extinguish this line in the spectrum of a spot near the limb by rotating the Nicol prism, used without quarter-wave plate. This experiment has been successfully performed.

I need not dwell here on the other evidences of the Zeeman effect, but the proof is very complete. The resolution of the spot lines is not sufficiently perfect to permit the numerous components shown in some cases by laboratory observations to be detected, but triplets and quadruplets can be distinguished, and the resemblance of the observed effects to those of a magnetic field is very close for all lines. One of the most important tests is afforded by the steady decrease in the average separation of the components toward the violet, corresponding with the fact that in a magnetic field their separation is proportional to the square of the wave-length. Here we have a marked disagreement with the Stark effect, where the separation of the components *increases* toward the violet.

In the case of the sun's general magnetic field, my conclusions are also based exclusively upon displacements due to circular or elliptical polarization. This field, which is about eighty times as intense as that of the earth, but of only about one hundredth of the intensity of the maximum sun-spot field, is quite insufficient to separate the solar lines. In fact, the widening which it produces is much too small to be detected, and it is only through the possibility of cutting off one or the other component, and thus of producing a slight shift, that it can be measured.

In the Stark effect the absence of circular or elliptical polarization compels us to seek for evidence presented by changes in the width of lines. The hydrogen lines $H\beta$ and $H\gamma$, when observed for the transverse effect, have been shown by Stark to have five components, the three inner polarized at right angles, the two outer parallel to the field. In the longitudinal effect the two outer components are absent, while the three inner components are present but unpolarized. In the general electric field of the sun the lines of force may be regarded as radial. Hence all lines having Stark effects similar to those of the hydrogen lines should be wider near the limb than at the center of the sun, and their plane polarized outer edges should be capable of extinction by a Nicol. Lack of symmetry in the distribution of the components of a line, such as Stark has observed in some cases, would cause a shift of the lines near the limit. Tests made some time ago, in connection with the study of the

Zaeman effect, indicate that the well-known widening and displacement of the solar lines near the limb are not due primarily to this cause, though there may prove to be a second order effect smaller than I have yet been able to recognize.

A compound half-wave plate, made of narrow strips of half-wave mica, so mounted that (when used with a Nicol) the alternate strips will transmit light polarized in planes at right angles to one another, is to be strongly recommended for this purpose. This will permit the widths and the positions of the solar lines to be compared on a single photograph, in the way which has proved so advantageous in the study of the sun's general magnetic field.[3]

In a preliminary study of our photographs of sun-spot spectra, some of which were taken with the Nicol alone, I have been unable to detect any promising evidence of the Stark effect· However, these plates are poorly adapted for the purpose, and the investigation will soon be continued, and extended to various parts of the solar atmosphere.

[3] Contributions from the Mount Wilson Solar Observatory, No. 71.

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1914

THE AMERICAN PHILOSOPHICAL SOCIETY

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1. The candidate shall, on or before November 1, 1914, deliver free of postage or other charges, his discovery, invention or improvement, addressed to the President of the American Philosophical Society, No. 104 South Fifth Street, Philadelphia, U. S. A., and shall distinguish his performance by some motto, device, or other signature. With his discovery, invention, or improvement, he shall also send a sealed letter containing the same motto, device, or other signature, and subscribed with the real name and place of residence of the author.

2. Persons of any nation, sect or denomination whatever, shall be admitted as candidates for this premium.

3. No discovery, invention or improvement shall be entitled to this premium which hath been already published, or for which the author hath been publicly rewarded elsewhere.

4. The candidate shall communicate his discovery, invention or improvement, either in the English, French, German, or Latin language.

5. A full account of the crowned subject shall be published by the Society, as soon as may be after the adjudication, either in a separate publication, or in the next succeeding volume of their Transactions, or in both.

6. The premium shall consist of an oval plate of solid standard gold of the value of ten guineas, suitably inscribed, with the seal of the Society annexed to the medal by a ribbon.

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TO THE SECRETARIES OF THE

AMERICAN PHILOSOPHICAL SOCIETY

No. 104 SOUTH FIFTH STREET

PHILADELPHIA, U. S. A

PROCEEDINGS

OF THE

American Philosophical Society

HELD AT PHILADELPHIA

FOR PROMOTING USEFUL KNOWLEDGE

VOL. LIII. AUGUST–DECEMBER, 1914. No. 215.

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American Philosophical Society

General Meeting—April 22-24, 1915

The General Meeting of 1915 will be held on April 22nd to 24th, beginning at 2 p. m. on Thursday, April 22nd.

Members desiring to present papers are requested to send to the Secretaries, at as early a date as practicable, and not later than March 3, 1915, the titles of these papers, so that they may be announced in the final programme which will be issued immediately thereafter, and which will give in detail the arrangements for the meeting.

Owing to the embarrassment heretofore caused in a crowded programme by the receipt of titles at a very late date, the Committee of Arrangements announces, as a tentative plan, that additional papers can only be inserted in the *final* programme as there appears to be probable time for their presentation.

The Publication Committee, under the rules of the Society, will arrange for the immediate publication of the papers presented.

I. MINIS HAYS
ARTHUR W. GOODSPEED
AMOS P. BROWN
HARRY F. KELLER
Secretaries.

Members who have not as yet sent their photographs to the Society will confer a favor by so doing; cabinet size preferred.

It is requested that all correspondence be addressed

To the Secretaries of the
AMERICAN PHILOSOPHICAL SOCIETY
104 South Fifth Street
Philadelphia, U S. A.

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THE VEGETATION OF THE SARGASSO SEA.

BY WILLIAM G. FARLOW, PH.D., LL.D.

(*Read April 24, 1914.*)

On September 16, 1492, Columbus encountered masses of floating seaweed in latitude 28° N. 58° W. as he was approaching the Bahama Islands. This is the first record of the existence of what is now known as the Sargasso Sea. Since that date many navigators and travellers, who have traversed that region, have described the general appearance of the sea and have attempted to ascertain its limits and to explain the source from which the floating gulf-weed was derived. Unfortunately, however, the earlier accounts were often rather vague and to some extent conflicting and even well-known scientific men, as Humboldt, have been too much inclined to call attention to the sea as one of the wonders of nature rather than to attempt to record the facts about it accurately. Humboldt, for instance, described the Sargasso Sea as an area six times as large as Germany covered with a growth of a single species of seaweed which he regarded as very remarkable considering the small size of the land areas covered by the growth of a single species of plant. Although Humboldt's account is in a sense true, the impression that those who read his account receive is misleading.

The account given by Alexander Agassiz in 1888 was less sensational. He says:

"The Sargasso Sea of the North Atlantic covers a rather indefinite between 22° and 36° N. and according to the statements of the older gators, the amount of Sargassum to be met with varies from occas patches to masses large enough to impede the progress of sailing ve The Sargassum probably changes its position constantly, according to seasons, the currents, and the direction of the wind; but within the bounded by the Gulf Stream on the West, the equatorial current or South and the return current from the Azores and Canaries the Sarga has always been found in larger or smaller quantities."

At the present day we have a definite knowledge of the o currents and the prevailing winds of the Atlantic which are portant factors in the distribution of the gulf-weed. My pers experience, which has been confined to that part of the Ocean l between New York and Bermuda, agrees with that of most re travellers who have traversed the Sargasso Sea in various direct and it may be said that the gulf-weed occurs in scattered pat which are usually from fifty to, at the most, a few hundred fe diameter. It appears to be certain that in no place is the Sea cov by the gulf-weed in continuous masses miles in extent and it is often the case that the patches extend over a space as large a acre. Their long diameter is usually in the direction of the and their frequency varies very much according to circumstar Whether they are ever so dense as actually to impede modern sa vessels seems to me doubtful, and it must be said that those make the statement that the progress of vessels may be impede the gulf-weed usually preface their remarks by saying 'accordin older navigators' and do not depend on their own observations.

If at the present day we have a good topographical knowl of the Sargasso Sea, the question as to the particular specie which the gulf-weed belongs still presents several perplexing p lems and, as to the origin of the gulf-weed, as recently as Sauvageau wrote: "les causes de sa formation ne sont guère m connues que lors du premier voyage de Christophe Colomb." gassum, to which the gulf-weed, *Sargassum bacciferum*,[1] belon

[1] The name *Sargassum bacciferum* is used here since it has been name most commonly used to designate the gulf-weed. Boergesen in paper, "The species of *Sargassum* found along the coasts of the D West Indies with Remarks upon the Floating Forms of the Sargasso Copenhagen, 1914, states fully the reasons for preferring the name *gassum natans* (L.) on the ground of priority.

a large genus, the largest of all the genera of the larger brown seaweeds, and includes mainly species which inhabit the tropics, or more accurately between about 42° N. and 42° S. latitude. They flourish just below low water mark but do not grow in deep water. They are attached to the substratum by a hold-fast and grow not infrequently to be three feet long, with a branching, slender stem bearing leaves with small, stalked air-bladders near their bases. The fruit, the spores, are in cavities on special branches. The genus is a difficult one for the systematic botanist because, to be sure of a species, one should have not only the stem with its leaves but also the base and the fruit and in many cases species have been described from fragments only. Furthermore the individuals of most species vary very much so that, without a study of a set of living specimens, an algologist might be pardoned for believing that he had not one but several species before him, judging by herbarium specimens only. The points I have just mentioned must be borne in mind in what I have to say about the gulf-weed. It remains for us to consider the two questions: What is the gulf-weed and where does it come from?

All observations agree that the masses of floating gulf-weed consist in far the greater part of the single species, called *Sargassum bacciferum*. If, however, we examine more closely the traditional gulf-weed we find that although it has the characteristic leaves and bladders of the genus, it has no remains of a basal attachment and no fruit except in certain very rare and not well authenticated cases. Some believing that, if not impossible, it is certainly very improbable that any species could continue to flourish indefinitely like the gulf-weed without at some time fruiting and, furthermore, seeing a certain resemblance of the leaves and bladders to those of certain species of Sargassum growing attached in the West Indies and on the Florida coast, have advanced the opinion that the floating form called gulf-weed consists of branches broken from the attached forms and carried by the gulf stream to the different parts of the Sargasso Sea. Others maintain that this is not the only case of a plant living and flourishing without producing fruit, and that, since up to the present time, no one has found the *Sargassum bacciferum* attached and fruiting, we are forced to believe that it is a distinct but always sterile species and not a form of any other attached

species. This latter opinion is the one held by most recent wri

The question is not as simple as it seems at first sight. It be asked whether *Sargassum bacciferum* occurs in other places the Sargasso Sea and its immediate vicinity. What has been sidered to be this species has been reported to occur in New Zeal Australia, Java and various places in the Pacific and Indian Oc as well as Valparaiso but only scattered specimens have been f and there is no evidence whatever that there is more than one gasso Sea in the world and it may be questioned whether al specimens supposed to be *S. bacciferum* from other regions really the same as the Atlantic form. I have a specimen ma New Zealand which seems to be the real gulf-weed but the dat the label are scanty and I do not feel sure that the locality is rectly given. Von Marten's theory that the gulf-weed originat the Indian Ocean and was carried by currents round the Cap Good Hope to the Sargasso Sea has nothing to support it, nor the theory of Ed. Forbes that the floating gulf-weed is the sur of Sargassum growing on the submerged Atlantis be seriously sidered.

As a waif, or straggler, the gulf-weed is occasionally depo on the shores of northwestern Europe but in Great Britain, at l it must be very rare for in his Phycologia Britannica Harvey obliged to draw his figure of *L. bacciferum* from an American a British specimen. On the east American coast specimens of the weed are very rare. The only specimen which I have is a frag washed ashore at Bath, Long Island. Some years ago I was to a sea captain that there was a bank of gulf-weed off Nantucke I have been unable to obtain any confirmation of this staten Even if there is such a bank, the chances are that it is compose *S. filipendula*, which is very abundant on the adjacent shore of Cod.

As has been said, by far the greater part of the gulf-weed m is composed of *S. bacciferum*. That it is exclusively so is not Agardh states that *S. Hystrix* is found with *S. bacciferum* recently Boergesen has reported the same species near the D West Indies; *S. vulgare*, a very common attached species o West Indies has also been found with the gulf-weed. The mi

of the two species does not appear to be common in the Sargasso Sea itself but, as one approaches the land, the floating *S. vulgare* mixed with *S. bacciferum* is common and one finds both common even on the surface of landlocked waters like Harrington Sound, Bermuda.

A very interesting case is that of the mixture of a species of Cystoseira and gulf-weed collected by Professor F. H. Storer on a voyage from the Cape of Good Hope to New York. The exact position cannot be stated but according to information given by Professor Storer it was approximately 10° N. by 40° to 45° W. One gathering only was made and from it was obtained the specimens of *S. bacciferum* distributed in the "Algæ Americæ Borealis" of Farlow, Anderson and Eaton. This set has been seen by all the well known algologists of the world and, as no one has questioned the determination, it may be supposed to be correct. The Cystoseira was entangled in the Sargassum. The species of Cystoseira are complicated and not easy to name and the specimens in question were not in fruit. As far as I could tell, the species appeared to be very near *C. crinita* Bory, a Mediterranean species. Specimens have been examined by Sauvageau, the expert student of the genus, whose opinion is that in spite of certain points in common with *C. crinita* he would not venture to assert that they belong to that species. The interesting fact, however, is that, whether *C. crinita* or not, it must have come from the southeastern shore of Europe or of Northern Africa including the Atlantic islands since the species of Cystoseira abound in that region and, with one exception, none are found on the east coast of North America. *C. Myrica* is a rare species of Florida and the Bahamas and is quite different from the floating Cystoseira. As far as could be told from the material collected by Professor Storer, the Cystoseira in spite of its long journey was in as good a condition as the gulf-weed with which it was found. This is instructive as showing how far specimens can be transported by currents without perceptible injury.

In conclusion, in the limited time at our disposal, I shall show you a few slides of the gulf-weed and related species to illustrate more clearly some of the points I have mentioned. Everything considered it seems to me that in the present state of our knowledge we

are not as yet warranted in assuming that the floating gulf-w
could not have been derived originally from some fixed, frui
form. Certainly we do not at present know from what specie
might have been derived but, until the distribution of the Sarg
on the eastern coast of America and the West Indies is better kn
and the characteristics and variations of the various described spe
have been more thoroughly studied, the question of the origin of
gulf-weed seems to me to be still open.

THE KINETIC SYSTEM.

By GEORGE W. CRILE, M.D.

(Read April 22, 1914.)

In this paper I formulate a theory which I hope will harmonize a large number of clinical and experimental data, supply an interpretation of certain diseases, and show by what means many diverse causes produce the same end effects.

Even should the theory prove ultimately to be true, it will meantime doubtless be subjected to many alterations. The specialized laboratory worker will at first fail to see the broader clinical view, and the trained clinician may hesitate to accept the laboratory findings. Our viewpoint has been gained from a consideration of both lines of evidence on rather a large scale.

The responsibility for the kinetic theory is assumed by myself, while the responsibility for the experimental data is shared fully by my associates, Dr. J. B. Austin, Dr. F. W. Hitchings, Dr. H. G. Sloan and Dr. M. L. Menten.

Introduction.

The self-preservation of man and kindred animals is affected through mechanisms which transform latent energy into kinetic energy to accomplish adaptive ends. Man appropriates from environment the energy he requires in the form of crude food which is refined by the digestive system; oxygen is taken to the blood and carbon dioxid is taken from the blood by the respiratory system; to and from the myriads of working cells of the body, food and oxygen and waste are carried by the circulatory system; the body is cleared of waste by the urinary system; procreation is accomplished through the genital system; but none of these systems are evolved primarily for the purpose of transforming potential energy into kinetic energy for specific ends. Each system transforms such amounts of potential into kinetic energy as are required to perform its specific work; but no one of them transforms latent into kinetic energy for the

purposes of escaping, of fighting, of pursuing; or for combat infection. The stomach, the kidneys, the lungs, the heart strike physical blow—their rôle is to do certain work to the end that blow may be struck by another system evolved for that purpose. propose to offer evidence that there is in the body a system evolv primarily for the transformation of latent energy into motion a into heat. This system I propose to designate the Kinetic Syste

The kinetic system does not directly circulate the blood, nor d it exchange oxygen and carbon dioxid; nor does it perform the fu tions of digestion, urinary elimination and procreation; but thou the kinetic system does not directly perform these functions, it d play indirectly an important rôle in each, just as the kinetic syst itself is aided indirectly by the other systems.

The principal organs which comprise the kinetic system are brain, the thyroid, the suprarenals, the liver and the muscles. T brain is the great central battery which drives the body; the thyr governs the conditions favoring tissue oxidation; the supraren govern immediate oxidation processes; the liver fabricates and sto glycogen; and the muscles are the great converters of latent ene into heat and motion.

Adrenalin alone, thyroid extract alone, brain activity alone, a muscular activity alone are capable of causing the body temperat to rise above the normal. The functional activity of no other gl of the body alone, and the secretion of no other gland alone cause a comparable rise in body temperature—that is, increased fu tional activity; and no active principle derived from the kidney, liver, the stomach, the pancreas, the hypophysis, the parathyroid, spleen, the intestines, the thymus, the lymphatic glands or the bo can, *per se,* cause a rise in the general body temperature compara to the rise that may be caused by the activity of the brain or muscles, or by the injection of adrenalin or thyroid extract. Th too, when the brain, the thyroid, the suprarenals, the liver or muscles are eliminated, the power of the body to convert latent i kinetic energy is impaired or lost. I shall offer evidence tending show that an excess of either internal or external environmer stimuli may modify one or more organs of the kinetic system, that this modification may cause certain diseases. For exampl

alterations in the efficiency of the cerebral link may yield neurasthenia, mania, dementia; of the thyroid link,—Graves' disease, myxedema; of the suprarenal link,—Addison's disease, cardiovascular disease.

This introduction may serve to give the line of our argument. We shall now consider briefly certain salient facts which relate to the conversion of latent energy into kinetic energy as an adaptive reaction. The amount of experimental data is so large that they will later be published in a monograph.

The amount of latent energy which may be converted into kinetic energy for adaptive ends varies in different species, in individuals of the same species, in the same individual in different seasons; in the life cycle of growth, reproduction and decay; in the waking and sleeping hours; in disease and in activity. We shall here consider briefly the reasons for some of those variations and the mechanism which makes them possible.

Biologic Consideration of the Adaptive Variation in Amounts of Energy Stored in Various Animals.

Energy is appropriated from the physical forces of nature that constitute the environment. This energy is stored in the body in quantities in excess of the needs of the moment. In some animals this excess storage is greater than in other animals. Those animals whose self-preservation is dependent on purely mechanical or chemical means of defense, such animals as crustaceans, porcupines, skunks or cobras, have a relatively small amount of convertible (adaptive) energy stored in their bodies. On the contrary, the more an animal is dependent on its muscular activity for self-preservation the more surplus available (adaptive) energy there is stored in its body. It may be true that all animals have approximately an equal amount per kilo of chemical energy—but certainly they have not an equal amount stored in a form which is available for immediate conversion for adaptive ends.

Adaptive Variation in the Rate of Energy Discharge.

What chance for survival would a skunk have without odor; a cobra without venom; a turtle without carapace; or a porcupine

shorn of its barbs, in an environment of powerful and hostile carn vora? And yet in such a hostile environment many unprotect animals survive by their muscular power of flight alone. It is ev dent that the provision for the storage of "adaptive" energy is n the only evolved characteristic which relates to the energy of t body. The more the self-preservation of the animal depends motor activity, the greater is the range of variation in the rate of d charge of energy. The rate of energy discharge is especially hi in animals evolved along the line of hunter and hunted, such as t carnivora and the herbivora of the great plains.

Influences That Cause Variation in the Rate of Output Energy in the Individual.

Not only is there a variation in the rate of output of ener among various species of animals, but one finds also variations in t rate of output of energy among individuals of the same species. our thesis that men and animals are mechanisms responding to vironmental stimuli is correct, and further, if the speed of ener output is due to changes in the activating organs as a result of ada tive stimulation, then we should expect to find physical changes the activating glands during the cycles of increased activation. Wh are the facts? We know that most animals have breeding seaso evolved as adaptations to the food supply and weather. Hence the is in most animals a mating season in advance of the season of ma mum food supply so that the young may appear at the period wh food is most abundant. In the springtime most birds and mamm mate, and in the springtime at least one of the great activating glan is enlarged—the thyroid in animals and in man shows seasonal largement. The effect of the increased activity is seen in the son the courting, the fighting, in the quickened pulse and in a sligh raised temperature. Even more activation than that connected w the season is seen in the physical act of mating—when the thyroid known to enlarge materially, though this increased thyroid activi as we shall show later, is probably no greater than the increas activity of other activating glands. In the mating season the kine activity is speeded up; in short, there exists a state—a fleeting state of mild Graves' disease; in the early stages of Graves' disease, befo

the destructive phenomena are felt, the kinetic speed is high and life is on a sensuous edge. Not only is there a seasonal rhythm to the rate of flow of energy, but there is a diurnal variation, the ebb is at night, and the full tide in the daytime. This observation is verified by experiments which show that certain organs in the kinetic chain are histologically exhausted, the depleted cells being for the most part restored by sleep.

We have seen that there are variations in speed in different species, and that in the same species speed varies with the season of the year and with the time of day. In addition there are variations also in the rate of discharge of energy in the various cycles of the life of the individual. The young are evolved at high speed for growth, so that as soon as possible they may attain to their own power of self-defense; they must adapt themselves to innumerable bacteria; to food, and to all the elements in their external environment. Against their gross enemies the young are measurably protected by their parents; but the parents—except to a limited extent in the case of man—are unable to assist in the protection of the young against infectious disease.

The cycle of greatest kinetic energy for physiologic ends is the period of reproduction. In the female especially there is a cycle of increased activity just prior to her development into the procreative state. During this time secondary sexual characters are developed—the pelvis expands, the ovaries and the uterus grow rapidly, the mammary glands develop. Again in this period of increasing speed in the expenditure of energy we find the thyroid, the suprarenal and the hypophysis also in rapid growth. Without the normal development of the ovary, the thyroid and the hypophysis, neither the male nor the female can develop the secondary sexual characters, nor do they develop sexual desire nor show seasonal cycles of activity, nor can they procreate. The secondary sexual characters—sexual desire, fertility—may be developed at will—for example, by feeding thyroid products from alien species to the individual deprived of the thyroid.

At the close of the childbearing period there is a permanent diminution of the speed of energy discharge, for energy is no longer needed as it was for the self-preservation of the offspring before

adolescence, and for the propagation of the species during the pro creative period. Unless other factors intervene this reduction i speed is progressive until senescent death. The diminished size c the thyroid of the aged bears testimony to the part the activatin organs bear in the general decline.

We have now referred to variations in the rate of discharge c energy in different species; in individuals of the same species; i cycles in the same individual—such as the seasons of food suppl the periods of wakefulness and of sleep; the procreative period—an we have spoken of those variations caused artificially by thyroi feeding.

Thus far we have referred to the conversion for adaptive pu poses of latent into kinetic energy in muscular and in procreativ action. We shall now consider the conversion of latent into kineti energy in the production of heat,[1] and endeavor to answer the que tions which arise at once:—Is there one mechanism for the conve sion of latent energy into heat and another mechanism for its co version into muscular action? What is the adaptive advantage c fever in infection?

The Purpose and the Mechanism of Heat Production in Infections.

Vaughn has shown that the presence in the body of any alie protein causes an increased production of heat, and that there is r difference between the production of fever by foreign protein and b infections. Before the day of the hypodermic needle and of ex perimental medicine, the foreign proteins found in the body outsic the alimentary tract were brought in by invading microörganism Such organisms interfered with and destroyed the host. The bod therefore, was forced to evolve a means of protection against thes hostile organisms. The increased metabolism and fever in infectio might operate as a protection in two ways: the increased fever b interfering with bacterial growth, and the increased metabolism b breaking up the bacteria. Bacteriologists have taught us that ba teria grow best at the normal temperature of the body, hence feve

[1] We use the terms heat and muscular action in the popular sens though physicists use them to designate one and the same kind of energy.

would interfere with bacterial growth. With each rise of one degree centigrade the chemical activity of the body is increased ten per cent. In acute infections there is aversion to food and frequently there is vomiting. In fever, then, we have a diminished intake of energy, but an increased output of energy—hence the available potential energy in the body is rapidly consumed. This may be an adaptation for the purpose of breaking up the foreign protein molecules composing the bacteria. Thus the body may be purified by a chemical combustion so furious that frequently the host itself is destroyed. The problem of immunity is not considered here.

As to the mechanism which produces fever, we postulate that it is the same mechanism as that which produces muscular activity. Muscular activity is produced by the conversion of latent energy into motion, and fever is produced largely in the muscles by the conversion of latent energy into heat. We should, therefore, find similar changes in the brain, the suprarenals, the thyroid, and the liver, whatever may be the purpose of the conversion of energy—whether for running, for fighting, for the expression of emotion, or for combating infection.

We shall first present experimental and clinical evidence which tends to show what part is played by the brain in the production of both muscular and febrile action, and later we shall discuss the parts played by the suprarenals, the thyroid, and the liver.

Histologic Changes in the Brain-Cells in Relation to the Maintenance of Consciousness and to the Production of the Emotions, Muscular Activity and Fever.

We have studied the brain-cells in human cases of fever, and in animals after prolonged insomnia; after the injection of the toxins of gonococci, of streptococci, of staphylococci, and of colon, tetanus, diphtheria and typhoid bacilli; and after the injection of foreign proteins, of indol and skatol, of leucin and of peptones. We have studied the brains of animals which had been activated in varying degrees up to the point of complete exhaustion by running, by fighting, by rage and fear, by physical injury and by the injection of strychnia. We have studied the brains of salmon at the mouth of the Columbia River and at its headwater; the brains of electric fish,

the storage batteries of which had been partially discharged, and those the batteries of which had been completely discharged; t brains of woodchucks in hibernation and after fighting; the brai of humans who had died from anemia resulting from hemorrhag from acidosis, from eclampsia, from cancer, and from other chron diseases. We have studied also the brains of animals after the e cision of the suprarenals, of the pancreas, and of the liver.

In every instance the loss of vitality—that is, the loss of the no mal power to convert potential into kinetic energy—was accon panied by physical changes in the brain-cells. The converse was al true—that is, the brain-cells of animals with normal vital pow showed no histologic changes. The changes in the brain-cells we identical whatever the cause. The crucial question then become Are these constant changes in the brain-cells the result of work do by the brain-cells in running, in fighting, in emotion, in fever? other words, does the brain perform a definite rôle in the conversi of latent energy into fever or into muscular action; or are the brai cell changes caused by the chemical products of metabolism? Ha pily this crucial question was definitely answered by the followi experiment: The circulations of two dogs were crossed in such manner that the circulation of the head of one dog was anastomos with the circulation of the body of another dog and vice versa. cord encircled the neck of each so firmly that the anastomosing circ lation was blocked. If the brain-cell changes were due to the met bolic products, then when the body of dog "A" was injured, the bra of dog "A" would be normal and the brain of dog "B" would sho changes. Our experiments showed brain-cell changes in the bra of the dog injured and no changes in the brain of the uninjured do

The injection of adrenalin causes striking brain-cell changes- first, a hyperchromatism, then a chromatolysis. Now if adrenal caused these changes merely as a metabolic phenomenon and not a "work" phenomenon, then the injection of adrenalin into the car tid artery of a crossed circulation dog would cause no change in i circulation and its respiration, since the brain thus injected is in e clusive vascular connection with the body of another dog. In o experiment the blood-pressures of both dogs were recorded on a dru when adrenalin was injected into the common carotid. The adr

nalin caused a rise in blood-pressure, an increase in the force of cardiac contraction, increase in respiration, and a characteristic adrenalin rise in the blood-pressure of both dogs. The rise was seen first in the dog whose brain alone received adrenalin and about a minute later in the dog whose body alone received adrenalin. Histologic examinations of the brains of both dogs showed marked hyperchromatism in the brain receiving adrenalin, while the brain receiving no adrenalin showed no change. Here is a clear-cut observation on the action of adrenalin on the brain—and both the functional and the histological tests showed that adrenalin causes increased brain action. The significance of this affinity of the brain for adrenalin begins to be seen when I call attention to the following striking facts:

1. Adrenalin alone causes hyperchromatism followed by chromatolysis, and in overdosage causes the destruction of some brain-cells.

2. When the suprarenal glands are both excised and no other factor is introduced, the Nissl substance progressively disappears from the brain-cells until death. This far-reaching point will be taken up later.

Here our purpose is to discuss the cause of the brain-cell changes. We have seen that in crossed brain and body circulation trauma cause changes in the cells of the brain which is disconnected from the traumatized body by its circulation, but which is connected with the traumatized body by the nervous system. We have seen that adrenalin causes activation of the body connected with its brain by the nervous system, and histologic changes in the brain acted on directly by the adrenalin, but we found no brain-cell changes in the other brain through which the products of metabolism have circulated.

In the foregoing we find direct evidence that the brain-cell changes are not due to the products of metabolism. We shall now present evidence to show that the brain-cell changes are "work" changes. What work? We postulate that it is the work by which the energy stored in the brain-cells is converted into electricity or some other form of transmissible energy which then activates certain glands and muscles, thus converting latent energy into heat and motion. It has chanced that certain other studies have given an analogous and convincing proof of this postulate. In the electric fish a part of the muscular mechanism is replaced by a specialized structure for storing

and discharging electricity. We found "work" changes in t brain-cells of electric fish after all their electricity had been rapic discharged. We found further that electric fish could not dischar their electricity when under anesthesia, and clinically we know tl under deep morphia narcosis, and under anesthesia, the producti both of heat and of muscular action is hindered. The action of m phia in lessening fever production is probably the result of its pressing influence on the brain-cells, because of which a diminish amount of their potential energy is converted into electricity anc diminished electric discharge from the brain to the muscles shou diminish heat production proportionally. We found by experim that under deep morphinization brain-cell changes due to tox could be largely prevented; in human patients deep morphinizati diminishes the production of muscular action and of fever, and we shall see later conserves life when it is threatened by acute fections. The contribution of the brain-cells to the production heat is either the result of the direct conversion of their stored ener into heat, or of the conversion of their latent energy into electric or a similar force, which in turn causes certain glands and muscles convert latent energy into heat.

A further support to the postulate that the brain-cells contrib to the production of fever by sending impulses to the muscles found in the effect of muscular exertion, or of other forms of mo stimulation in the presence of a fever-producing infection. Unc such circumstances muscular exertion causes additional fever, a causes also added but identical changes in the brain-cells. Thyrc extract and iodin have the same effect as muscular exertion and inf tion in the production of fever and the production of brain-c changes. All of this evidence is a strong argument in favor of t theory that certain constituents of the brain-cells are consumed the work performed by the brain in the production of fever.

That the stimulation of the brain-cells without gross activity the skeletal muscles and without infection can produce heat is sho as follows:

(*a*) Fever is produced when animals are subjected to fear wit out any consequent exertion of the skeletal muscles.

(*b*) The temperature of the anxious friends of patients will rise while they await the outcome of an operation.

(*c*) The temperature of patients will rise as a result of the mere anticipation of a surgical operation.

(*d*) There are innumerable clinical observations as to the effect of emotional excitation on the temperature of patients. A rise of a degree or more is a common result of a visit from a tactless friend. There is a traditional Sunday increase of temperature in hospital wards. Now the visitor does not bring and administer more infection to the patient to cause this rise, and the rise of temperature occurs even if the patient does not make the least muscular exertion as a result of the visit. I observed an average increase of one and one eighth degrees of temperature in a ward of fifteen children as a result of a Fourth of July celebration.

Is the contribution of the brain to the production of heat due to the conversion of latent energy directly into heat, or does the brain produce heat principally by converting its latent energy into electricity or some similar form of transmissible energy which through nerve connections stimulates other organs and tissues, which in turn convert their stores of latent energy into heat?

According to Starling, when the connection between the brain and the muscles of an animal is severed by curare, by anesthetics, by the division of the cord and nerves, then the heat-producing power of the animal so modified is on a level with that of cold-blooded animals. With cold the temperature falls, with heat it rises. Such an animal has no more control over the conversion of latent energy into heat than it has over the conversion of latent energy into motion.

Electric stimulation done over a period of time causes brain-cell changes, and electric stimulation of muscles causes a rise in temperature.

SUMMARY.

In our crossed circulation experiments we found that the brain-cell changes were not due to waste products or to metabolic poisons. We found that in the production both of muscular action and of fever there were brain-cell changes which showed a quantitative relation to the temperature changes or to the muscular

work done. We observed that under deep morphinization the fe response or the muscular work done was either diminished or minated and that the brain-cell changes were correspondingly di ished or eliminated. We found also that brain-cell changes muscular work followed electric stimulation alone. I conclude, th fore, that the brain-cell changes are work changes.

We shall next consider other organs of the kinetic system in relation to muscular activity, to emotion, to consciousness, to s to hibernation, and to heat production.

The Suprarenal Gland.

In our extensive study of the brain in its relation to the pro tion of energy and consequent exhaustion caused by fear and r by the injection of foreign proteins, of bacterial toxins and of str nin; by anaphylaxis; by the injection of thyroid extract, of ad alin, and of morphin; we found that with the exception of mor each of these agents produced identical changes in the brain- As we believed that the suprarenal glands were intimately associ with the brain in its activities, we concluded that the suprare also must have been affected by each of these agents. To prove relation, we administered the above-mentioned stimuli to animals studied their effects upon the suprarenal glands by functional, h logical and surgical methods, the functional tests being made by non's method.

Functional Study of the Suprarenal Glands by Cann Method.

Our method of applying the Cannon test for adrenalin wa follows: (*a*) The blood of the animal's was tested before the app tion of the stimulus. If this test was negative, then (*b*) the stim was applied and the blood again tested. If this test was nega a small amount of adrenalin was added. If a positive reaction then given, the negative result was accepted as conclusive. (*c* the control test was negative, then the stimulus was given. If blood after stimulation gave a positive result for adrenalin, a se test of the same animal's blood was made twenty-five minute

more later. If the second test was negative, then the positive result of the first test was accepted as conclusive.

We have recorded sixty-six clear-cut experiments on dogs, which show that after fear and rage, after anaphylaxis, after injections of indol and skatol, of leucin and tyrosin, of the toxins of diphtheria and colon bacilli, of streptococci, and staphylococci, of foreign proteins and of strychnin, the Cannon test for adrenalin was positive. The test was negative after trauma under anesthesia, and after intravenous injections of thyroid extract, of thyroglobin and of the juices of various organs injected into the same animal from which the organs were taken. Placental extract gave a positive test. The test was sometimes positive after electric stimulation of the splanchnic nerves. On the other hand, if the nerve supply to the suprarenals had been previously divided, or if the suprarenals had been previously excised, then the Cannon test was negative, after the administration of each of the foregoing adequate stimuli. Blood taken directly from the suprarenal vein gave a positive result, but under deep morphinization the blood from the suprarenal vein was negative, and under deep morphinization the foregoing adequate stimuli were negative.

In brief, the agencies that in our brain-cell studies were found to cause hyperchromatism followed by chromatolysis, gave positive results in the Cannon test for adrenalin. The one agent which was found to protect the brain against changes in the Nissl substance—morphin—gave a negative result in the Cannon test for adrenalin. After excision of the suprarenals, or after division of their nerve supply, all Cannon tests for adrenalin were negative.

Histologic Studies of the Suprarenal Glands.

Histologic studies of the suprarenal glands after the application of the adequate stimuli which gave positive results to the Cannon tests for adrenalin are now in progress and thus far the histologic studies corroborate the functional tests.

In hibernating woodchucks, the cells of the adrenal cortex were found to be vacuolated, and shrunken. In 100 hours of insomnia, in surgical shock, in strong fear, in exhaustion from fighting, in peptone injections, in acute infections, the suprarenal glands undergo

histological changes characteristic of exhaustion. Alkalies c
suprarenal changes, but acids do not.

We have shown that brain and suprarenal activity go han
hand—that is, that the suprarenal secretion activates the brain,
that the brain activates the suprarenals. The fundamental ques
which now arises is this: Are the brain and the suprarenals in
dependent? A positive answer may be given to this question,
the evidence of the dependence of the brain upon the suprarena
as clear as is the evidence of the dependence of the suprarenals u
the brain. (1) After excision of the suprarenals, the brain-
undergo continuous histological and functional deterioration
death. During this time the brain progressively loses its powe
respond to stimuli and there is also a progressive loss of musc
power and a diminution of body temperature. (2) In our cro
circulation experiments we found that adrenalin alone could c
increased brain activity, while histologically we know that adren
alone causes an increase of the Nissl substance. An animal bot
whose suprarenals had been excised showed no hyperchromatism
the brain-cells after the injection of strychnin, of toxins, of for
proteins, etc. (3) When the suprarenal nerve supply was div
(Cannon-Elliott), then there was no increased suprarenal activit
response to adequate stimuli.

From these studies we are forced to conclude not only that
brain and suprarenals are interdependent, but that the brain is
tually more dependent upon the suprarenals than the suprare
upon the brain, since the brain deteriorates progressively to d
without the suprarenals, while the suprarenals whose connection
the brain has been broken by the division of their nerve supply
still produce sufficient adrenalin to support life.

From the strong affinity of the brain-cells for adrenalin w
was manifested in our experiments, we may strongly suspect
the Nissl substance is a volatile, extremely unstable combinatio
certain elements of the brain-cells and adrenalin because the su
renal glands alone do not take the Nissl stain and the brain depr
of adrenalin does not take Nissl stain. The consumption of the N
substance in the brain-cells is lessened or prevented by morphin
the output of adrenalin; and the consumption of the Nissl subst

is also lessened or prevented by nitrous oxid. But morphin does not prevent the action of adrenalin injected into the circulation, hence the control of morphin over energy expenditure is exerted directly on the brain-cells. Apparently morphin and nitrous oxid both act through this interference with oxidation in the brain. We, therefore, conclude that within a certain range of acidity of the blood adrenalin can unite with the brain-cells only through the mediation of oxygen, and that the combination of adrenalin, oxygen, and certain brain-cell constituents causes the electric discharge that produces heat and motion. In this interrelation of the brain and the suprarenals, we have what is perhaps the master key to the automatic action of the body. Through the special senses environmental stimuli reach the brain and cause it to liberate energy which in turn activates certain other organs and tissues, among which are the suprarenal glands. The increased output of adrenalin activates the brain to still greater activity, as a result of which again the entire sympathetic nervous system is further activated, as is manifested by increased heart action, more rapid respiration, raised blood-pressure, increased output of glycogen, increased power of the muscles to metabolize glucose, etc.

If this conclusion is well founded, we should find corroborative evidence in histologic changes in that great store-house of potential energy, the liver, as a result of the application of each of the adequate stimuli which produced brain-cell and suprarenal changes.

The Liver.

Prolonged insomnia, prolonged physical exertion, infections, injections of toxins, and of strychnin, rage and fear, physical injury under anesthesia, in fact all of the adequate stimuli which affected the brain and the suprarenals, produced constant and identical histologic changes in the liver—the cells stained poorly, the cytoplasm was vacuolated, the nuclei were crenated, the cell membranes were irregular, the most marked changes occurring in the cells of the periphery of the lobules. In prolonged insomnia the striking changes in the liver were repaired by one seance of sleep.

Are the histologic changes in the liver cells due to metabolism or toxic products or are they "work" changes incident to the conversion of latent into kinetic energy? Are the brain, suprarenals and liver

interdependent? The following facts establish the answers to tl queries:

1. The duration of life after excision of the liver is about same as after adrenalectomy—approximately eighteen hours.

2. The amount of glycogen in the liver was diminished in al the experiments showing brain-suprarenal activity; and when histologic changes were repaired, the normal amount of glycogen again found.

3. In crossed circulation experiments changes were found in liver of the animal whose brain received the stimulus.

From these premises we must consider that the brain, the su renals, and the liver are mutually dependent on each other for conversion of latent into kinetic energy. Each is a vital organ— equally vital. It may be said that excision of the brain may parently cause death in less time than excision of the liver or su renals, but this statement must be modified by our definition of de If all the brain of an animal be removed by decapitation, its b may live on for at least eleven hours if its circulation be maintai by transfusion. An animal may live for weeks or months after cision of the cerebral hemispheres and the cerebellum, while an o transfused animal may live many hours, for days, even after destruction of the medulla. It is possible even that the brain actu is a less vital organ than either the suprarenals or the liver.

In our research to discover whether any other organs shoul included with the brain, the suprarenals and the liver in this m ally interdependent relation, we hit upon an experiment which thr light upon this problem.

Groups of rabbits were gently kept awake for 100 hours by re of students,—an experiment which steadily withdrew energy caused not the slightest physical or emotional injury to any of th no drug, toxin, or other agent was given to them; they were gi sufficient food and drink. In brief, the internal and external e ronments of these animals were kept otherwise normal excepting the gentle stimuli which ensured continued wakefulness. This tracted insomnia gradually exhausted the animals completely, s to the point of death even. Some of the survivors were killed

mediately after the expiration of 100 hours of wakefulness, others after varying intervals.

Histological studies were made of every tissue and organ in the body. Three organs, the brain, the suprarenals, and the liver, and these three only showed histologic changes. In these three organs the histologic changes were marked, and were almost wholly repaired by one seance of sleep. In each instance these histologic changes were identical with those seen after physical exertion, emotions, toxins, etc. It would appear, then, that these three organs take the stress of life—the brain is the battery, the suprarenals the oxidizer, and the liver the gasoline tank. The clear-cut insomnia experiment corresponds precisely with our other brain-suprarenal observations.

With these three kinetic organs we may surely associate also the "furnace," the muscles in which the energy provided by the brain, suprarenals and liver, plus oxygen, is fabricated into heat and motion.

Benedict in his monumental work on metabolism has demonstrated that in the normal state, at least, variations in the heart beat parallel variations in metabolism. He and others have shown that all energy of the body, whether evidenced by heat or by motion, is produced in the muscles. In the muscles then, we find the fourth vital link in the kinetic chain. The muscles move the body, circulate the blood, effect respiration, and govern the body temperature. They are the passive servants of the brain-suprarenal-liver syndrome.

Neither the brain, the suprarenals, the liver, nor the muscles, however, nor all of these together, have the power to change the rate of the expenditure of energy; to make possible the increased expenditure in adolescence, in pregnancy, in courting and mating, in infections. No one of these organs, nor all of them together, can act as a pacemaker or sensitizer. The brain acts immediately in response to the stimuli of the moment; the suprarenals respond instantly to the fickle brain and the effects of their actions are fleeting; the liver contains fuel only and cannot activate, and the muscles in turn act as the great furnace, in which the final transformation into available energy is made.

Another organ—the thyroid—has the special power of governing the *rate* of *discharge* of energy; in other words, the thyroid is the pace-maker. Unfortunately, the thyroid cannot be studied to

advantage either functionally or histologically, for there is as ye available test for thyroidism in the blood as there is for adren and thyroid activity is not attended by striking histologic chan Therefore the only laboratory studies which have been satisfac thus far are those by which the iodin content of the thyroid has l established. Iodin is stored in the colloid lacunæ of the thyroid in combination with certain proteins is the active agent of the thyr

Beebe has shown that electrical stimulation of the nerve su of the thyroid diminishes the amount of iodin which it contains it is known that in the hyperactive thyroid in Graves' disease iodin content is diminished. The meagerness of laboratory stu however, is amply compensated by the observations which the geon has been able to make on a vast scale—observations which as definite as are the results of laboratory experiments.

The Thyroid.

The brain-cells and the suprarenal glands are securely conce from the eye of the clinician, hence the changes produced in then different causes escape his notice, but the thyroid has always l closely scrutinized by him. The clinician knows that every on the above mentioned causes of increased brain-cell, suprarenal, l and muscle activity may cause an increase in the activity of both normal or the enlarged thyroid; and he knows only too well tha a given case of exophthalmic goiter, the same stimuli which ex the brain, the suprarenals, the liver, and the muscles to increa activity will also aggravate this disease.

The function of the thyroid in the kinetic chain is best eviden however, by its rôle in the production of fever. Fever results f the administration of thyroid extract alone in large doses. In hyper-activity of the thyroid in exophthalmic goiter, one see marked tendency to fever; in severe cases there is daily fever. fact, in Graves' disease we find displayed to an extraordinary de an exaggeration of the whole action of the kinetic mechanism.

We have stated that in acute Graves' disease there is a tend to the production of spontaneous fever, and that there is a magni diurnal variation in temperature which is due to an increased ou of energy in even the normal reaction producing consciousness.

Graves' disease there is, therefore, a state of intensified consciousness, which is associated with low brain thresholds to all stimuli—both to stimuli that cause muscular action and to stimuli that cause fever. The intensity of the kinetic discharge is seen in the constant fine tremor. It is evident that the thresholds of the brain have been sensitized. In this hypersensitization we find the following strong evidence as to the identity of the various mechanisms for the production of fever. In the state of superlative sensitization which is seen in Graves' disease, we find that the stimuli that produce muscular movement, the stimuli that produce emotional phenomena and the stimuli that produce fever are as nearly as can be ascertained equally effective. Clinical evidence regarding this point is abundant, for in patients with Graves' disease we find that the three types of conversion of energy resulting from emotional stimulation, from nociceptor stimulation (pain), and from infection stimulation are, as nearly as can be judged, equally exaggerated. In the acute cases of Graves' disease the explosive conversion of latent energy into heat and motion is unexcelled by any other known normal or pathological phenomenon. Excessive thyroid secretion, as in thyrotoxicosis from functioning adenomata, and excessive thyroid feeding, cause all the phenomena of Graves' disease except the exophthalmos and the emotional facies. Ligation of arteries, division of nerve supply and excision of part of the gland may reverse the foregoing picture and restore the normal condition. The patient notes the effect on the second day and often within a week is relatively quiescent. On the contrary if there is thyroid deficiency there is the opposite state, a reptilian sluggishness.

At will, then, through diminished, normal or excessive administration of thyroid secretion, we may produce an adynamic, a normal, or an excessively dynamic state. By the thyroid influence, the brain thresholds are lowered and life becomes exquisite; without its influence the brain becomes a globe of relatively inert substance. Excessive doses of iodin alone cause most of the symptoms of Graves' disease. The active constituent of the thyroid is iodin in a special protein combination. Thus is stored in the colloidal spaces. Hence one would not expect to find changes in the cells of the thyroid gland as a result of increased activity unless it be prolonged.

We have thus far considered the normal rôles played by the br the suprarenals, the liver, the muscles and the thyroid in transfor ing latent into kinetic energy in the form of heat and motion as adaptive response to environmental stimuli.

The argument may be strengthened, however, by the discuss of the effect of the impairment of any of these links in the kin chain upon the conversion of latent into kinetic energy.

Effect Upon the Output of Energy of Impaired or Lost Fu tion of Each of the Several Links in the Kinetic Chain.

1. *The Brain: Cerebral softening.*—In cerebral softening may find all the organs of the body comparatively healthy except the brain. As the brain is physically impaired it cannot norm stimulate other organs to the conversion of latent energy into h or into motion, but on the contrary in these cases we find feeble m cular and intellectual power. I believe also we find that in patie with cerebral softening, infections such as pneumonia show a lo temperature range than in patients whose brains are normal.

2. *The Suprarenals.*—In such destructive lesions of the sup renal glands as Addison's disease one of the cardinal symptoms i subnormal temperature and impaired muscular power. Animals u whom double adrenalectomy has been performed show a strik fall in temperature, muscular weakness—after adrenalectomy animal may not be able to stand even—and progressive chroma lysis. The significance of the last will be pointed out later.

3. *The Liver.*—When the function of the liver is impaired tumors, cirrhosis, or degeneration of the liver itself, then the ent energy of the body is correspondingly diminished. This diminut of energy is evidenced by muscular and mental weakness, by dim ished response and by a gradual loss of efficiency which fina reaches the state of asthenia.

4. *The Muscles.*—It has been observed clinically that if muscles are impaired by long disuse, or by a disease such as my thenia gravis, then the range of production of both heat and mot is below normal. This is in agreement with the experimental fi ings that anesthetics, curare, or any break in the muscle-brain c nection causes diminished muscular and heat production.

5. *The Thyroid.*—In myxedema one of the cardinal symptoms is a persistently subnormal temperature and though prone to infection, subjects of myxedema show but feeble febrile response and readily succumb. This clinical observation is strikingly confirmed by laboratory observations; normal rabbits subjected to fear showed a rise in temperature of from one to three degrees while two rabbits whose thyroids had been previously removed and who had then been subjected to fright showed much less febrile response. Myxedema subjects show a loss of physical and mental energy which is proportional to the lack of thyroid. Deficiency in any of the organs of the kinetic chain causes alike loss of heat, loss of muscular and emotional action, of mental power and of the power of combating infections—the negative evidence thus strongly supports the positive. By accumulating all the evidence we believe we are justified in associating the brain, the suprarenals, the thyroid, the muscles and the liver as vital links in the kinetic chain. Other organs play a rôle undoubtedly, though a minor one. If our conclusions are sound, then in the kinetic system we should find an explanation of many diseases, and having found an explanation, we may find new methods of combating them.

Kinetic Diseases.

In the foregoing conclusions we find a simple explanation of certain diseases. When the kinetic system is driven at an overwhelming rate of speed—as by severe physical injury, by intense emotional excitation, by perforation of the intestines, by the pointing of an abcess into new territory, by the sudden onset of an infectious disease, by an overdose of strychnin, by a Marathon race, by a grilling fight, by foreign proteins, by anaphylaxis,—the result of these acute overwhelming activations of the kinetic system is clinically designated shock, and according to the cause is called traumatic shock, toxic shock, anaphylactic shock, drug shock, etc.

The essential pathology of shock is identical whatever the cause. If, however, instead of an intense overwhelming activation, the kinetic system is continuously or intermittently overstimulated through a considerable period of time, as long as each of the links in the kinetic chain takes the strain equally the result will be excessive

energy conversion, excessive work done; but usually, under st some one link in the chain is unable to take the strain and then evenly balanced work of the several organs of the kinetic syste disturbed. If the brain cannot endure this strain, then neurasth nerve exhaustion, or even insanity follows. If the thyroid ca endure the strain it undergoes hyperplasia, which in turn may r in a colloid goiter or in exophthalmic goiter. If the suprarenals not endure the strain, cardiovascular disease may develop. If liver cannot take the strain then death from acute acidosis may low, or if the neutralizing effect of the liver is only partially then the acidity may cause Bright's disease. Over-activation of kinetic system may cause glycosuria and diabetes.

Identical physical and functional changes in the organs of kinetic system may result from intense continued stimulation f any of the following causes, excessive physical labor, athletic e cise, worry or anxiety, intestinal auto-intoxication, chronic infect such as oral sepsis, tonsillitis and adenoids; chronic appendic chronic cholecystitis, colitis, and skin infections; the excessive take of protein food (foreign protein reaction); emotional st pregnancy, stress of business of professional life—all of which known to be activators of the kinetic system.

From the foregoing statements we are able to understand the n cular weakness following fever; we can understand why the se have neither muscular power nor strong febrile reaction; why lo continued infections produce pathologic changes in the organs c stituting the kinetic chain; why the same pathologic changes re from various forms of activation of the kinetic system. In hypothesis we find a reason why cardiovascular disease may caused by chronic infection, by auto-intoxication, by overwork, o emotional excitation. We now see that the reason why we find much difficulty in differentiating the numerous acute infections f each other is because they play upon the same kinetic chain. (postulate harmonizes the pathological democracy of the kin organs, for it explains not only why in many diseases the pathol ical changes in these organs are identical, but why the same chan are seen as the result of emotional strain and overwork. We

thus understand how either emotional strain or acute or chronic infection may cause either exophthalmic goiter or cardiovascular disease; how chronic intestinal stasis with the resultant absorption of toxins may cause cardiovascular disease; neurasthenia or goiter. Here is found an explanation of the phenomena of shock, whether the shock be the result of toxins, of infection, of foreign proteins, of anaphylaxis, of psychic stimuli, or of a surgical operation with its combination of both psychical and traumatic elements.

This conception of the kinetic system has stood a crucial test by making possible the shockless surgical operation. It has offered a plausible explanation of the cause and the treatment of Graves' disease. Will this kinetic theory stand also the clinical test of controlling that protean disease bred in the midst of the stress of our present-day life? Present-day life, in which one must ever have one hand on the sword and the other on the throttle, is a constant stimulus of the kinetic system. The force of these kinetic stimuli may be lessened at the cerebral link by intelligent control—a protective control is empirically attained by many of the most successful men. The force of the kinetic stimuli may be broken at the thyroid link by dividing the nerve supply, reducing the blood supply, or by partial excision; or if the suprarenals feel the strain, the stimulating force may be broken by dividing their nerve supply, reducing the blood supply, or by partial excision. No theory is worth more than its yield in practice, but already we have the shockless operation, the surgical treatment of Graves' disease, the control of shock and the acute infection by overwhelming morphinization.

Conclusions.

To become adapted to their environment animals are transformers of energy. This adaptation to environment is made by means of a system of organs evolved for the purpose of converting potential energy into heat and motion. The principal organs and tissues of this system are the brain, the suprarenals, the thyroid, the muscles and the liver. Each is a vital link—each plays its particular rôle and one cannot compensate for the other. A change in any link

of the kinetic chain modifies proportionately the entire kin
system, which is no stronger than its weakest link.

In this conception we find a possible explanation of many disea
—one which may point the way to new and more effective th
peutic measures than those now at our command.

Cleveland, O.,
April 23, 1914.

NEWER ASPECTS AND METHODS IN THE STUDY OF THE MECHANISM OF THE HEART-BEAT.

By ALFRED E. COHN, A.B., M.D.

(*Read April 23, 1914.*)

The interest now very widespread, in the physiology of the heart-beat developed from certain observations which Carlo Matteucci made some seventy-two years ago, and which he communicated in 1842 to the Academy at Paris. He established the fact that the muscle of a nerve-muscle preparation contracted if its nerve were laid across a second muscle which had been made to contract. He believed that the stimulus which the nerve received, and which it conveyed to its attached muscle was electrical in nature. Thirteen years later, in 1855, Kölliker and H. Müller widened the scope of Matteucci's observations by demonstrating in the same way that, if the nerve of a similar nerve-muscle preparation were laid across a heart, the muscle of the preparation likewise contracted, because, as in Matteucci's experiment, a current, called a current of action, was discharged from the contracting heart and was conveyed to the muscle.

These discoveries continue to be the primary subjects of experiment in contemporary studies in the mechanism of the heart-beat. The first experiments dealing with action currents were made by Marchand, Engelmann, and by Burdon-Sanderson and Page, who used a Bernstein rheotome in their investigations, but later the use of the capillary electrometer of Lippmann was introduced, especially by Marey, Waller, by Bayliss and Starling, Gotch and others. Marey in 1876 was the first to obtain permanent records of the action currents of the heart by photographing the motions of the meniscus of the mercury column of the electrometer on a moving sensitive surface. This record was a continuous curve in which could be distinguished various waves, one of which has been identified as synchronous with the contraction of the auricles, the upper chambers

of the heart, and certain others with the contraction of the v tricles, the lower pair of heart chambers. At first all records w obtained by applying electrodes directly to the surface of the he as it lay exposed in the opened chest, but in 1889 Waller showed t one could obtain records of these currents by applying suitably c structed electrodes to the surface of the body without opening injuring it. This discovery opened the way for studying these curre in the human subject. Waller also showed which were favorable which unfavorable locations for placing electrodes, by varying the uation at which they were applied. From a consideration of records

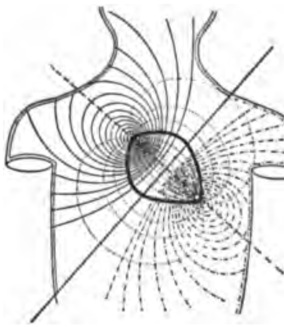

FIG. 1. After Waller. To indicate the spread of the cardiac ac current through the human subject.

tained from a number of positions, now called leads or derivations determined the location of a plane, on the two sides of which greatest differences in potential were developed (Fig. 1). Th locations were called favorable which yielded curves showing largest waves, and the records were called electrocardiograms. differences between electrocardiograms taken indirectly in this and those taken directly from the surface of the heart are ones contour and do not involve the important time relations of various elements composing the curve.

With the knowledge that the heart discharged action curre and a method for conducting these from the uninjured surface the body to a registering instrument, the time was ripe for the struction of a galvanometer better fitted to the purposes of phy logical and medical research. W. Einthoven of Leyden in 190 described and built this instrument. Its completion at that time

especially fortunate, for fresh discoveries in anatomy and physiology were almost simultaneously announced. In the interpretation of the significance of these, the galvanometric method of registration was especially valuable.

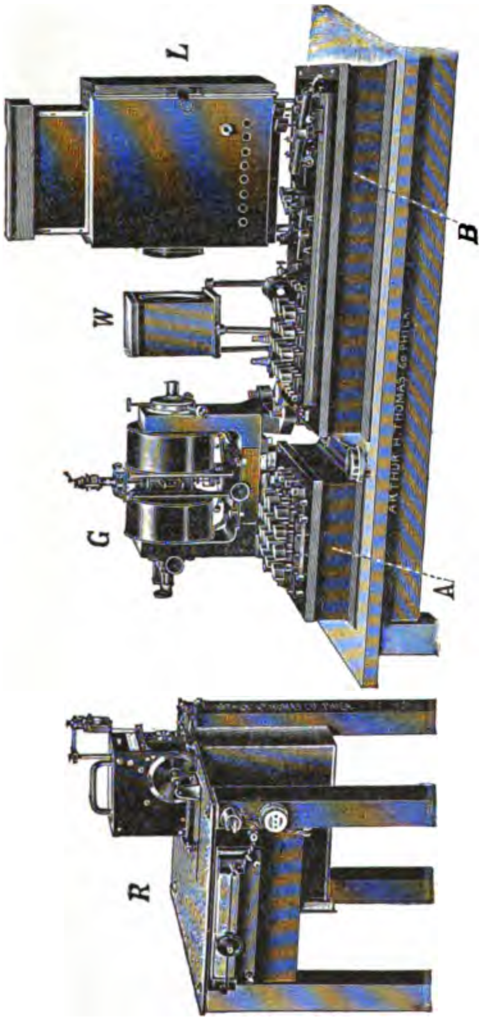

Fig. 2. String galvanometer, Edelmann's model. *R*, photographic recorder; *G*, galvanometer; *L*, arc light; *W*, water-bath; *A* and *B*, resistance and compensating boxes.

The principles Einthoven employed were in use in Deprez-D'Arsonval's instrument. From these he developed formulæ which formed the basis of the galvanometer he constructed. An instru-

ment built on similar lines for use as an ocean telegraph reco had already been devised by Ader, but of this he was unaware at time. It depends on the principle that a conductor suspended magnetic field is deflected at right angles to the lines of force w a current passes through it. The conductor chosen is usuall silvered quartz or platinum thread, 87 to 100 mm. long, 3 to 6 m thick, having a resistance of 3,000 to 6,000 ohms It is suspen vertically between the poles of two powerful electro-magnets (2). The thread is illuminated by the rays of an arc light which focused on it by a system of lenses and a substage condensor. accommodate this condensor the pole of one electro-magnet is bo The motions of the string are magnified and projected on a rec ing photographic surface by a microscope held in a similar bor the pole of the other electro-magnet. The degree of magnifica may be varied according to the needs of the investigator, but order to maintain a degree of uniformity in the appearance an the electrical value of curves obtained in different laboratories, tain arrangements have become conventional. These include strength of the magnetic field, the tension of the string and its flection time. The strength of the field depends, of course, on construction. The tension of the string is adjusted in an approp manner so that a current having the value of one millivolt, w allowed to pass through it, causes a deflection of 1 cm. It has found desirable to obtain a deflection of this extent within a def length of time, usually 0.02 seconds or less. When the deflec time is longer, certain waves in the electrocardiogram tend to appear.

But in order to obtain an electrocardiogram, more is neces than to include an individual in the string circuit. For althoug usual cardiac action current does not deflect the string, when it the prescribed tension, beyond the optical field, the skin or cons current, which is also continuously discharged from the body, which is composed of the summed discharges from the other trically active tissues of the organism, may do so, and it is usu sufficiently great to deflect the string far beyond the field of microscope. This current does not show the rapid changes of pote difference that the action current from the heart does. On

whole, its strength is uniform and changes so little within small limits of time as to render the change negligible. There is consequently no danger of confusing it with the rapid changes in electrical potential which compose the electrocardiogram. In order to keep the string in the optical field, from which the constant or skin current tends to deflect it, a system of compensation has been found necessary. This system comprises another source of current, a commutator and a series of resistances which are introduced into the string-heart circuit. The action current to be recorded, and with it the constant current, are permitted by a shunt to pass through the string in increasing amounts, so enabling one to allow a sufficient electromotive force of opposite polarity to the skin current to enter the circuit. Compensation in routine examinations becomes a simple procedure. To complete the records, a time curve and a millimeter scale are photographed on the record.

The most fruitful method of investigating the identity of the parts of the electrocardiogram has probably been that of recording synchronously on the same curve the electrocardiogram and mechanical curves representing the motions of the heart. As the result of these studies, it has been concluded that the contraction of the auricles is represented electrically by the *P* wave, the first wave in the cardiac cycle (Fig. 4), while the Waves *Q, R, S, T,* which follow it form a group in the electrocardiogram which are associated with ventricular activity. The term ventricular activity is purposely chosen, for there is as yet no uniformity of opinion in respect to designating which ventricular function it is which this complex of waves represents. Opinion is divided as to whether the complex represents the act of conduction, the state of muscular irritability, or actual contraction. It is doubtless unprofitable to analyze this discussion, and probably quite impossible to decide between these interpretations now. Of this much one can be certain, that the *QRST* complex does not occur unless the ventricles have been seen or known to contract. The significance of the individual waves of this group is also still a matter wrapped in doubt. Most writers favor the view that the wave *Q,* when present, signifies that the earliest ventricular activity has taken place near the apex of the heart, that the *R* wave represents the assumption of predominance

by the ventricular base, while S indicates a return of activity apex. The significance of the T wave is a hotly disputed Some hold it to represent the return of activity to the base ventricles in the later part of systole, because of an analogy w drawn between the arterial base of the heart and the distal the primitive cardiac tube, which is, of course, the last segm become affected by the wave of peristalsis which passes o The most important arguments against this interesting assu have been supplied by Garten and his pupils, Clement and Er These investigators have all shown that the T wave occurs same instant of time at all points on the heart's surface, and its occurrence, as does also Einthoven, to a function inher muscular contraction. According to these authors, it represe second wave in a current essentially diphasic. One need s point out the fact that its presence simultaneously througho cardiac surface precludes the possibility of its occurring as t phase of a peristaltic contraction.

Aside from the auricular representative and the group of representing ventricular activity, two other portions of an e cardiographic curve must be described. The less debated o is the portion following the T wave, the isoelectric period b the end of T and the beginning of P. It represents the dias the heart cycle,—from the end of the ventricular to the begin the next auricular contraction,—the rest period of the heart. other portion is that lying between the wave P and the co $QRST$; this portion also is usually isoelectric, but occasiona Einthoven has pointed out, its level departs from the base line the period which represents the time occupied by the passage pulses from the contracting auricles to the beginning of vent activity, and is called the conducting period.

We must now return to consider those other newer aspects study of the mechanism of the heart-beat to which I have re in speaking of recent anatomical and physiological contrib Before 1883 the theories held to explain coördination betwe upper or auricular pair of the cardiac chambers and the lowe tricular pair consisted principally of an old notion of Haller's effect that the ventricles contracted in response to stimuli co

to them by the act of filling, while others held and some do still that coördination between the two pairs rested on a carefully adjusted mechanism involving the passage of impulses over nervous channels. The need for theories of this sort lay in the fact that no muscular connection between the auricles and ventricles was known to exist. But in 1883, W. Gaskell convinced himself that the conduction of impulses in the heart must pass over muscular pathways, and Wooldridge and Tigerstedt contributed experiments which pointed to the

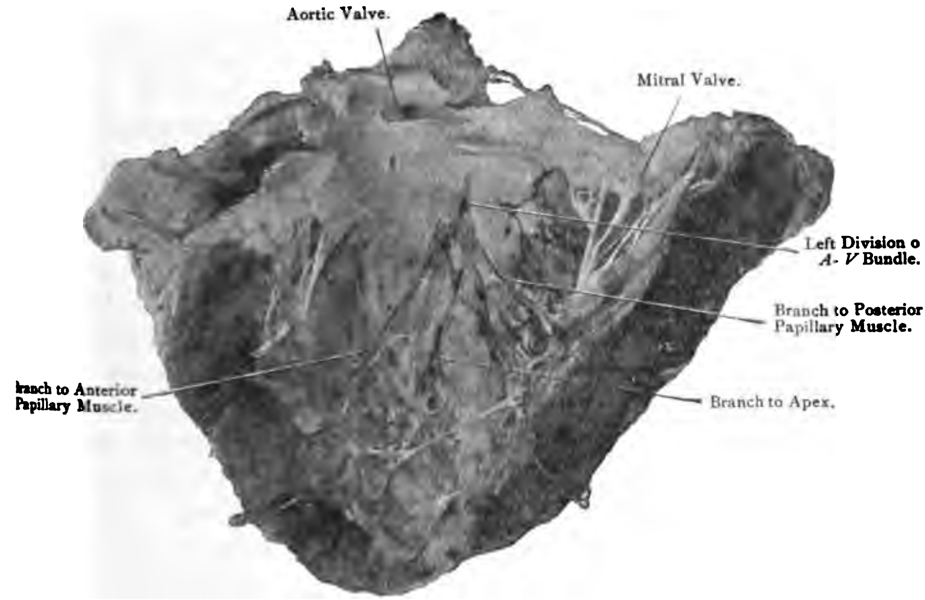

Fig. 3. Ox-heart. Injection with dilute India ink of the bursa-like spaces surrounding the left branch of the *A–V* bundle.

probability of Gaskell's contention. Ten years later (1893) Stanley Kent and His, Jr., actually saw and described a bundle of connecting muscular fibers. Since then (1893–1908) the existence of this structure, best called the auriculo-ventricular bundle, has been sufficiently confirmed. It passes from the lowest level of the auricles, divides into two and supplies a branch to each ventricle (Fig. 3). It has a peculiar muscular structure. In some species it contains large neural elements, but in man and the higher mammals only fine nerve fibrillæ

are found within it. With the establishment of the fact that auriculo-ventricular bundle possessed this mixed structure, discus as to whether conduction was nervous or muscular has gone ou fashion. Its essential function consists in conducting impulses so coördinating the rhythms of the auricles and ventricles. To p that it actually performs this function, its structure has been stroyed in experiments. In these the result anticipated was reali auricles and ventricles continued to beat, each at a rate of its and each in a rhythm without reference to the other. Two c facts are known about this bundle; first, that it can conduct imp in a backward direction (from ventricles to auricles) as well as wards; and second, that it conducts a little slower, especially a upper terminal, than the rest of the heart muscle. We shall re to a consideration of this structure in relation to electrocardiogra

In 1906 another structure situated at the junction of the sup vena cava and the right auricular appendix was discovered by I and Flack. It is a small structure called the sino-auricular I It has a sectional area of 0.3 by 0.1 cm., and attains a length of 2.0 to 3.0 cm. Its existence has been abundantly verified. Lik conduction bundle, it also contains large and fine nerve elem The comparative anatomy of both these structures has been tr by Ivy Mackenzie and by Külbs, while the embryology has studied by Professor Mall. It is the discovery of these two s tures, the sino-auricular node and the auriculo-ventricular sys which has added a second new and significant chapter to our sto information relating to the mechanism of the heart-beat.

The sino-auricular node has been recognized as the stru which usually initiates impulses for the contraction of the w heart, and at the same time sets and maintains its rate. These p erties have been attributed to it because excision or exclusion o node from function reduces the rate of the heart-beat, and s times even stops it. Afterwards it begins gradually to con again, but the original rate is not restored. It can usually be sh that another portion of the heart now sets the pace. Other met have been employed to ascertain its functions; warmth applied t site of the node accelerates the rate of the heart; cooling slow Attempts to alter the temperature elsewhere of the surface do

have this effect. But more important information still is gained by means of the galvanometer. It has been demonstrated that each heart-beat begins at the node, because artificial contractions due to stimuli applied here yield curves of a shape identical with those resulting from spontaneous discharges; similar stimuli applied elsewhere fail of this identity. And finally, the law that the site at which contraction begins is primarily negative to other portions of the same strip of muscle is valid here. Lewis and Oppenheimer, Wybauw, Clement and Sulze have all been able to show that in contraction, the site of the node is primarily negative to all other portions of the auricular surface.

We have described the newly discovered structures in the mammalian heart. The function of the sinus node, in so far as it is now known, is to initiate impulses for the whole heart and to determine their rate; Lewis has aptly called it the pacemaker. The auriculo-ventricular bundle provides for coördination in the complicated mechanism of the heart. We must next show how, in the light of these structures, the electrocardiogram has been employed in elucidating the mechanism of the heart-beat. Althought the sinus node and the conduction bundle are very small indeed in comparison with the size of the whole heart, it is chiefly to these that the attention of investigators has been directed, while to the great mass of the organ which is charged with the real work of carrying on the circulation, very little research has been devoted. Our account of the electro-physiology of the heart must, therefore, be necessarily incomplete, and incomplete in just the direction in which one had hoped for light, —namely, in an attempt to employ electrical estimations as measures of the contractile force of the heart.

To be useful, the first demand of a method is that it give constant readings, and observation has shown that the electrocardiogram satisfies this demand, for its waves tend to remain unaltered in shape and size. When they alter, an ascertainable disturbance has in many cases been found as the cause. An electrocardiogram is, therefore, a reliable record. Its constancy is illustrated in records obtained from various classes of animals. They have certain characteristics in common, so that one can easily distinguish, for example, electrocardiograms of amphibia from those of the higher mammals. And

of the mammals, the species which have been studied, the hor rabbit, cat, dog and man, each shows certain definite characters on t basis of which it can be recognized. Einthoven and Lewis and G der have studied human electrocardiograms and have defined witl certain limits the usual form of curves taken from normal perso But differentiation can go further, for the records of individuals ha been found to differ widely from person to person in health, a more widely still in disease. Whatever form, either normal or normal, the curve assumes, this remains characteristic for the in vidual, for a certain length of time at all events, though the reco be registered by different instruments and recorded by different vestigators. An electrocardiogram may indeed attain a form peculiarly personal that the suggestion has actually been made tha be employed to serve the purposes of identification in much the sa way as the Bertillon system does. We may therefore regard the el trocardiogram as a valid and reliable record.

A number of the factors which can bring about variations fron normal curve are understood. Some of these may now be enum ated. The auricular wave, in the first place, is modified by the nat of the derivation or lead used; in this case the most favorable si ations to employ are usually the two upper extremities, but un certain circumstances two points on the chest wall have been fo preferable. A rare and not altogether satisfactorily established fect in the auricular mechanism is a lack of synchronicity betw the contractions of the two auricles. This defect has occasion been found to split the *P* wave. But the most significant alterat occurs when the *P* wave, instead of being directed upward, the dir tion which in the usual arrangement is associated with primary ne tivity at the site of the sinus node, is directed downward, an alterat which presumably shows that primary negativity has occurred a lower level of the auricles. This change takes place when the si node is excluded from function, and when another part of the aur sets the pace instead, but it also occurs spontaneously as the re of causes, the nature of which is not clear. It has been our g fortune to observe on a few occasions a gradual transition fron waves directed upward to *P* waves directed downward; if the api of succeding *P* waves in such curves are joined, a curved line resu

This very curious phenomenon depends on an obscure mechanism, and unfortunately we have no satisfactory explanation for it. We know that where we have seen it best, it was associated with accidental intoxication by the drug digitalis. But we have seen indications of it in other connections which render its interpretation impossible with our present knowledge.

Some of the changes which are observed in the ventricular electrocardiogram are more easily explained. Considered as a whole, that is to say, in the light of the three usual leads of Einthoven (the first from the two arms, the second from the right arm and the left leg, and the third from the left arm and the left leg), one obtains a great many curves in which the waves in the first lead are inverted, and others in which inversion takes place in the third lead. Changes such as these result from a variety of causes. A heart which is not firmly anchored, but is easily shifted within the chest cavity by changes in the position of the body, may yield curves in which inverted waves appear. Comparable changes may result when the heart is pushed by air or fluid introduced in the chest, or when it is pulled to one or the other side by bands of adhesion stretched between the heart's surface and the chest wall. The explanation now offered for these phenomena by Einthoven and others is that the relation, determined by Waller, of the electrical axis of the organ to the body axis has become altered. Similar causes are probably at work in the electrical changes which are seen in increases in size of the heart, whether due to dilatation of its cavities, or to thickening of its walls; and the curves vary according to which side of the organ is involved. When the right side undergoes these changes, it is in the first lead that the ventricular waves become negative (Fig. 4); when it is the left side (Fig. 5), the negative waves appear in the third lead. Although alteration in the relation of the electrical axis and the body axis is the cause, that is to say, the mechanical, anatomical cause, commonly given for such deviations from the normal curve, it appears necessary to remember that in hypertrophy of the heart the balance of the sum of potential differences which produces the normal electrocardiogram must be disturbed, and that a rearrangement, that is to say, a functional rearrangement, of the parts of this sum must occur and might of course result in the changes we

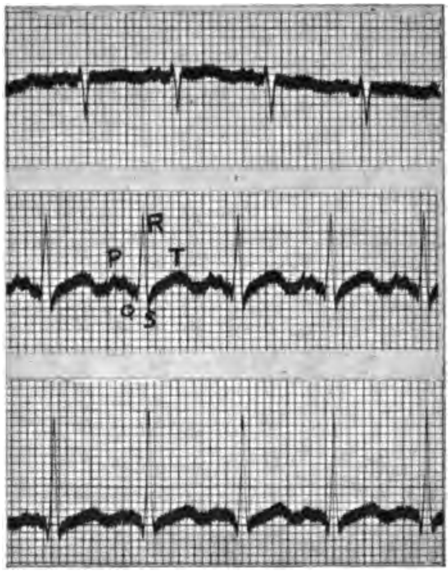

FIG. 4. Electrocardiogram showing the three usual leads of Einth
From a heart showing hypertrophy of its right ventricle.

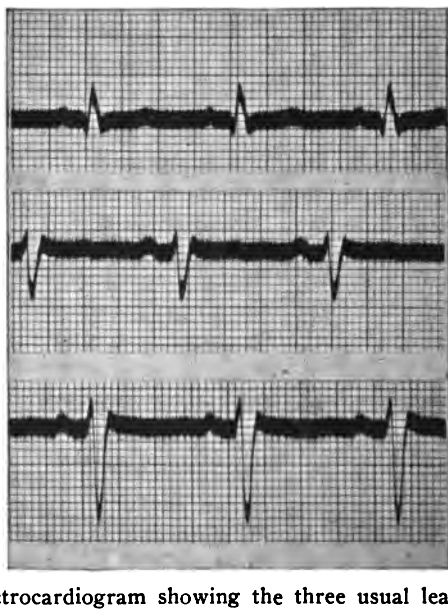

FIG. 5. Electrocardiogram showing the three usual leads of Einth
From a heart showing hypertrophy of its left vertricle.

are describing. More exaggerated changes still are observed when there is no alteration, either in the size or in the position of the heart; and these are due to the manner in which impulses are propagated to the ventricles from the contracting auricles. The pathway followed normally has already been described, but now the normal path cannot be taken, for it has been partly destroyed. It has been shown that when the conduction bundle to the right ventricle is

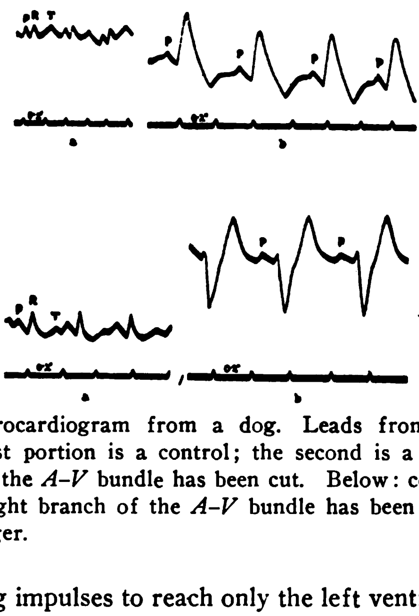

FIG. 6. Electrocardiogram from a dog. Leads from oesophagus and anus. Above: first portion is a control; the second is a curve taken after the left branch of the *A–V* bundle has been cut. Below: control and a curve taken after the right branch of the *A–V* bundle has been cut. After Rothberger and Eppinger.

severed, allowing impulses to reach only the left ventricle, the electrocardiogram immediately takes on another shape, the shape being an exaggeration of what occurs during enlargements of the left side of the heart. The first ventricular wave is sharp downward and the second an upward deflection. If, on the other hand, a similar injury is done to the conduction bundle to the right ventricle, there is a reversal of electrocardiographic curve. It consists of a sharp upward, followed by a downward deflection (Fig 6). Finally, if a curve of one or other form has been obtained by cutting one or other branch of the conducting bundle, one can, by severing the still uninjured branch, obtain an electrocardiogram which differs from both the preceding and resembles, though not exactly, the original curve. These changes depend, then, upon the way impulses pass through

the heart. An explanation of the nature of these abnormal curves is obtained by comparing them with others from hearts to which direct mechanical stimuli have been applied. A stimulus to the left side of the heart (ventricle) yields a curve like that seen when the right branch of the conducting bundle is cut, while one applied to the right side is like the curve seen when the left tract is cut. Under both conditions, contraction is initiated at the site of the stimulus and the impulse spreads over the heart from this area. The form of the curve yielded depends on whether the right or left ventricle initiates the contraction.

The conduction bundle, then, is an important factor in the orderly cardiac mechanism, but, on account of its exposed position in the heart, it is frequently injured. The pathway between the auricles and the ventricles is, therefore, interrupted and impulses from one to the other are consequently blocked. The ventricles, deprived of stimuli from above, contract independently and without reference to the rate or rhythm of the auricular beats. In the study of derange-ments of the cardiac mechanism of this nature, electrocardiography has rendered distinct service.

So far we have discussed the mechanism of the heart-beat only in so far as it relates to the intrinsic arrangements of the heart. Bu for the proper exercise of a number of its functions, the heart is sub-jected to the influence of the central nervous system. In the study of this influence the galvanometer has been not only useful, bu essential. Branches from the central nervous system to the hear exercise functions of two sorts,—inhibitory and accelerator. It has been especially our work to show that the inhibitory or slowing function is not simple and is not possessed equally by both vagus nerves. To say that the right vagus nerve principally modifies the rate of the hear and the left vagus chiefly conduction between its chambers states a truth and also indicates the presence of a complicated mechanism which may be explained in the following way: The complexity de-pends on the fact that the heart was originally an unpaired organ with a symmetrically distributed nerve supply. This supply, so far as we are informed, was directed to the junction between the old sinus venosus and the auricles. In the development of the heart a division of the junctional tissue took place; one portion, the right

Electrocardiogram f on of the left vagus nerve. Divisions of the
dinates are 0.1 millivolt; of

remained at its original level, but became incorporated in the wall of the right auricle as the sinus node; the other portion, the left, became dislocated and moved downward the distance of a whole chamber, becoming, in the adult mammalian heart, the auriculo-ventricular node which lies between the auricles and the ventricles. The importance of these changes in position, from the point of view of innervation, lies in the fact that the cardiac branches of the two vagus nerves, which were distributed symmetrically in the primitive heart, have followed the changed positions of the structures they innervated originally and have become incorporated with them in their new situations. Consequently, the nodes and their nerves have assumed different functions. We were led to believe that this change must have taken place, first by clinical observation, and we have tested this hypothesis by experiment. As the result of experiments on many dogs, we were able to decide that when the right vagus nerve was electrically stimulated, the heart stopped beating. But when the left vagus nerve was stimulated, the auricles did not stop beating, but continued, though often at a slower rate. The striking thing now observed was that these auricular beats failed, either entirely or only occasionally, of being followed by a ventricular response (Plate II).

These were the facts. In the light of current teaching, the motions of the heart are initiated and rate is maintained by the sinus node. Impulses so initiated are conducted from the auricles to the ventricles over the narrow conduction path, with which we have become familiar. It follows that, when we find the heart stop as the result of a stimulus, we must assume the stimulus to have been distributed to that portion of the heart where the pace-making function resides, that is to say, at the sinus node. In the same way, when a disturbance in coördination between auricles and ventricles occurs, we have sufficient evidence to indicate that this occurs as the result of an effect produced at the junctional connecting tissues. We must, therefore, conclude that if stimulation of the left nerve brings about this disturbance, it must necessarily be distributed to this portion of the heart, that is to say, to the conducting system. Although we think that these sites, the sino-auricular and the auriculo-ventricular nodes, are the main terminals of these nerves, it is clear that other functions

are influenced at the same time when they are stimulated. We have indeed shown that this is the case. But our main concern has been to obtain information about essential differences; the likenesses and the additional influences exerted will be apparent. In this problem again, the galvanometer has been invaluable. On the basis of ordinary mechanical records, of which we have made many in the course of this work, we could not have drawn the conclusions just detailed.

Stimulation of the augmentor nerves shows that these have distributions similar to those of the inhibitors. Rothberger and Winterberg have contributed these facts. They have shown that stimulation of one or other augmentor nerve produces effects which can be referred to a modification of the function of the special cardiac tissues we have discussed in connection with the inhibitory nerves; and they have also shown that other differences of an electrocardiographic nature take place.

We have traced, in recording the newer aspects and methods of the study of the heart-beat, the influence exerted by the introduction of electrical methods. Advances by this method were due in large measure to the construction of an adequate galvanometer. But the advances in our understanding of the heart have depended on detailed anatomical and physiological investigation of the newly discovered structures in the heart itself. How small a portion it is that has been studied in relation to the whole heart, and how relatively few functions of the efficient organ have been included in the recent investigations, has been indicated. Much remains to be done by the means at our command, but much also by others still to be devised.

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THE USE OF A PHOTOGRAPHIC DOUBLET IN CATALOGUING THE POSITIONS OF STARS.

By FRANK SCHLESINGER.

(*Read April 24, 1914.*)

In order to utilize for cataloguing and for similar purposes the positions of stars derived from photographs, it is necessary to know the scale of the plates, their orientation and their positions in the sky. In the early days of astronomical photography attempts were made to determine the scale and the orientation by such methods as measuring the absolute focal length of the photographing telescope, and by impressing an orienting star trail upon the plate. These attempts have not been successful and it has now become the universal practice to employ comparison stars for these purposes: that is, stars that appear on the plate with positions known in advance, usually through observations with the meridian circle. Here again experience has shown that in the determination of star places by photography, this matter of comparison stars is usually the weakest link in the chain. For example, in the case of the Astrographic Catalogue much of the relatively high precision with which the plates can be measured is lost on account of the lack of suitable comparison stars.

An experiment that aims to overcome this difficulty is in progress at the Allegheny Observatory. Instead of employing a simple objective to photograph the stars, a doublet lens is used. This has the advantage of much greater extent of field of good definition, at least six times as great as in the case of the objectives used for the Astrographic Catalogue. Consequently in surveying any large area of the sky, plates taken with the latter kind of telescope will require (other things being equal) six times as many comparison stars as the doublet.

The use of the doublet for these purposes was advocated twenty-five years ago by Pickering, but astronomers have feared that the use of such an objective might introduce serious errors of various kinds. The experiments already completed at Allegheny prove that,

with proper precautions, these fears are groundless and that this form of instrument is admirably adapted for cataloguing purposes. The detailed results of these experiments will soon appear as No. 9, Volume 3 of the "Publications of the Allegheny Observatory." On this occasion it will suffice to give merely the results.

A number of regions covering about twenty-five square degrees each, were photographed in duplicate, first with the center of the plate at the center of the region and then with the edge of the plate in that position. A comparison of the two sets of positions gives

$$0''.18$$

as the probable error of the measurement of one image. This quantity includes not only the accidental error but the following as well: (1) outstanding errors in the measuring engine; (2) the optical distortion of the objective, due to a possible failure of the objective to give a truly linear reproduction of the object photographed; (3) magnitude distortion due to spherical aberration and causing bright stars to appear systematically nearer or systematically farther from the center than faint stars. The result shows that all of these errors are very small, and additional experiments indicate independently that (2) and (3) are negligible or very nearly so.

Doublets have been extensively used in astronomy for pictorial purposes. The doublet that we have used for the above experiments differs in one important respect from these: the ratio of the aperture to its focal length is twenty-one instead of five or six This circumstance is favorable to its performance over a wide field and is doubtless responsible for the smallness of the optical and the magnitude distortions.

Our objective covers well a field of twenty-five square degrees, as compared with four square degrees in the case of the plates for the Astrographic Catalogue. For the latter, as already stated, six times as many comparison stars would be necessary in a large piece of work in order to determine the plate constants equally well. The scale of the astrographic plates is a little more than double that of ours, and the purely accidental error of measurement is therefore somewhat smaller, but not as much smaller as this difference in scale would imply. In practice, so much of the accuracy of photographic positions depends upon the comparison stars, that if a large area

were to be surveyed with both instruments and the same comparison stars were used in the two cases, our doublet would give the more accurate results. If there were at hand in some special case comparison stars that are much superior in number and in accuracy to those that are generally available, then the astrographic plates would give the better positions. Needless to say, a doublet like ours with twice its aperture and twice its focal length would give still better results, but this would necessitate plates about 35 centimeters square and a measuring engine large enough to accommodate them.

Our objective would also be well adapted for compiling zone catalogues of faint stars similar to those of the *Astronomische Gesellschaft*. The latter now extend from 80° north declination to 18° south, a work that has required the cooperation of sixteen observatories during a considerable number of years. Observations are in progress that will extend this catalogue farther south, but no provision has yet been made for the southernmost zones. When these come to be actively considered, the claims for the doublet should be carefully weighed. The original plans for the Gesellschaft Catalogue included the repetition of the observations after the lapse of about half a century. This time is now approaching in the case of some of the northern zones, though others (notably the one between 70° and 75°) are of more recent origin. The repetition of all the northern zones, if carried out photographically by means of a doublet, would be a task well within the powers of a single observatory. Moreover there would be a very considerable gain in accuracy. From the prefaces to the various zones of the Gesellschaft Catalogue we learn that the probable error of one observation in either right ascension or declination is on the average well over 0″.50. This was derived from observations made with the same telescope on different nights, and does not include certain errors that would be brought out by comparing observations made at different observatories. Our plates yield for the probable error of one observation, 0″.18. This was obtained by comparing overlapping plates, but it does not include errors due to inaccuracies in the positions of comparison stars.

Although the tests we have made with our doublet may be ac-

cepted as indicating the size of accidental errors and of certain kinds of systematic error, it cannot be claimed that they tell the whole story. To make certain that star places thus derived are free from serious systematic error of any kind, would require much more observational material and would involve certain extensive intercomparisons as well as comparisons with star positions derived by wholly different methods. Such a work is contemplated at Allegheny and is in fact well under way. The zone extending from declination 2° north to 2° 10′ south is being photographed in duplicate, the centers of one set of plates being upon the eastern or western edges of the other. Each plate will embrace 24 minutes in right ascension or 6°; the program at the telescope therefore calls for 120 plates. To determine the plate constants 602 comparison stars will be employed, an average of ten on a plate. The mean of the two positions for each catalogue star will therefore depend upon about fifteen comparison stars. Thanks to the courtesy and cooperation of Director Campbell and Professor Tucker of the Lick Observatory, the positions of these 602 stars are being determined with the meridian circle of that institution.

Within the limits of declination selected we are measuring the positions of all the stars that appear in the three Gesellschaft zones that overlap. The observing list for the latter was made up of the stars that are designated with (visual) magnitudes 9.0 or brighter in the Bonn Durchmusterung, and as many fainter stars as circumstances permitted. In our work we shall necessarily omit a few stars that are photographically faint by reason of their color, a few stars that are so bright as to present images too large for accurate bisection, and a few doubles that are too close for separation on plates of this small scale. Making allowance for these omissions the completed catalogue will contain about 7,100 stars. It is hoped that this work may not only prove a valuable contribution to our knowledge of the positions and motions of faint stars, but that it may enable astronomers to decide definitely as to the advantages and disadvantages of this form of instrument for the wider applications of the same kind that the future will demand.

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SOME OBSERVATIONS ON THE PSYCHOLOGY OF JURORS AND JURIES.

By PATTERSON DuBOIS.

(*Read April 23, 1914.*)

Interpretation is the crux of science and the chief point of failure in scientific men. When concerned with human behavior it reaches the most subtle nicety—a kind of nicety, indeed, which projects far beyond the helping hand of instruments of precision or mathematical formulæ. Human interpretation is the pivot of human intercourse.

Under the prescriptions of our common jurisprudence the verdict of a court jury is mainly a resultant of many subconscious forces and unseen influences. Standing obviously to reason, this needs no demonstration. Comparatively little of "the evidence" admitted by the court during the trial and little of the pleading operates directly upon the mind of the jury—even though, paradoxically, without these there could be no trial.

It is no part of this paper to attempt the impossible task of unravelling the real or probable complex of past experiences, prejudices, emotions, misunderstandings or logical balances of judgment affecting the minds of the jurors.

Chiefly, it is limited to the proposition that, so far as the juror is concerned, court discipline tends to diminish rather than to increase, and to hamper rather than to facilitate his efficiency as an agency of justice. In other words, the conscientious juryman of good average equipment goes handicapped to his task largely because he is in subjection to an iron rule which takes no note of his individuality of fitness or unfitness, distress or ease, or of other personal and court conditions which themselves condition his judiciality. The problem is largely one of attitudes—a comparatively neglected field in pure or applied psychology.

The court takes cognizance of certain classes of influences likely

to affect the jury mind, giving way to some and aiming to suppress others that tend to prejudice and disturb the equanimity which makes for justice. But the main thesis here is that there are other and subtler influences which are often a still greater menace to justice and that there is little thought of reducing them to a minimum.

Manifestly, this should be the first consideration—just as the reduction of friction and of waste effort are of prime importance to the mechanist. Similarly, a business requiring so much personal sacrifice, such delicate mental balance coupled with resolute and courageous control, such openness to conviction without the weakness of hasty consent,—such a business should be carried on under the most favorable conditions instead of under conditions tending to defeat the desired ends.

In his delicate and often vital responsibilities everything should, as far as possible, conspire toward the best use of the juror's morally controlled intellectual judgment, whereas this judgment is subjected to an automatic conspiracy of obstacles, infirmities, and irritants. He is expected to be, and, in my experience, generally wants to be, free of all forms of bias for which the challenging attorneys take little thought. The juror, indeed, is in no small degree a marionette of which his subconscious self holds the controlling wires while the whole inherited system of trial by so-called peers is the grinning instigator. And the marionette is seldom wholly self-respecting.

All the way down from the earliest English times when witnesses were jurors and jurors were witnesses, gathering their own testimony and acting on their own knowledge, there have been many modifications of the jury system and diverse views as to its value.

The juror's office is, in some respects, the most ironical and paradoxical in the whole range of human service. Its requirements are those of a learned profession with demands of extraordinary versatility. The profession is to be followed by the individual for a very brief period with uncompensating "compensation" (often at an actual loss to the juror.) Yet the juror is so hedged about, officially suppressed and oppressed that the opportunity for initiative, the value of his knowledge, and the freedom of his judgment are reduced to the minimum instead of being employed at their maximum. He is, in effect, both puppet and prisoner.

I am especially considering the juror of the more intelligent and selected class. As a rule, he admits that jury service, in spite of its drawbacks, is educationally profitable and often interesting. He means that his duty shall be conscientiously performed. He is sensitive about sitting in judgment upon his fellows—for he knows his own liability to appear at any time as plaintiff or as defendant.

Yet withal, he is asked to swim with a stone on his neck, to be agile in a straight-jacket, to be patiently complaisant in duress—a mental Blondin in hobbles.

His bristles begin to rise, when, in his own home, the summons is served with a threat. The "law" first greets him with flashing eye and teeth uncovered. The more conscientious and honorable the man the more offensive appears the attitude of his legalized captor.

In the United States courts this summons may compel the victim to attend court fifty or a hundred miles away. He may live on a branch road where train service is such that he could not go and come every day. Even if he could, he would be out of pocket since the fee is insufficient to pay the passage. This condition may require the juror to be absent for a week at a time from his business, or from a sick wife, virtually without compensation and really without assurance of release in emergency. How can a man's judgment be at its best under such circumstances?

In any case, whether he lives far or near, the average man on receiving notification that he is wanted is, for the moment at least, disconcerted or timorous. Before him rises the giant of business demands, home dependents, personal physical disabilities or pathological possibilties, a sense of being penalized, of loss of freedom without cause, a danger of "contempt" through sheer ignorance and inexperience, a vision of all-night incarceration with uncongenial strangers, perhaps sickness in his family unwaited upon and beyond communication with him. I doubt whether any jury panel is ever drawn entirely free from some or all of these mental perturbations in some or all of the individuals. They are, however, especially true of the first-time juror. To add to his discomfiture his neighbors either pity his plight or make a jest of it or advise his making an effort to get excused. I have talked with two classes of drawn

men. One never expects to serve if release is possible, another chafes under his weary durance yet says that he ought and means to serve if possible.

Once in the traces and more or less accustomed to the routine the first pressure of the screws is less felt. But, none the less, the juror has a mind and a body. He does not forget that he was born a freeman nor that he is absent from farm or store or office. The delays, the wearisome reiterations and professional fencing, the interruptions and general unbusinesslike pace of procedure are at his cost. They irritate and disgust.

When the juror is drawn from the "panel" to serve on a particular case he is ordered into his seat with pretty much the same show of authority and sense of subjection as he sees when the accused defendant is policed into his presence. In an interesting reminiscence of long and varied jury experience published in a magazine article some years ago Mr. Joseph Hornor Coates speaks deprecatingly of these indignities:

> "The juryman from his first entrance in response to the court's peremptory summons finds little in his treatment to impress him with an idea of special dignity in his position, even if he has no overt cause of compliant. He is herded with his fellows, ordered about by the tipstaves or bailiffs of court, addressed in peremptory tones . . . . He sits in the court room with an ever-present sense if he be sensitive, that he must be careful not to get into trouble; the feeling of liberty is gone, he is enveloped in an atmosphere of restraint. Really, he is placed more on an equality with the prisoner at the bar than with the judge on the bench, yet he is as essentially a part of the court as that august potentate and may have at any time a greater responsibility imposed upon him. In some court rooms when disengaged from the actual trial of any case and awaiting summons to the jury-box, the juror is often forced to sit among criminals, witnesses, loafers and ill-smelling persons attracted to the court by business or curiosity."

While the average juror resents these low estimates of his office he does not, perhaps, fully realize how his own judicial faculty is lowered by his lowered estimate of the court as an institution. He becomes critical and dislikes being party to the system.

The juror's disposition to criticize the system (rather than court officers themselves, for the officers naturally fall into the formal bondage of a system) increases through the meanderings of the trial, —the surplussage, the objecting attorney, the lust for filing ex-

ceptions, the hectoring and heckling of witnesses, the muteness of the jury itself, and finally the mistrust of its honor in locking it up to reach an agreement—not always real but formal—all these incite the sensitive and the honorable mind especially to secret rebellion against the system. This state of mind cannot but endanger verdicts.

There are other influences of entirely conscious and purposive kind which affect verdicts too. Thus, in the jury's deliberations under lock and key a juryman will openly confess that he will not vote to acquit a negro prisoner because he *is* a negro; or he will stand by a chauffer in a suit for damages because he himself drives an automobile; or he is moved by fear of the judge to decide against what he has ordered or what he believes to be the judge's opinion; or he is politically afraid; or he dislikes to be the target of ten or eleven others; or he wants to go home, to take the next and last train, perhaps, to avoid another night at a hotel; or he cannot understand the case—its technical terms, its arithmetic, its alternatives, its fine-spun distinctions, some of which are purely technical and have no direct bearing on justice.

But while some of these influences arise directly out of the system and are corrigible they are mostly more obvious and overt than the subconscious causes of mental sway, arising immediately out of juridical prescription and attitude. Some of these have already been noted. Timidity, restraint, sense of personal loss, personal discomfort, offensive environment and treatment, worry over private troubles caused in part by absence from home or business —all of these affect the balances of a sensitive mind and are in a large measure corrigible. The implication that as a juror one cannot be trusted to come and go while the judge and the witnesses and attorneys are free hurts a sensitive mind and excites a rebellious spirit.

In fact, the wearisome reiterations in the examination and cross-examination of witnesses, the time consumed in legal fencing, the objecting and excepting, sometimes give the juror a sense of being unfairly exploited for the gain of attorneys rather than for the settlement of real difficulties. Juries are jealous of their time and personal freedom; so surplus verbiage and legal loquacity irritate to

the extent of damaging a cause—for it is a fact that a verdict or a disagreement is not seldom a slap at a lawyer's course. Many a case is undone by overdoing. This is a hard lesson for lawyers to learn. In his reminiscences of a long life at the bar Theron G. Strong (*The Outlook*) notes this danger of time-wasting excess and overdoing. He says:

"The man who can say the most good sense and sound law in the shortest time has a decided advantage. Juries are not much influenced by outbursts of eloquence, and appellate tribunals will not tolerate them. A tired and yawning jury will not be likely to take the most favorable view of an advocate's case, and when the attention of an appellate tribunal is lost and the judges begin to converse in whispers or bury themselves in the record, the oral argument is little more than a waste of time. When you have lost attention, you have probably lost your case. Juries and judges have become so accustomed to business-like methods that they appreciate a simple and clear presentation of the essential facts, each argument in its support clearly stated in a few well-conceived sentences, with no haltings and no revertings to things inadvertently omitted, no fumbling of documents, and no reading from authorities . . . .

"One of the most important arts of the court lawyer is to know when to keep still, and be able to exercise the self-command to do so. Many a case has been won by paying due regard to the attitude of the judge when he essays to combat the views of opposing counsel. The lawyer is indeed wanting in tact and discretion who then assumes any other rôle than that of a spectator of the proceedings. By all means let the judge do your arguing for you if he is so inclined, and if in this way he indicates that he is favorably disposed it is folly to attempt to reinforce his views; even though they could probably be reinforced to advantage, they do not need reinforcement so long as he adheres to them. The moment the court appears favorably inclined to your side of the case is the time to preserve discreet silence. This is equally true with juries, and if in the course of the trial there is the slightest leaning in your favor, then is the time to do as little as possible by objections or long cross-examinations, which can only have a tendency to lead the court and jury to think that you consider it necessary to strengthen your case when it needs no strengthening, the only effect being to counteract the favorable impression that has been made. Many a case has been spoiled by an inability to recognize the appropriate time to say nothing."

I quote this at some length because few lawyers are so discerning of the juror's point of view. I confess to the feeling of an oncoming bias against the lawyer who is working too hard. Too much repetition of evidence, too many witnesses, too great detail in the pleading, too much swelling molehills to mountains, too noisy oratory—all these excesses tell on the juror's temper. He does not

care to be treated as though he had not the brains to see the bearings of the testimony as it came from the mouths of the witnesses.

The court is quite right in sustaining objections to much extraneous matter that clogs the proceedings and overloads the jurors. But it is questionable whether the loss of time by a large majority of these objections does not tend to irritate jurors who are rightly jealous of their time and critical of a system which compels its loss without corresponding gain to them. Much of the matter ruled out or admitted after a battle between attorneys goes for what it is worth to the jury and not a little of it is without effect on the juror's mind either way, except as a general irritant. Whatever is evidence to his mind *is* evidence and willy-nilly his mind is affected by it. To a juror with imagination and the gift of interpretation evidence is often felt as atmosphere and is much more than the dry bones of admitted testimony.

For instance, in a trial in which the verdict turned chiefly on the motive or purpose of a paper engaging to purchase and pay for a large block of stock on a certain date, the lack of frankness and the constant evasiveness in one of the witnesses so strongly discredited him in the minds of at least half the jury that they virtually agreed that they would not employ such a man in their own businesses. Indeed the other half of the jury did not defend him. As this witness substantially agreed with and was on terms of more or less intimacy with the four others who told the same story the whole five were greatly weakened as witnesses. This was evidence unavoidable in coloring the minds of jurors, although not "evidence" on the record.

Again, in the case of a woman suing for damages as the alleged result of a fall by a defective brick pavement, more than one juror believed that the defense lost an opportunity in not bringing out in cross-examination the height and slope of the heels of her shoes—as these might have been more responsible for her tumble than the pavement itself. But as these jurors did not know anything about it—nothing having been said in court about it—they of course did not openly consider it. But the point is, that even such a case no one knows how far such a passing thought gives cast to the mind

consciously balancing itself to a nicety of honesty and absolute justice. It becomes evidence.

It is questionable whether the cause of justice would not be better served by a much more liberal attitude toward the admission of evidence than our jurisprudence usually allows.

This particular case is also a good illustration of jury-irritation through excess of detail and repetition. Large photographs were displayed and re-displayed and pencil-marked and passed and re-passed among the jury to show the pavement and the curbstone. All witnesses but one corroborated what the photographs were supposed to picture and no juror or any one else probably had any doubt about the pavement or about the slipping of the woman's foot. But it would have been just as effective to show the photographs once and for all, and obtain a simple statement of fewer witnesses without such over-elaboration.

There was a juror, for instance, who was suffering from a racking cough, who lived on a small branch railroad at perhaps fifty miles distance. He had left many business interests. It was now the last day of the term—Saturday. If he lost a certain afternoon train he would lose a whole day in getting home. He knew that a team had been sent for him a dozen miles or so through a snow-drifted country and if he could not catch that train his non-arrival would not be understood. Naturally he looked repeatedly at his watch when every senseless repetition delayed the trial. The judge did his part to push the trial through but there is a limit to the judge's action at least as a matter of expediency. The juror with dismay saw his train-time go by with suppressed irritation. One or two others from other counties were similarly affected. The insistence here is that the trial could have been gone through within half or two thirds of the time it really took. The jury knew no more for the redundancy and were less fit for the balance of their powers than they would be under more favorable conditions. Few men can be at their best as dispensers of justice when they see their valuable time frittered away—for what? To settle a contention in which they have no live interest and for which they are held in duress as though they were not to be trusted out of sight.

Mr. Coates's experience agrees with mine—that the pleadings

of the lawyers have very little direct effect on a substantial jury. In simple cases not involving too much consideration of human nature or of the motives perhaps most jurors catch the drift of the evidence and make up their minds before the trial is through—even though they suppose themselves open to conviction either way.

Lawyers damage their cases—sometimes lose them, by what the jury regards as an unfair trick. Some jurors are affected by the fact that a famous lawyer is on the case—his reputation alone carrying weight.

A very slight thing may bias the juror. One lawyer lost favor with some jurors because of his continual smile and offhand pleasantry in his pleading. Another quite properly asked one of the jurors, whom he called by name, to define a technical business term —the lawyer being or assuming to be not quite sure of it himself. This fact was noted in the juryroom and charged up against that juryman's opinion—one of his confreres asserting that apparent friendship with the plaintiff's attorney affected his judgment. This charge was entirely good natured but was given to offset a charge that the juror favoring the defence was partisan because of his local affiliations—the defendant hailing from the juror's locality. The jury disagreed in this case which was one of great difficulty because of local interests, human motives, and the large losses and gains involved. And yet no one can question the honesty of every juror there —while also no one can help questioning the amount of coloration in the minds of not a few of the jury arising from these causes apart from the formal evidence. It was noteworthy that all the jurors who were of a certain foreign descent and understood the same foreign tongue although not previously acquainted with each other, voted together. An unconscious esprit-de-corps arose which banded them solidly against the other half of the jury. I do not think that either side noticed this fact but I am quite sure that the subtle influence of nativity and speech worked on them without their knowing it. Of course no court could foresee or suspect or avert this. I instance it only as an illustration of unconscious congregate influences on honest and conscientious but untrained minds.

I have been strongly impressed with the comparatively helpless situation of the defense in many cases of suit for damages especially

when the plaintiff is poor and the defendant in comparative affluence and still more especially when a corporation. Quite apart from the traditional attitude toward the so-called soulless corporation a class of circumstances may put the defendant at a great and unfair disadvantage.

The case of the plaintiff who slipped on a curbstone and claimed that a sunken brick pavement compelled her to so step on the curb as to slip, is in point here. The very fact that the photographs are handed about and continually referred to, that witnesses have seen that condition there for years, that the woman's leg was so sprained by her fall as to prevent her making her living as heretofore gradually works upon the minds of the jurors because no one can say that none of these things are so.

I remember seeing nothing very bad in the photographs at first but the incessant references to the lines of the picture (I now see) exaggerated in my own mind the dangers from such conditions at a crossing. Some of the jury were particular to say that they believed in discouraging the legal traffic in damage suits but they also believed that sidewalks should not be allowed to become a menace to the walking public. Small compensation was therefore agreed upon by the clumsy process of averaging the vote.

A few weeks later I went, out of curiosity, to see the place itself. I am quite confident that as a casual pedestrian the slight sinking of the bricks and the slope of the curbstone would never have attracted my notice. The photographs were in a sense true and in a sense untrue. The proportions of the street were distorted and the viewpoints were selected for a purpose. Much was made, at the trial, of the rounded curb which had been worn that way by the grating of heavy wheels against it on the corner. The rounding never looked bad to me on the photograph but as I think back to an objective view of my own mind during the trial, excusing the photograph's supposed untruth as being too small or perhaps not properly posed to show the real danger, I see how gradually I began to think against my real judgment and see untruly. And now in its very presence the stone itself looked no worse to me than did the photograph at first; and I am bound to wonder why if this pavement and curb were entirely to blame there are not such accidents going on all over the

city every day, for I notice that that pavement was relatively not dangerous or bad.

I am not quarrelling with the verdict in which I had a part. It was not very serious either way except as a matter of abstract justice. I am merely trying to show how much more those pictures, in conjunction with the strained reiterations of witnesses, counted for than they were really worth. The mind believed what was true in them because they were photographs and excused and apologized for what seemed untrue (but was really very true)—also because they were photographs.

In an article on "Photography and Crime" Mr. C. H. Claudy says:

> "Any capable photographer knows how to magnify or minimize certain parts of the perspective of any view. Thus, a long-focus, narrow-angle lens will give a totally different result from a wide-angle, short-focus lens. In a suit for damages because of obstructions left upon the street, for instance, a lawyer will have a photographer use the latter lens and stand close to the alleged obstructions. A pile of earth, particularly if photographed low, will appear very large in proportion to the vanishing perspective of the street. A natural-angle photograph, made with a ten-inch lens on a five-by-seven plate, will give a totally different idea of the size of the obstruction.
>
> "Cracks in buildings, as evidence of the damage done by subway construction or sewer-laying, can not be brought before a jury; but photographs of them can be so used as evidence. A clever photographer, by manipulation of his illumination, so that one side of the crack throws a heavy shadow can make such fissures appear far larger than they really are. Pictures of hills, to show the locality of a runaway, can be made steep or flat according to how the camera is handled. It is not, therefore, necessary to resort to actual changing of the negative and print to make the camera deceptive, and more and more are our courts coming to understand this fact."

I have been startled several times with the seeming unevenness and bad brick-laying in a brick extension wall of my own house. In early summer the low afternoon sun throws long shadows lengthwise from irregularities in the brick quite unobservable at other times. A photograph of the wall at such a time might be shown as strong evidence that the wall had suffered some kind of disruption. Yet the fact is that it is finely and evenly laid.

No allegations of attempt to falsify in the case under consideration are here intended. It is true that in the pictures the little streets looked broad and fine but the defects of the pavement and the curb

were not exaggerated. The point is, that when photographs are shown in support of the verbal testimony even the discriminating mind is apt to be over-persuaded by the mere fact of pictures. Any seeming lack therefore on the part of the picture is subconsciously excused on the ground that a depression of two inches in a pavement is necessarily diminished to almost nothing in a photograph. Then the mind rebounds to an exaggeration of the truth notwithstanding the claim that the depression does not exceed two inches.

Manifestly, the defense has little show in such a case. He cannot prove that the plaintiff did not fall from this cause. His one witness denied the dangerous character of conditions and to us jurors this denial seemed fatuous and partisan. But when I saw the place itself I thought this witness more right than wrong—more true indeed to the moral fact than all the others.

I am quite sure that had the jury been taken in a body to the actual scene of this accident the outcome would have been different. I could scarcely believe my own eyes. I tried to slip on the curb as the plaintiff slipped because of the slightly sunken but not rough pavement, but I failed. True, I had rubber heels but true, also, the woman may have had suicidal "French heels." Of this we jurors had no knowledge but some of us thought of it. All the verbal testimony of many witnesses corroborated what the photograph was supposed to show. It did show lines and shadows but not danger. The witness said danger and the jury believed that the photographs showed it too. Doubtless, also, the degree of damage which the woman suffered became, sub-consciously in the jury mind, the measure of that danger. I see now, that the pictures did not prove danger—not relatively, at least, as I find pavements and curbs wherever I go as bad and worse but they do not seem to me dangerous. If the defendant was guilty of negligence, comparatively few property holders are not guilty.

This case has been reviewed at some length because of its illustrative values in pointing out how the mind shifts itself under the subtilties of "evidence" which is in reality no evidence, but which cannot be denied or assailed as untrue and cannot be easily ruled out.

It seems to me that with a knowledge of the psychology of the gradual winning of the juryman's mind in spite of his own better

thinking, appeal for retrial should have serious consideration by the judges. In such a case the defense in not weak because the lawyer is weak, not because the defense has not been properly worked up but because all the activity, all the pictoriality, all the interest, lies with the plaintiff. One woman slips; a hundred thousand do not slip; but they never come on the witness stand. Moreover, the mind assumes that unless the case were good no photographs would have been taken, as the risk of recoil would have been too great.

In cases where medical experts are called to testify lawyers are too much inclined to display their crammed knowledge of the anatomy and physiology of injured limbs. Direct and cross-examination in these cases are sometimes carried to absurd length and minuteness. It makes little difference to a jury how a nerve moves the muscle and how the life process itself produces movement. I am quite aware that some of these foolish professional displays are made with the hope of discrediting the opposite expert witness but as a rule they are wearisome and even ludicrous—and neither of these conditions helps the cause.

Whatever makes for straightforward simplicity counts for the jury's favor. It is then that the real evidence weighs at its true value. Of course there are jurymen who admire adroitness, shrewdness, cleverness, even craftiness in an attorney and are much influenced by that admiration rather than by the real merits of the case. But these very attainments or methods work the other way with many jurors; and the insolent brow-beating of witnesses or manifest effort to confuse and "rattle" a simple-minded and honest witness is pretty sure to awaken indignation in the jury and recoil on the parties indulging in that kind of practice. This indignation is no part of the evidence which jurors are sworn to decide the case upon but it goes for evidence as weight in the mental balance.

And on this point, when the court orders a specific verdict without consulting the mind of the jury, is it not virtually ordering possible perjury or, in effect, subornation? Seeing their liability to this kind of termination to their labors jurors sometimes grow lax or indignant, according to their temper. They become puppets either way and official "evidence" becomes of less moral moment in their service.

In point here, is a paragraph from *Law Notes* (March, 1910):

" Judge Caldwell who had served nearly 35 years on the bench of the Federal District and Circuit Courts said that trial by jury was guaranteed in the Constitution 'because the people knew the judges were poor judges of the facts' and that 'every day's experience confirms the wisdom of their action. Equally strong testimony has been given by some of the greatest judges this country has ever known,' . . . . How many self-respecting men will condescend to serve willingly on juries if they know the judge is likely to hold them up to public scorn because he disagrees with their unanimous opinion delivered under oath?"

Now let us add to this question the weight of the fact that the greater the intelligence and moral force of the juror the greater his antipathy to such an unjust service. And in his resentment lies the psyschological menace to his natural qualifications. And surely the lower grade of man is not to be preferred because of his being the more subservient.

Not having sufficiently investigated the claim of the incompetency of judges to judge of "facts" I make no comment on it—except that apriori grounds seem to me to favor it. But if it be true then many a case suffers (as indeed every juror knows) from the exclusion as well as the inclusion of testimony. If the juror is a better judge of "facts" than the judge is then the juror should have power to call for such facts as appear to him to bear on the case. I for one have heard witnesses choked off by the incessant objecting of attorneys until the testimony was squeezed dry of all that essence which gives to a story its true value. True, a witness may overcolor and may run into imaginings and expeditionary sallies of sentiment and statement and this freedom should be limited. Nevertheless, the dry skeleton of what the court calls "facts" is often as untrue by default of important facts as the overweighted story tends toward giving an untrue impression of the case.

The juror should not be submitted to the strain of shutting out what he heard ruled out by the judge as not being relevant or admissible as evidence. I remember hearing a jury debate the import of an offhand written promise to pay a large sum. A youthful juror reminded us that the judge had said that we could consider the oral testimony in such a case. At least half the jury disregarded the paper because the judge permitted the oral testimony to count. The

other half saw in the paper the most vital evidence not, however, because the judge admitted it but because it appealed to their sense of business honor. Another judge might have ruled it out entirely as it lacked some factors of a formal document. What is evidence to some is not evidence to all, and no line can be drawn for testimony. Should not jurors have some right to demand such information as seems to them to be evidence? The " rules of evidence " are sometimes as wooden as they are usually useful. The mind of the average jury is in danger of being befogged by the ins and outs of matter offered as testimony.

It is true that a jury has more power than it usually takes in examining witnesses or inquiring of the court. But that is in large degree because the juror feels his automatism and his incarceration. Many a juror would like to ask questions but he has not been invited and he is afraid of attitude. He works under threat and all penologists are agreed that timidity and fear under threat do not make for strong intitiative or moral control.

In no situation in life is what Bergson calls the "professional comic" more evident than in the court room. It is always conspicuous in convocations of the clergy, of physicians, and even of men of science. The professional comic is the all-enveloping rut and the discerning layman is usually the stone that throws the wheel out on to new ground. This is the value of the jury. The juror is a layman but unfortunately he is too much a strangled and manacled servant to have either a layman's self-respecting freedom or the self-imposed constrictions of professionalism. He is never quite himself because he is under duress involuntarily doing work of professional nicety—involving complex calculation and insight to human motives, a gift of interpretation, a sense of probability, the elimination of personal bias. He is physiologist, pathologist, physicist, psychologist, detective, financier, moralist, jurist. And he is only a lackey without personality.

It is comparatively seldom that the witness who is sworn to give the " whole truth " is permitted to do so. The juror sees the effort of counsel to prevent his getting hold of it. He notes how the wooden " rules of evidence " sometimes cut out a body of testimony, the

pores of which are juicy with the very quintessence of evidence. The keen juror, scenting the aroma, longs to tap or squeeze out the sap of truth. But who is he, forsooth, but a juryman?

A physician, speaking out of much experience as an expert witness, told his medical associates that it is not enough that the evidence which they might be called on to give should be the truth. This, indeed, he said, has little to do with it for it must agree with the lawyers' and the court's idea of what is evidence, which is sometimes quite another thing.

Meantime the poor juror is between the devil and the deep sea—between the professional battle to suppress facts and his own sense of essential truth; between his own real conscience and the artificially imposed conscience of the court. So long, therefore, as "the evidence" is prescribed by the court the juror ought not to be sworn to render verdict according to "the evidence," but without prejudice to strive to be just to all parties concerned. No other discipline is just to any party concerned.

There is strong tendency to reduce the employment of juries. The question is whether there could not be a better kind of jury, partaking at once of lay and of professional advantages. It would be perhaps a permanent board adequately paid, non political, smaller than our present jury, not bound by the rule of unanimity, having large authority in taking and using testimony, and treated with the dignity due to the upper officers of a court of justice. In other words the disciplinary irritants, drawbacks, indignities, coercion, threats and sources of indifference and personal bias should be reduced to the minimum. Only thus can the balances of the juror's mind work as freely as possible in the valuation of evidence—which evidence, in very truth is not limited to the sworn statements of witnesses or other facts of the testimony but grows out of the interpretation of experience and of human nature in the large. The system, whatever it is, should not indifferently permit or encourage self-defeat. Justice to the contending parties rests in no small degree on justice to the jury.

ON THE PRODUCTION OF AN ARTIFICAL HISS.

By E. B. TITCHENER.

(Read November 6, 1914.)

In *Nature* of May 29, 1913, Lord Rayleigh asked to be told "how to make an artificial hiss." I replied that, if Köhler's observations are reliable, "a Galton whistle, set for a tone of 8,400 v. d., will give a pure *s*."[1] Lord Rayleigh, however, was not impressed by the suggestion.[2]

It occurred to me that the question might be put to the test of experiment. The sound of a Galton whistle set for 8,400 v. d. might be imitated by the mouth, and a series of observations might be taken upon material composed partly of the natural (mouth) sounds and partly of the artificial (whistle) tones. If a listening observer were unable to distinguish between the two stimuli, and if the mouth-sound were shown, phonetically, to be a true hiss, then it would be proved that the whistle also gives an *s*, and Lord Rayleigh would be answered.

The experiment was more troublesome that I had anticipated; but I may say at once that it has been carried out, and with affirmative result.

We used an Edelmann-Galton whistle (No. 423) actuated by a rubber bulb.[3] Our first difficulty was to find a competent experimenter. For the sound of the whistle is clean-cut, uniform, so to say dogmatic. This very definite stimulus has to be duplicated by a certain setting of tongue and lips, and by voluntary regulation and

[1] *Nature,* July 3, 1913; W. Köhler, *Zeits. f. Psych.,* LXIV., 1913, 93.

[2] *Nature,* July 31, 1913.

[3] The bulb that comes with the instrument must soon be renewed. It may be worth while to point out that bulbs of white or grey sulphur-coated rubber should never be employed; the fine dust chokes the mouthpiece and plays havoc, *e. g.,* with terminal determinations. We use a black rubber that is slightly more flaccid than that furnished by Edelmann.

direction of a current of air. But not only are there gross differences in the mode of formation of the *s*-sound; there are also individual differences, due apparently to the arrangement of the teeth, the shape and size of the tongue, and so forth;[4] and beside these, there are differences in ability to maintain the sound begun, to hold it from fluctuation during its course. The sound that one emits, on first trying to imitate the whistle, may therefore be almost comically wrong,—broad, harsh, irregular, soft, wavery, instead of sharp and keen.

We presently found two experimenters, Mr. N. P. Stephens and Mr. P. T. Carson, who after practice were able in a large proportion of successive attempts to reproduce the sound of the whistle. Neither these nor the other volunteers whom we tried out could, however, imitate the sound obtained by the ordinary vigorous squeeze of the bulb, such a squeeze as one gives in determinations of the terminus. We therefore had recourse to a compression which, though sharp and definite, was still weaker than that by which the whistle is usually actuated. The difference between the mouth-sound and the whistle-sound given at full intensity is, so far as our observations go, that the latter is beady, intense, compact, while the former is broader, weaker, coarser. The compression which we used was, nevertheless, strong enough to yield clear dust-figures by the Kundt method.[5] No doubt, it varied somewhat, both in the experimental series and in the dust-figure determinations. But the hand-pressure becomes rather surprisingly even, with a little practice; and determinations made at different times by Dr. Foster varied only between the limits of 8,594 $\pm$ 63 v. d. and 8,522 $\pm$ 27 v. d. It is therefore certain that we were working in the near neighborhood of Köhler's optimal 8,400 v. d.

[4] See, *e. g.*, C. L. Merkel, "Physiologie der menschlichen Sprache," 1866, 186 ff., Taf. III.; G. H. von Meyer, "The Organs of Speech," 1884, 314f.; G. E. Sievers, "Grundzüge der Phonetik," 1885, 56 ff., 122 f.; O. Jespersen, "The Articulations of Speech Sounds," etc., 1889, 61 f., 87; H. Hoffmann, "Die Lautwissenschaft," etc., 1901, 62; W. Vietor, "Elemente der Phonetik," etc., 1887, 125 ff.; E. Seelmann, "Die Aussprache des Latein," etc., 1885, 245 f.; H. Sweet, "A Handbook of Phonetics," 1877, 33, 39 f.

[5] M. T. Edelmann, "Studien über die Erzeugung sehr hoher Töne, etc.," Drude's *Annalen,* II. [CCCVII.], 1900, 469 ff.

An ideal experimental series would now have consisted of equal numbers of mouth-sounds and whistle-sounds arranged in haphazard order. But the experimenters could not thus accurately reproduce the whistle-sound. If the series chanced to call for several mouth-sounds in succession, then the setting of the mouth could be maintained; but if mouth and whistle alternated, or if a single mouth-sound were to be given among several whistle-sounds, there was need of readjustment and possibility of initial failure. It seemed best that whenever the mouth-sound was obviously wrong, and when (as sometimes happened) the squeeze of the bulb was too light or too heavy, the experimenter should say: "Don't count that! Repeat!" and should simply try again. This procedure gave the observers a certain advantage; but we thought it preferable to a voluntary change or a further haphazard determination of the stimulus. The more successful of our two experimenters, Mr. Stephens, was obliged, even at his highest level of practice, to repeat the mouth-sound in 12 to 15 per cent. of the trials.

There was a further complication. The observers sat at a distance of not more than 1 m. from the source of sound; they declared that, if they were to judge discriminatingly, they must be as near as possible; and they tended to lean or move in toward the experimenter. The preliminary experiments showed that, under these conditions, their judgment might be influenced by secondary indications—the direction of the sound, the noise of breathing, of setting the mouth, even of the squeeze of the bulb, the noise of preparatory movements in general. Hence they were informed before the regular series began that these indications were not to be relied upon, but that the experimenter might in any given test make misleading preparations. In fact, 50 per cent. of the mouth-sounds were accompanied by noises of bulb and table, and 75 per cent. of the whistle-sounds by noises of breathing and mouth-setting. The numerical results and the introspective reports prove that this ruse was successful. The observers based their discriminations, for the most part, upon the temporal course and the "size" of the stimuli; the sound was judged to be "whistle" if it was hard, clear-cut, abrupt, and to be "mouth" if it was fluctuating, "trembly," soft, diffuse. Sometimes pitch was referred to (whistle higher), and sometimes

intensity (whistle louder). The number of confusions testifies that these differences were not very dependable.

The first series of experiments (July, 1914; Mr. Stephens, experimenter; Miss F. A. Bean, observer) consisted, as planned, of 70 mouth-sounds and 70 whistle-sounds taken in haphazard order. Aside from the disturbances to which I have referred, the results were:

	Whistle.	Mouth.
Whistle judged as..................	60	10
Mouth judged as....................	25	45

Mr. Stephens was at this date relatively unpractised, while Miss Bean had had extended practice in the discrimination of whistle-tones. The number of confusions (25 per cent. of the whole number of observations) was, evidently, large enough to warrant a continuation of the experiment.

Other series, made by Messrs. Stephens and Carson with other observers, brought results of the same numerical order; they need not be cited. I pass at once to the two final series made (August, 1914) by Mr. Stephens. The first comprised two part-series of 50 tests, each composed of 25 mouth-sounds and 25 whistle-sounds. The observer, Dr. W. S. Foster, knew the plan of the investigation, had himself tried to reproduce the whistle-tone by mouth, and had had recent and unusually extended practice in the discrimination of whistle-tones. The percentages of confusion were:

Whistle judged as mouth......................	18
Mouth judged as whistle......................	20

or an average confusion of 19 per cent.

In the second series, two part-series of 50 tests were composed, the one of 22 whistle and 28 mouth sounds, the other of 28 whistle and 22 mouth sounds. The observers, Dr. E. G. Boring, Dr. L. D. Boring and Dr. M. E. Goudge, sat together for the experiment. Dr. E. G. Boring had had a good deal of practice with the whistle, and the other observers had performed the regular laboratory experiments in which it is employed. All three were, however, given special practice (with knowledge) in the discrimination of the stimuli now to be used. The percentages of confusion were:

Observer.	Whistle Judged as Mouth.	Mouth Judged as Whistle.
E. G. B.	20	36
L. D. B.	45	38
M. E. G.	35	39

or an average confusion of 35.5 per cent.

There is, naturally, a tendency, on the part of the practised observers, to judge "mouth as whistle" more often than "whistle as mouth": the percentages are, for Miss Bean, 35.7 : 14.3, and for Dr. E. G. Boring, 36 : 20. Dr. Foster, who can hardly be deceived, gives approximately equal percentages of confusion; but in his case too the ratio 20 : 18 favors "mouth as whistle." The less practised observers, however, offset each other. I had expected a far greater preponderance of correct judgments of the whistle, *i. e.*, a lesser number of judgments of "whistle as mouth"; and I think that the percentages actually obtained speak well for the skill of the experimenter.

It remains to show that our mouth-sound was a hiss. Neither of the experimenters was versed in phonetics; but we asked them to observe and describe carefully the position of lips and tongue during imitation of the whistle-sound. Mr. Stephens writes:

"The position of the tongue is substantially, so far as I can judge, the same as that in which we produce the sound of the letter *s-s-s*. The sides of the tongue are so curled up that they rest against the inside of the upper teeth, on the sides. The middle of the tongue thus of course forms a hollow, up to the tip,—which very nearly touches the roof of the upper jaw just about a quarter-inch above the upper teeth . . . . For the production of a light hiss which is not to be heard loudly the tongue-muscles are semi-tightened as also are the muscles of the jaw and throat. The thin column of air which is forced lightly between the tip of the tongue and the point on the roof of the mouth makes production of sound. Teeth along sides and back are possibly 5 or 6 mm. apart, thus leaving plenty of opening for ejection of air. The lips stand a quarter-inch apart, with little or no drawing at the corners, for the light hiss. Lips, in the production of this sound, play little or no part; they merely are separated sufficiently so as not to interfere with air and sound. Unless they are well apart, however, they do interfere with the intensity and seeming pitch of the hiss."

This is a very fair amateur account of the production of a hiss; and if it is compared with the formula given, *e. g.*, by Jepersen, we

cannot doubt that Mr. Stephens was sounding an English *s*.[6] At the point where I have marked an omission, he draws a diagram which, with allowance made for amateur draughtmanship, is identical with the "[s] nach Bremer" of Jespersen's plate; it is needless to say that he had never opened Jespersen's book. Mr. Carson gives a very similar account, except that he appears to place the tip of his tongue a trifle further forward, and thus to approximate the German *s*. It is quite clear, then, that the experimenters were hissing.

So we have the artificial hiss that Lord Rayleigh asked for. It may be too weak for his purposes; and, more especially, it may be of too brief duration. We were able, however, to match the abrupt hiss of the experiments to a continuative hiss sounded by a second Edelmann whistle (No. 679) connected with the Whipple tanks: intensively, the match was only approximate; qualitatively, we regard it as fairly accurate.

For the qualitative determination we employed two methods. (1) The sound of whistle No. 423, actuated as in the experiments, was compared with three sounds from whistle No. 679 actuated by a regulated current of air from the tanks. These three sounds lay at what we supposed to be the point of equality with the sound of No. 423, at a pitch some 400 v. d. higher, and at a pitch some 400 v. d. lower. The three comparative pitches were intermixed in haphazard order; both time-orders were presented; and for the final series of observations we had the services of Professor H. P. Weld, a skilled musician as well as psychologist. (2) By a roundabout method of determination, which involved reliance on the Edelmann tables, we established the "identical" pitch of No. 679 as 8,430 v. d. Since Professor Weld's judgment made this pitch equal to or very slightly lower than the given pitch of No. 423, and since the error of our determination (provided always that Edelmann's tables are correct) can hardly have exceeded ± 100 v. d., we may assume that the two whistles gave very nearly the same *s*.

[6] O. Jespersen, "Lehrbuch der Phonetik," 1904, 34, 127 f. Mr. Stephens' use of the word "hiss" was spontaneous, not due to suggestion.

We were unable to determine the pitch of the continuative hiss by the Kundt dust-method; the lycopodium powder obstinately refused to move.

The accuracy of the Edelmann tables has been questioned by C. S. Myers ("On the Pitch of Galton Whistles," *Journ. of Physiol.*, XXVIII., 1902, 417 ff.). Edelmann does not give the m. v. of his scale readings; but it is possible that his technique is so accurate that the variation is minimal, or even that a single count suffices. Neither does he tell us how he compresses his bulb; it is probable that he uses some mechanical device which ensures a constant compression. We have ourselves made the following determinations with whistle No. 423 (temperature read as the mean of four thermometers):

(1) Ordinary vigorous squeeze, such as is employed in terminal determinations:
  Found from 5 trials with the dust-method.............. 8,897 ± 18 v. d.
  By Edelmann's table.................................. 8,775

(2) Weaker squeeze, used in our experiments:
  Found by dust-method................ 8,594 ± 63 v. d. to 8,522 ± 27 v. d.
  By Edelmann's tables + judgments of coincidence of tones 8,430

(3) Very violent squeeze, 10 trials by dust-method........ 9,046 ± 71 v. d.

It is clear that Edelmann uses a "normal" compression, of the same order as that which an experimenter naturally employs for terminal tests. Random determinations of our two whistles at different points of the scale, with the ordinary vigorous squeeze, agree within about 100 v. d. with the Edelmann tables.

We have had but little experience with continuous tones under change of water-pressure; but we find, so far, that the pitch of our whistles does not alter appreciably within the limits of 90 to 160 mm. of pressure.

While, then, we do not question the accuracy of Myers' determinations, we think that there is no need either to doubt the reliability of the Edelmann whistle under "normal" conditions.

MINUTES

MINUTES.

Stated Meeting January 2, 1914.

WILLIAM W. KEEN, M.D., LL.D., President, in the Chair.

Invitations were received:

From La Reale Accademia de Ciencias y Artes de Barcelona to be represented at the celebration of the 150th anniversary of its foundation on the 18th, 19th and 20th of January next.

From the Regents of the University of the State of New York to be represented at the Inauguration of John Huston Finley as President of the University and Commissioner of Education of the State of New York, at Albany on January 2d, 1914.

The list of donations to the library was laid on the table and thanks were ordered for them.

The decease was announced of James MacAlister, A.M., LL.D., at sea on December 11, 1913; æt. 74.

Prof. E. G. Conklin read a paper on "Some Facts and Factors of Development"; which was discussed by Doctors Donaldson and Dercum, and Prof. Conklin.

The judges of the annual election of officers and councillors held on this day between the hours of two and five in the afternoon, reported that the following named members, according to the laws, regulations and ordinances of the Society, were elected to be the officers for the ensuing year:

President.

William W. Keen.

Vice-Presidents.

William B. Scott,
Albert A. Michelson,
Edward C. Pickering.

Secretaries.

I. Minis Hays,
Arthur W. Goodspeed,
Amos P. Brown,
Harry F. Keller.

Curators.

Charles L. Doolittle,
William P. Wilson,
Leslie W. Miller.

Treasurer.

Henry LaBarre Jayne.

Councillors.

(To serve for three years.)
Samuel Dickson,
Ernest W. Brown,
Morris Jastrow, Jr.,
Arthur Gordon Webster.

Stated Meeting February 6, 1914.

WILLIAM W. KEEN, M.D., LL.D., President, in the Chair.

Mr. Samuel Rea, a newly elected member, subscribed the Laws and was admitted into the Society.

The decease was announced of the following members:

At St. Petersburg on January 2/15, 1914, Théodose Tschernyscheff, Director of the Geological Survey of Russia; æt. 67.

At Philadelphia on January 4, 1914, S. Weir Mitchell, M.D., LL.D.; æt. 84.

At Plattsburgh, N. Y., on January 7, 1914, William D. Marks; æt. 65.

At Cambridge, Mass., on January 14, 1914, Benjamin Osgood Peirce, Ph.D.; æt. 60.

At London on January 24, 1914, Sir David Gill, K.C.B.; æt. 70.

At Philadelphia on February 1, 1914, Charles E. Dana, C.E., æt. 71.

Dr. P. B. Hawk read a paper on "The Relationship of Water to Certain Life Processes, and More Especially to Nutrition"; which was discussed by Dr. Brubaker, Prof. Kraemer, Dr. Dercum, Prof. Keller and Mr. Carson.

Stated Meeting March 6, 1914.

WILLIAM W. KEEN, M.D., LL.D., President, in the Chair.

Invitations were received:

From the Deutsche Shakespeare Gesellschaft to participate in the celebration of the 50th Anniversary of its founding on April 22 to 24, 1914.

From the Premier Congrès de Police Judiciare Internationale to participate in the Congress to be held at Monaco on April 14 to 20, 1914.

From the 19th International Congress of Americanists to be held at Washington October 5 to 10, 1914, to be represented thereat.

The decease was announced of:

Edwin J. Houston, Ph.D., at Philadelphia, on March 1, 1914; æt. 67.

Stuart Wood, Esq., at Philadelphia, on March 2, 1914; æt. 62.

Dr. Samuel W. Stratton read a paper on "Standards of Quality;" which was discussed by Dr. Donaldson, Dr. Keller, Mr. Day and Mr. F. Rawle.

Stated Meeting April 3, 1914.

WILLIAM W. KEEN, M.D., LL.D., President, in the Chair.

A letter was received from Senatore Giovanni Celoria expressing the grateful thanks of the Committee for 283.25 lire contributed by the members of this Society as a subscription to the Giovanni Schiaparelli Memorial.

The decease was announced of:

Edward S. Holden, A.M., LL.D., at West Point on March 16th, 1914; æt. 68.

Sir John Murray, K.C.B., Sc.D., LL.D., at Edinburgh on March 16th, 1914; æt. 73.

The following papers were read:

"Explorations in the Hudson Bay Regions with references to Unusual Topographic and Hydrographic Features and Mineral Deposits," by Mr. Ambrose E. Lehman; which was discussed by Mr. Willcox, Mr. Garrison and Mr. DuBois.

"On Psychology as the Behaviorist Views It," by E. B. Titchener, Ph.D., Sc.D., LL.D.

Stated General Meeting April 23, 24 and 25, 1914.

WILLIAM W. KEEN, M.D., LL.D., President, in the Chair.

Thursday, April 23—Opening Session.

The decease was announced of George William Hill, Sc.D., LL.D., at West Nyack, N. Y., on April 18, 1914; æt. 76.

The following papers were read:

"The Physical Cause of the Unsymmetrical Equilibrium of the Earth Between the Land and Water Hemispheres, with a Theorem on the Attraction of the Terrestrial Spheroid," by T. J. J. See, Ph.D., U. S. Naval Observatory, Mare Island, California.

"Some Observations on the Psychology of Juries and Jurors," by Patterson DuBois, Esq., of Philadelphia.

"Factors of Influence in the Origin and Circulation of the Cerebro-Spinal Fluid," by Charles H. Frazier, M.D., Professor of Clinical Surgery, University of Pennsylvania. Discussed by Dr. Crile.

"Aspects and Methods of the Study of the Mechanism of the Heart Beats," by Alfred E. Cohn, A.B., M.D., Associate in Medicine, Rockefeller Institute for Medical Resarch, New York. (Introduced by Dr. Keen.) Discussed by Dr. Keen.

"The Kinetic System," by George W. Crile, M.D., Professor of Clinical Surgery, Western Reserve University, Cleveland. Discussed by Dr. Keen and Dr. W. P. Wilson.

"The Hereditary Basis of Certain Emotional States," by Charles

B. Davenport, A.M., Ph.D., Director of Station for Experimental Evolution (Carnegie Institution), Cold Spring Harbor, New York.

"Syriac Socrates—A Study in Syrian Philosophy," by W. Romaine Newbold, Ph.D., Professor of Philosophy, University of Pennsylvania.

"The Evolution of Pine Barren Plants," by John W. Harshberger, Ph.D., Professor of Botany, University of Pennsylvania.

"Segretion of 'Unit Characters' in the Zygote of Oenothera with Twin and Triplet Hybrids in the First Generation," by George Francis Atkinson, Ph.D., Professor of Botany, Cornell University. Discussed by Prof. Bradley M. Davis and Prof Kraemer.

"The Vegetation of the Sargasso Sea," by William G. Farlow, Ph.D., LL.D., Professor of Cryptogamic Botany, Harvard University. Discussed by Prof. Harshberger, Prof. Atkinson, and Prof. Farlow.

"A New Type of Oak for America," by William Trelease, Sc.D., LL.D.

Friday, April 24.

Executive Session—9:30 o'clock.

William W. Keen, M.D., LL.D., President, in the Chair.

The proceedings of the officers and council were submitted and the list of nominations for membership recommended for election this year was reported.

The following resolution recommended by the officers and council was adopted:

That a Committee of eight members shall be appointed by the president at the first executive session of the general meeting in every year to nominate officers and councillors for the succeeding year. The president shall be ex-officio a member of this committee, but shall have no vote except in case of a tie. This committee shall report at the stated meeting on the first Friday in December and its

report shall be regarded as nominations duly made for the respective offices. But nothing herein contained shall be considered as restricting the right of any individual member to make nominations as provided in the Laws.

Members are invited to make to this Committee suggestions of appropriate nominations and it shall be its duty to give full consideration to the same.

The President thereupon appointed the following members to constitute the Committee: Dr. Hays, Prof. E. W. Brown, Prof. Trelease, Dr. R. S. Woodward, Hon. Charlemagne Tower, Dr. Donaldson, Prof. Gummere, Prof. Farlow.

Morning Session—9:35 o'clock.

ALBERT A. MICHELSON, Ph.D., Sc.D., LL.D., F.R.S.

Vice-President, in the Chair.

The following papers were read:

"Phase Changes Produced by High Pressures," by Percy W. Bridgman, Instructor in Physics in the Jefferson Physical Laboratory, Harvard University. (Introduced by Prof. Goodspeed.) Discussed by Professors Pickering, Hobbs, Bogert, Noyes and Michelson.

"The Influence of Atmospheric Pressure on the Forced Convection of Heat from Thin Electric Conducting Wires," by Arthur E. Kennelly, Sc.D., Professor of Electrical Engineering, Harvard University.

"Some New Tests of Quantum Theory and a Direct Determination of *h*," by Robert Andrews Millikan, A.M., Ph.D., Professor of Physics, University of Chicago. (Introduced by Prof. Goodspeed.) Discussed by Prof. Michelson.

Discussion of "A Kinetic Theory of Gravitation": (1) Gravitation is Due to Intrinsic Energy of the Ether; (2) Transmission of Gravitation Cannot be Instantaneous, by Charles F. Brush, Ph.D., Sc.D., LL.D., of Cleveland. Discussed by Prof. Nipher.

"Behavior of Metals and Other Substances Under Stress Near the Rupture Point," by A. A. Michelson, Ph.D., LL.D., Professor of Physics, University of Chicago. Discussed by Prof. E. W. Brown, Mr. Bridgman, and Mr. Brush.

"On Highly Radio-Active Solutions," by William Duane, A.M., Ph.D., Assistant Professor of Physics and Research Fellow of the Cancer Commission, Harvard University. (Introduced by Prof. Goodspeed.) Discussed by Mr. Brush and Dr. Donaldson.

"Some Further Considerations in the Development of the Electron Conception of Valence," by K. G. Falk, of the Harriman Research Laboratory, Roosevelt Hospital, New York. (Introduced by Prof. Bogert).

"The Valence of Nitrogen in Ammonium Salts," by William Albert Noyes, Ph.D., LL.D., Professor of Chemistry, University of Illinois, and R. S. Potter. (Introduced by Prof. H. C. Jones.)

"Determination of the True Atomic Weight of Radium," by Gustavus Hinrichs, of St. Louis.

Afternoon Session—2 o'clock.

EDWARD S. PICKERING, D.Sc., LL.D., F.R.S.,

Vice-President, in the Chair.

Dr. Cyrus Adler in presenting a portrait of the late Samuel Pierpont Langley, LL.D., a former Vice-President of the Society, spoke as follows:

On behalf of a number of members of the Society I have the honor to present a portrait of Samuel Pierpont Langley, a former member and Vice President of the American Philosophical Society.

Mr. Langley, the third Secretary of the Smithsonian Institution, was a man of national and international fame which rested primarily upon his epoch making researches in solar physics. All of the recognition which came to him was based upon his discoveries in physics and astronomy. But he was also a pioneer in another field, being the first distinguished man of science to devote himself to the

subject of aerial navigation, at a time when this was not considered within the realm of scientific study. The mere fact that a man of his reputation and position gave serious attention to this important subject lent it an impetus and standing which it would not have otherwise received for many years and therefore greatly advanced the development of this science and art. But he did more than give an impetus, for he not only discovered principles of prime importance in connection with aerodynamics, but was the first to produce a machine heavier than air, supported and propelled by its own engine and possessed of no extraneous or lifting power which actually made an independent flight.

The first flight of a heavier than air machine which ever occurred took place over the Potomac River on May 6, 1896 and was succeeded by numerous other flights of various models, which he built, all of the monoplane type.

He had received honorary degrees from Oxford and Cambridge Universities in England and from Harvard, Michigan, Princeton and Wisconsin in the United States. Medals were awarded him by the National Academy of Sciences, the Royal Society of London, the American Academy of Arts and Sciences, the Institute of France and the Astronomical Society of France and he was a member or correspondent of all of these and many other learned Societies including the Academia dei Lincei of Rome.

He had an especial affection for the American Philosophical Society, one of the few organizations in which he accepted an office.

In addition to these great achievements, Mr. Langley was a man of wide culture and of deep sympathy and insight. That I was permitted to enjoy his friendship was one of the most profoundly valued and touching experiences of my life.

[The donors are Messrs. Cyrus Adler, Carl Barus, L. A. Bauer, Alex. Graham Bell, John A. Brashear, George F. Edmunds, George E. Hale, David Jayne Hill, George Gray, T. C. Mendenhall, Charles E. Munroe, Edward L. Nichols, Richard Olney, Henry Fairfield Osborn, Edward C. Pickering, Raphael Pumpelly, Edward B. Rosa, Frank Schlesinger, Samuel W. Stratton, Mayer Sulzberger, Elihu Thomson, Otto H. Tittmann, Charles D. Walcott, Andrew D. White and Robert S. Woodward.]

VICE-PRESIDENT PICKERING in accepting the portrait, said:

My acquaintance with Samuel Pierpont Langley goes back to the winter of 1870, when we crossed the ocean together to observe the total eclipse of the sun, in Spain. We maintained an unbroken friendship until his death, nearly forty years later. As a young man he was enthusiastic, and full of hope for the future. When placed in charge of the observatory at Pittsburgh, he found that the smoke in the atmosphere rendered stellar observations difficult. He therefore selected the sun as his object for study, since the smoke by cutting off the heat, rendered the air more steady. A skillful draughtsman, his drawing of that complicated object, a large sunspot, is probably the best ever made. In accepting the position of secretary of the Smithsonian Institution, he stipulated that he should be enabled to continue his scientific investigations. This led to the establishment of the astrophysical observatory, which has continued and extended his work to the present time. He devoted many years to the construction and improvement of the bolometer, one of the most delicate devices for measuring heat, and a most difficult instrument to adjust and use. For many years, the question of artificial flight had an absorbing interest for him. His investigations were long and laborious, and finally he attained success with a small model. When constructing a larger instrument, his sensitiveness induced him to avoid publicity, thus greatly annoying those whose business it is to keep the public informed of the latest news. They had their revenge when a misplaced nail in his launching apparatus ruined his aeroplane on its trial trip, and the subsequent ridicule and criticism saddened his last days, and shortened his life. The success of aerial navigation is largely due to his work, which has only received the credit it deserves since his death.

Langley, by his devotion to the advancement of human knowledge, well deserves a place among those whose portraits adorn these walls, and in the name of the American Philosophical Society held at Philadelphia for Promoting Useful Knowledge, I accept this portrait and extend grateful acknowledgments to the donors.

The following papers were read:

"The Magnetic Phenomena of Sun-spots."

"The General Magnetic Field of the Sun." (Illustrated with lantern slides.) By George E. Hale, Ph.D., LL.D., F.R.S., Director of the Solar Observatory of the Carnegie Institution at Mt. Wilson, Cal.

"On the Colors of the Stars in the Cluster M 13," by Edward E. Barnard, Sc.D., LL.D., Astronomer of the Yerkes Observatory, Williams Bay, Wis.

"The Use of a Photographic Doublet in Cataloguing the Position of Stars," by Frank Schlesinger, M.A., Ph.D., Director of the Allegheny Observatory, Allegheny Pa. Discussed by Prof. Pickering.

"The Distribution in Space of 90 Eclipsing Stars," by Henry Norris Russell, Ph.D., Professor of Astronomy, Princeton University.

"The Eclipsing variable Stars ψ Orionis and 88 d Tauri," by Harlow Shapley, Ph.D., of Princeton University Observatory. (Introduced by Prof. H. N. Russell.)

"Some Features of The Moon's Motion and a Problem in Isostasy," by Ernest W. Brown, M.A., Sc.D., F.R.S., Professor of Mathematics, Yale University.

"The United States as a Factor in World Politics," by Leo S. Rowe, Ph.D., LL.D., Professor of Political Science, University of Pennsylvania.

"A Sumerian Nature Hymn from Nippur, of the Time of the Dynasty of Agade, 2800–2600 B. C.," by George A. Barton, Ph.D., Professor of Biblical Literature, Bryn Mawr College.

Evening Session—8:15 o'clock.

Arthur L. Day, Ph.D., Director of the Geophysical Laboratory of the Carnegie Institution of Washington, gave an illustrated lecture on "Some Observations of the Volcano Kilauea in Action."

Saturday, April 25.

Executive Session—9:30 o'clock.

WILLIAM W. KEEN, M.D., LL.D., President, in the Chair.

Prof. George F. Atkinson and Prof. Charles Edwin Bennett, recently elected members, subscribed the Laws and were admitted into the Society.

Pending nominations for membership were read and spoken to.

Mr. J. Edward Whitfield and Dr. James McKeen Cattell were appointed tellers of election and the Society proceeded to ballot for members.

The tellers of election reported that the following nominees had been elected to membership:

Residents of the United States.

Charles Greeley Abbot, S.B., Washington.
James Wilson Bright, Ph.D., LL.D., Litt.D., Baltimore.
Bradley Moore Davis, A.M., Ph.D., Philadelphia.
Thomas McCrae, A.B., M.D., Philadelphia.
William Diller Matthew, A.M., Ph.D., New York.
Alfred Goldsborough Mayer, Ph.D., M.E., Washington.
Samuel Jones Meltzer, M.D., LL.D., New York.
John Campbell Merriam, B.S., Ph.D., Berkeley, Cal.
Robert Andrews Millikan, A.M., Ph.D., Chicago.
William Albert Noyes, Ph.D., LL.D., Urbana, Ill.
Stewart Paton, M.D., Princeton.
Richard Mills Pearce, Jr., M.D., Philadelphia.
Palmer Chamberlaine Ricketts, C.E., LL.D., Troy.
Harold A. Wilson, M.A., D.Sc., F.R.S., Houston.
Frederick Eugene Wright, Ph.D., Washington.

Foreign Residents.

Shibasaburo Kitasato, M.D., Tokyo.
Heike Kamerlingh Onnes, Ph.D., Leyden.
Vito Volterra, Sc.D., Ph.D., Rome.

Morning Session—10 o'clock.

William B. Scott, Sc.D., LL.D., Vice-President, in the Chair.

The following papers were read:

"Primary Cambrian Manganese Deposits of Newfoundland," by Nelson C. Dale, Fellow, Princeton University. (Introduced by Prof. W. B. Scott.)

"Geology of the Wabana Iron Ores of Newfoundland," by Albert O. Hayes, Fellow, Princeton University. (Introduced by Prof. W. B. Scott.)

"Hewettite, Metahewettite and Pascoite, Hydrous Calcium Vanadates," by W. F. Hillebrand, Ph.D., H. E. Merwin and Frederick E. Wright, Ph.D., of U. S. Geological Survey.

"The Relations of Isostasy to a Zone of Weakness—the Asthenosphere," by Joseph Barrell, Ph.D., Professor of Structural Geology, Yale University. (Introduced by Prof. Charles Schuchert.)

"Evidence for a Pulsational Change of Climate in the Libyan Desert," by William H. Hobbs, Ph.D., Professor of Geology, University of Michigan.

"The Cretaceous-Tertiary Boundary in the Rocky Mountain Region," by F. H. Knowlton, U. S. Geological Survey. (Introduced by Prof. John M. Clarke.) Discussed by Prof. W. B. Scott.

"The Geologic and Biologic Results of a Study of the Tertiary Floras of Southeastern North America," by Edward W. Berry, Associate Professor of Paleontology, Johns Hopkins University. (Introduced by Prof. William B. Clarke.) Discussed by Prof. W. B. Scott.

"On Multiple Treatment of One and the Same Story 'Motif,'" by Maurice Bloomfield, Ph.D., LL.D., Professor of Sanskrit, Johns Hopkins University.

"Some Biblical Miracles," by Paul Haupt, Ph.D., LL.D., Professor of Semitic Philology, Johns Hopkins University.

"The Sumerian Pronunciation of the Name 'Ninib' as the Chief Deity of Umma," by Alfred T. Clay, Ph.D., Laffan

Professor of Assyriology and Babylonian Literature, Yale University.

"Panama Tolls and Tonnage Rules," by Emory R. Johnson, Ph.D., Professor of Transportation and Commerce, University of Pennsylvania.

"Passamaquoddy Morphology," by J. Dyneley Prince, A.B., Ph.D., Professor of Semitic Languages, Columbia University, New York.

Afternoon Session—2 o'clock.

WILLIAM W. KEEN, M.D., LL.D., President, in the Chair.

Prof. William Albert Noyes, of Urbana, Ill., and Prof. Bradley Moore Davis, of Philadelphia, newly elected members, subscribed the Laws and were admitted into the Society.

Dr. WILLIAM G. FARLOW unveiled a Wedgwood medallion portrait of the late Sir Joseph Dalton Hooker, O.M., G.C.S.I., C.B. and spoke as follows:

Today we are so fortunate as to be able to add a medallion of one of the world's great botanists to the already large number of memorials of distinguished men which adorn this Hall and give it a dignity which is justly envied by other scientific societies in this country. Joseph Dalton Hooker, the more distinguished son of a distinguished father, was born in Halesworth, Suffolk, England, in 1817 and, retaining his scientific activity until the last, died at Sunningdale in 1911, a record very rarely equalled.

When four years of age his father, Sir William Hooker, removed to Glasgow, where he had been appointed Professor of Botany in the University, so that from his early childhood, the son was placed in surroundings which naturally pointed to botany as his life work. While a student of medicine, Hooker had the opportunity of reading the proofs of Darwin's "Voyage of the Beagle" which aroused in him an intense desire to travel. This desire was fortunately soon gratified, for immediately after receiving the degree of Doctor of Medicine in 1839, he was appointed assistant surgeon and botanist to the *Erebus* under the command of Sir James Ross, then about to start on his memorable voyage to the Antarctic regions.

On his return in 1843 Hooker made his home at Kew where his father had been appointed director of the Royal Gardens. He was appointed assistant director in 1855 and, on the death of his father in 1865, director, which position he held till his retirement twenty years later. We always think of Hooker as at Kew. It was there, aided by the large collections formed in great part by his father and himself, that he finished his different floras; there that he brought to perfection the Garden which had been raised by his father from insignificance to be the leading botanical garden of the world; there that many American botanists were received with a cordiality doubly welcome because they were encouraged by his sympathy and aided by his advice.

Hooker was undoubtedly the leading botanical systematist of his day. For this branch of botany he not only had great natural ability, but he also had opportunities for studying in the field the floras of distant and little-explored regions such as few trained botanists have had. Besides his Antarctic voyage, when he visited New Zealand and Tasmania as well as more southern regions, he spent the years 1848 to 1851 in an exploration of the Himalayas in company with Thomas Thomson,—an expedition involving great hardships among hostile people, but rich in results, and later he made trips to Palestine and Morocco. On his last long journey in 1877, he travelled with his old friend, Asa Gray, among the Rocky Mountains and in California.

On this occasion we need not consider in detail Hooker's various descriptive works on the floras of the countries he had visited, nor works like the great "Genera Plantarum," written in collaboration with Bentham, technical systematic treatises belonging to the classics of botany. Let us recall rather those qualities of Hooker which made him more than a systematist, which entitled him to rank with Darwin, Wallace, Lyall and Huxley in the brilliant group of naturalists which has never been surpassed, if it has ever been equalled, in any other country. Like Darwin, Hooker began his botanical career as an explorer of remote regions. The delightful account of the "Voyage of the Beagle" has its counterpart in the "Himalayan Journals" of Hooker. In both we recognize the fact that the

authors were something more than interesting relators of what they had seen. To them biological facts were only significant as indicating so many steps in the sequence of cause and effect. The genius of Darwin was manifested in his ability to see clearly in the beginning of his career the true direction in which the facts he had observed pointed, so that his lifework was unified, one step leading inevitably to another in the development of a great theory. It was much the same with Hooker. The writings which mark him as a philosophical botanist are the "Introductory Essay to the Flora of Tasmania"; the "Essay on the Distribution of Arctic Plants"; the "Discourse on Insular Floras"; the address at York on "Geographical Distribution," and the "Essay on the Vegetation of India," publications extending over a period of forty years.

The study of plant distribution involving a consideration of the geological phenomena which could account for it, and also of the question as to the effect of altered environment in modifying the characters of plants, naturally led to the fundamental question: What are Species and what are Varieties? One who, like Hooker, was master of the facts and without prejudice, could not fail to recognize that species are not fixed creations, but transitional stages in the progress of evolution. Hooker was a Darwinian even before the appearance of the "Origin of Species." It has been said with truth that, with the exception of Wallace, Hooker was the first adherent of Darwin in his views on evolution. How much that means is hardly realized at the present day. With us the question is not whether there is such a thing as evolution in plants and animals. We accept evolution as a fact, and, if there be any question, it is as to whether the explanation of the mode of its operation as presented by Darwin was satisfactory in all its details. In 1859, however, the date of the publication of the "Origin of Species," and for a considerable number of years later it required a good deal of courage as well as an unbiased mind for anyone, especially for an Englishman, to declare his assent to the revolutionary views advanced by Darwin.

In closing I may be permitted to repeat the words describing the position of Hooker among botanists on the occasion of the presentation to him of the Linnean gold medal at Stockholm in 1907.

"By scientific expeditions to many different parts of the world he has revealed the secrets of their vegetation. His extraordinary experience embraces both the nature of tropical India as also the subtropical and temperate climates, as well as of the cold antarctic regions. The contents of his floristic works are therefore exceedingly rich. He has furthermore enriched botany by splendid works in other departments of this science, for instance concerning the geographical distribution of plants, their classification and other matters."

I have the honor of unveiling the medallion of Sir Joseph Dalton Hooker, the energetic explorer, the eminent systematist, the distinguished investigator of the problem of plant evolution.

The following papers were read:

"The Burgess Shale Fauna of the Canadian Rockies," by Charles D. Walcott, Ph.D., Sc.D., LL.D., Secretary of the Smithsonian Institution, Washington.

"Summary of Researches, Department of Terrestrial Magnetism, 1904–14." (Illustrated.) By Louis A. Bauer, Ph.D., D.Sc., Director of the Department of Terrestrial Magnetism of the Carnegie Institution, Washington, D. C. Discussed by Prof. Pickering.

Symposium on Physics and Chemistry of Protoplasm:

"The Germ Plasm as a Stereochemic System," by Edward T. Reichert, M.D., Prof. Physiology in Univ. of Penna. (Introduced by Dr. Keen.)

"Arrangement and Distribution of Substances in the Cell," by Edwin Grant Conklin, Ph.D., Sc.D., Professor of Zoology at Princeton University.

"Vital Staining of Protoplasm," by Herbert McLean Evans, M.D., Associate Professor of Anatomy, Johns Hopkins University. (Introduced by Prof. Piersol.)

"The Physical State of Protoplasm," by G. L. Kite, M.D., Ph.D., Phipps Institute, Philadelphia. (Introduced by Prof. McClung.)

"The Physico-Chemical Organization of the Cell," by Lawrence J. Henderson, A.B., M.D., Assistant Professor of Biological Chemistry, Harvard University. (Introduced by Dr. H. F. Keller.)

Stated Meeting May 1, 1914.

William W. Keen, M.D., LL.D., President, in the Chair.

Dr. Thomas McCrae, of Philadelphia, a newly elected member, subscribed the Laws and was admitted into the Society.

Letters accepting membership were received from:

Charles Greeley Abbot, S.B., Washington.
Bradley Moore Davis, A.M., Ph.D., Philadelphia.
Thomas McCrae, A.B., M.D., Philadelphia.
Samuel James Meltzer, M.D., LL.D., New York.
William Albert Noyes, Ph.D., LL.D., Urbana, Ill.
Stewart Paton, M.D., Princeton.
Richard Mills Pearce, Jr., M.D., Philadelphia.
Palmer Chamberlaine Ricketts, C.E., LL.D., Troy.
Frederick Eugene Wright, Ph.D., Washington.

The list of donations to the library was laid upon the table and thanks were ordered for them.

The decease was announced of George F. Baer, at Philadelphia, on April 26, 1914, in the 72d year of his age.

Prof. J. Russell Smith read a paper on "Tree Breeding with Relation to Conservation and the Food Supply," which was discussed by Prof. Bradley M. Davis and Dr. Keen.

Hon. Charlemagne Tower, Chairman, presented and read at length the report of the Committee on the Date of Origin of the Society.

On motion, by unanimous vote, the report was accepted; the year 1727 was declared to be the date of foundation of the Society, in accordance with the finding of the committee; and the committee was discharged with the thanks of the Society for its exhaustive report.

Stated Meeting October 2, 1914.

William W. Keen, M.D., LL.D., President in the Chair.

Dr. Richard Mills Pearce, a newly elected member, subscribed the Laws and was admitted into the Society.

Letters accepting membership were received from:

James Wilson Bright, Ph.D., LL.D., Litt.D., Baltimore.

William Diller Matthew, A.M., Ph.D., New York.
Alfred Goldsborough Mayer, Ph.D., M.E., Washington.
John Campbell Merriam, B.S., Ph.D., Berkeley.
Robert Andrews Millikan, A.M., Ph.D., Chicago.
Harold A. Wilson, M.A., D.Sc., F.R.S., Houston.
Shibasaburo Kitasato, M.D., Tokyo.
Heike Kamerlingh Onnes, Ph.D., Leyden.
Vito Volterra, Sc.D., Ph.D., Rome.

Invitations were received:

From the Ohio State Archaeological and Historical Society, to be represented at the dedication of its Museum and Library Building, at Columbus, on May 30th.

From the University of Missouri, to be represented at the 75th Anniversary of its founding on June 3d.

The decease was announced of the following members:

Edward Suess, Ph.D., at Vienna, on April 29, 1914, æt. 73.

William Aldis Wright, LL.D., D.C.L., Litt.D., at London, on May 19, 1914.

John Robert Sittlington Sterrett, Ph.D., LL.D., at Ithaca, N. Y., on June 16, 1914, æt. 63.

Frederick William True, M.S., LL.D., at Washington, on June 25, 1914, æt. 56.

John Barnard Pearse, at Roxbury, Mass., on August 24, 1914, æt. 72.

Morris Longstreth, A.M., M.D., at Barcelona, Spain, on Sept. 19, 1914, æt. 68.

Special Meeting October 30, 1914.

William W. Keen, M.D., LL.D., President in the Chair.

A paper was read on "A new Means of Studying Submarine Animal and Vegetable Life," illustrated by moving pictures of tropical deep sea flora and fauna.

Stated Meeting November 6, 1914.

William W. Keen, M.D., LL.D., President, in the Chair.

The decease of the following members was announced:

Léon de Rosny, at Fontenay-aux-Roses (Seine), on August 28, 1914, æt. 78.

Theodore Nicholas Gill, M.D., Ph.D., at Washington, on September 25, 1914, æt. 77.

The following papers were presented:

"On Wireless Longitude Determination," by Eric Doolittle. Discussed by Professor C. L. Doolittle, Professor Snyder, Mr. Mitchell and Professor Miller.

"On the Production of an Artificial Hiss," by E. B. Titchener, Ph.D., D.Sc., LL.D.

Stated Meeting December 4th, 1914.

William W. Keen, M.D., LL.D., President, in the Chair.

An invitation was received from the Chairman of the 11th Annual Conference of Historical Societies to be held in Chicago in connection with and as part of the Thirtieth Annual Meeting of the American Historical Association, December 28 to 31, to participate in the conference by appointing delegates.

The decease was announced of

August Weismann, D.Ph., D.C.L., at Freiburg on November 5, 1914, æt. 80.

Charles Sedgwick Minot, M.D., Sc.D., LL.D., at Boston on November 19, 1914. æt. 61.

Alfred Thayer Mahan, LL.D., D.C.L., Rear Admiral U.S.N, at Washington on December 1, 1914. æt. 74.

Dr. John Mason Clarke of Albany read a paper on "The Magdalen Islands—a Relict Archipelago," which was discussed by Mr. Willcox and Prof. Pilsbry.

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MAGELLANIC PREMIUM

FOUNDED IN 1786 BY JOHN HYACINTH DE MAGELLAN, OF LONDON

1915

THE AMERICAN PHILOSOPHICAL SOCIETY

HELD AT PHILADELPHIA, FOR PROMOTING USEFUL KNOWLEDGE

ANNOUNCES THAT IN

DECEMBER, 1915

IT WILL AWARD ITS

MAGELLANIC GOLD MEDAL

TO THE AUTHOR OF THE BEST DISCOVERY, OR MOST USEFUL INVENTION, RELATING TO NAVIGATION, ASTRONOMY, OR NATURAL PHILOSOPHY (MERE NATURAL HISTORY ONLY EXCEPTED) UNDER THE FOLLOWING CONDITIONS :

1. The candidate shall, on or before November 1, 1915, deliver free of postage or other charges, his discovery, invention or improvement, addressed to the President of the American Philosophical Society, No. 104 South Fifth Street, Philadelphia, U. S. A., and shall distinguish his performance by some motto, device, or other signature. With his discovery, invention, or improvement, he shall also send a sealed letter containing the same motto, device, or other signature, and subscribed with the real name and place of residence of the author.

2. Persons of any nation, sect or denomination whatever, shall be admitted as candidates for this premium.

3. No discovery, invention or improvement shall be entitled to this premium which hath been already published, or for which the author hath been publicly rewarded elsewhere.

4. The candidate shall communicate his discovery, invention or improvement, either in the English, French, German, or Latin language.

5. A full account of the crowned subject shall be published by the Society, as soon as may be after the adjudication, either in a separate publication, or in the next succeeding volume of their Transactions, or in both.

6. The premium shall consist of an oval plate of solid standard gold of the value of ten guineas, suitably inscribed, with the seal of the Society annexed to the medal by a ribbon.

All correspondence in relation hereto should be addressed

TO THE SECRETARIES OF THE

AMERICAN PHILOSOPHICAL SOCIETY

No. 104 SOUTH FIFTH STREET

PHILADELPHIA, U. S. A.

PROCEEDINGS

OF THE

American Philosophical Society

HELD AT PHILADELPHIA

FOR

PROMOTING USEFUL KNOWLEDGE

VOLUME LIV

1915

PHILADELPHIA
THE AMERICAN PHILOSOPHICAL SOCIETY
1915

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OF THE

AMERICAN PHILOSOPHICAL SOCIETY

HELD AT PHILADELPHIA

FOR PROMOTING USEFUL KNOWLEDGE

VOL. LIV JANUARY–APRIL, 1915 No. 216

THE PROBLEM OF ADAPTATION AS ILLUSTRATED BY THE FUR SEALS OF THE PRIBILOF ISLANDS.

BY GEORGE H. PARKER.

(*Read April 23, 1915.*)

The breeding habits of the Alaskan fur seals are so unusual as to make these animals unique among mammals. During much of the year, these seals are strictly pelagic roaming over the eastern expanse of the northern Pacific as far southward as the latitude of southern California. As summer approaches, practically the whole herd consisting of several hundreds of thousands of individuals repairs to the two small islands of St. George and St. Paul in Bering Sea for the breeding season. It is the relative proportions of the various constituents of the herd during this breeding period that affords material for interesting speculation.

The movements of the fur seals in their arrival and departure from the Pribilof Islands take place with much regularity. Early in May and June the mature males or bulls, having made their way through the passes of the Aleutians, reach the breeding beaches or rookeries on the islands of St. George and St. Paul. Here they take their positions, fighting all intruders while they await the coming of the females or cows. The cows arrive on the islands chiefly during June and July. They associate themselves with particular bulls and the bull with his group of cows constitutes the family unit or harem. In 1914 the average harem was not far from one bull

to sixty cows, and the range extended from harems containing one cow to some that contained over a hundred. Because of the many years of commercial killing, chiefly directed against the males, it is impossible to state what the size of the normal average harem should be, but probably not far from one bull to thirty or forty cows.

Within a short time after the arrival of the cow, in the harem, *i. e.*, within a few days or a week or so, she gives birth to a single young or pup. So far as is known, cows do not produce more than one pup at a time. Shortly after the birth of her pup, the cow goes into heat, pairs with the bull, and becomes pregnant again. As these are annual occurrences, the period of gestation in the fur seal must be a few days less than a year. The pups are born males and females in about equal numbers. The counts of former years, as well as those of 1914, show a slight predominance of males, the excess being from a little over two per cent. to about seven per cent. of the total births.

The breeding season closes toward the end of July or early in August and this close is marked by the disintegration of the harems. During August most of the bulls begin their migrations back to the Pacific, and the pups, which heretofore have remained on the beaches, begin to take to the sea. They and the cows stay about the islands till November, when they too start on their migration to the open ocean. The only important constituent of the herd that has not yet been mentioned is the class known as the bachelors, *i. e.*, the young males that have not yet attained to breeding. The bachelors move with the cows, arriving for the most part in June and July, and departing in November, though some are found on the islands in December or even later. The bachelors do not mingle on the beaches with the rest of the herd, but gather to one side of the breeding grounds proper in the so-called bachelors hauling grounds, where they lead an idle rollicking existence suggested by their name.

The maximum age of the fur seal is believed to be about twelve to fourteen years for both males and females. In the migration, the males return to the islands approximately in the sequence of their ages; the old bulls arrive first in May and June followed by the younger bulls and bachelors and lastly by the yearling males, which arrive in the latter part of July and in August. The year-

ling males on arrival associate with the pups and cows rather than with the other bachelors. The bachelors may begin breeding at five years of age or even four, but they do not normally undertake this function until they are six or seven years old, when they desert the bachelors' hauling grounds for the breeding rookeries. The period of their normal breeding life covers, therefore, a term of perhaps some seven years or more.

It is not impossible that the yearling females do not return to the islands or, if they do, it is probable that they do so only in small numbers and late in the season. The two-year-old females return to the islands in July and August as virgin females, pair with the younger bulls, and reappear a year later, the end of their third year, with their first pup. From that time on they enter into the regular breeding of the herd and continue in all probability to produce one pup annually. Their breeding life, therefore, extends over some ten or more years.

These in brief are the main facts concerning the breeding habits of the Alaskan fur seal, an animal that exhibits one of the most remarkable examples of concentrated and localized breeding known. When it is recalled that these seals range over thousands of miles in the northern Pacific and that all sexually active members of the species without exception congregate in the appropriate season on the two small islands of St. George and St. Paul for breeding, the very exceptional nature of their reproductive activities must be evident.

The proportion of the two sexes at birth is very nearly equal, yet when the breeding age has been reached, the natural relations are not far from one male to thirty or forty females. As there is no reason to suppose that the death rate is higher in males than in females and as the length of the breeding life of the two sexes is not very different, about seven years for the bulls and about ten for the cows, it follows from the sexual proportions already mentioned, that we should expect an excess of bulls to be present. As a matter of fact, such is the case, for even in 1914, after the excessive commercial killing of males in the past, the so-called idle bulls were much in evidence. It thus appears that the Alaskan fur seal produces at birth approximately equal numbers of males and females

and yet in its breeding activities needs only relatively few males, a condition which when viewed as a whole seems to be a misadjustment rather than a close adaptation to the actual needs of the species. The measure of this misadjustment would be the proportion of idle bulls naturally present. Unfortunately, the commercial activities of the past in exploiting the herd for its fur prevent the possibility of accurate statement on this point, but the presence of idle bulls in the herd today is enough to show that this class under natural conditions would be abundantly represented.

The fur seal, however, is not the only one of the higher animals to show this misadjustment in the ratio of males to females. A prosaic example of the same kind is seen in the barn-yard fowl. Here the sexes hatch in nearly equal numbers, there being perhaps a slight predominance of females, but in maturity the cock holds sway over a flock of hens. This condition is almost exactly parallel with that of the fur seal except that it occurs under domestication. Nevertheless it has probably been inherited from the wild stock, for Finn states that though the red jungle fowl will live quite happily with a single hen, this is not universal and harems are often found. The bull of the American elk or wapiti, as my friend Dr. J. C. Phillips tells me, also forms, during the breeding season, a harem of cows from which he will drive away other bulls of his own kind, much as the fur seals do. Dr. Phillips further informs me that there are among the higher vertebrates many other instance of that particular form of polygamy in which one male during the breeding season naturally associates with many females. Such examples are found among some of the larger antelopes, wild sheep, and wild goats, and among certain birds such as the black grouse, capercaillie, and wild turkey. Although in these several species, the proportions of sexes at birth, so far as I am aware, are not definitely known, they probably follow the rule of approximate equality so common among many of the other higher animals and thus in reality illustrate much the same condition as that seen in the Alaskan fur seal.

Among the lower animals, particularly the insects, exceptional ratios in the sexes have long been known, the classic example of the honey bee being the most commonly quoted. Here a few

males, the drones, are set off against one perfect female, the queen, and a host of imperfect ones, the workers. These cases differ from those in the higher animals, however, in that the sex ratios appropriate for the breeding colony are determined from the beginning, *i. e.*, the young are not produced males and females in equal numbers. Such cases as the honey bee and other like insects exhibit, therefore, in their sex ratios much more accurate adjustments to their breeding requirements than do the higher animals; in fact they may be said to show a very high order of intracolonial sex adaptation.

Throughout the animal kingdom as a whole sexual reproduction seems to be best adjusted where the sexes are represented in approximately equal numbers and this relation is probably determined by the production of equal numbers of male-determining and female-determining sexual elements. The sperm cells of most species of animals, perhaps of all, are apparently the prime factors in this determination, and the dimorphism of these cells in the sense that one class is made up of male-determiners and the other of female-determiners as well as the production of these two classes in equal numbers may be looked upon as the chief adaptation of the animal kingdom so far as sex ratios are concerned. But the reproductive activities of a limited number of animals, such as the honey bee and the fur seal, have developed in directions in which equal numbers of the two sexes serve no longer as an advantageous combination. To meet these new conditions, further adaptation would be needed and, from what has been said, this adaptation would involve readjustments in the powers of the sex-determining reproductive cells. Such readjustments seem to have been carried out in the insects as seen in the honey bee, etc., where through the development of natural parthenogenesis the usual sex ratio has been entirely set aside and a new one favorable to the new requirements has been established. This has not been accomplished by the fur seals and other higher animals which in this respect remain poorly adapted to their new relations. From this standpoint, then, such lower animals as the insects show a higher order of adaptation than either the mammals or the birds. An explanation of this paradox may be found in the fact that the rate at which generation follows generation in in-

sects is enormous compared with that in the higher animals and further that geologically speaking the insects are much older than the mammals or birds. Hence they have had a much greater opportunity to adapt themselves to their conditions than has fallen to the lot of the higher animals. If the maladjustments of the sex ratios as exhibited by the fur seals and other higher animals are to be interpreted in the way indicated, it is clear that the evolutionary processes by which adaptation is brought about must often be slow and imperfect with the result that adaptation itself is better described, in the words of Bateson, as a poor fit than in the extravagant terms of eulogy with which many of the older writers clothed it.

Harvard University,
April 23, 1915.

THE LARGE FRUITED AMERICAN OAKS.

By WILLIAM TRELEASE, Sc.D., LL.D.

Plates I–III.

(Read April 23, 1915.)

When Alphonse de Candolle monographed the oaks of the world something over a generation ago,[1] he distinguished with a varietal name a form of our common white oak with small acorns some 8 × 14 mm., which Engelmann had sent him—the usual fruit of *Quercus alba* measuring about 14 × 18 mm. Those who have examined numerous specimens of our common red oak, *Q. rubra,* and its double, *Q. Schneckii,* have noted that they occur in forms varying in diameter of the acorn from about 10 to about 20 mm. The assemblage of forms clustering about the Californian *Q. chrysolepis,* the oldest of our existing types of oak, geologically, also show a comparable or even greater difference in the size of the fruit of what are otherwise held to be mere variants of a single species; and the polymorphic *Q. dumosa* presents a similar if less extended range of fruit size.[2]

The most surprising of our species in this respect is the bur oak, which joins to its great range in size a difference in fruit which is even more startling; for while the usual diameter of the acorns of this species is somewhere about 25 mm., and of the cup five or ten millimeters more, the acorns frequently measure 40 mm. in diameter with a cup fully 50 mm. across on the one hand, while on the other hand they may scarcely reach a diameter of 10 mm. Perhaps no oak presents so great a range of cup characters as this species does.[2] While the round or ovoid scaly-fringed form covering the acorn nearly or quite to its top is taken as the most typical and has given to the tree its common name of mossy or overcup oak, it is not un-

[1] *Quercus alba microcarpa* A. de Candolle, Prodromus, 16^2 22. 1864.

[2] On these consult Sargent, Silva, vol. 8.

common to find a broad saucer-shape assumed with the fringe of slender scales either seemingly absent, because closely inflexed beside the acorn, or extended in its development over a considerable part of the outside of the cup; and it is even possible, as Professor Pieters has recently shown me by material collected about Ann Arbor, for the cup of the smaller type of fruit to be shallow and thin as in the post oak, and either delicately ciliate at top or entirely without a fringe even on a single tree.

Even the largest acorns produced by these or our other familiar oaks seem small when compared with those of some tropical species of *Quercus,* or of the related genus *Pasania.* On our own continent, where the true oaks extend from the far north into the high Andes of Colombia, these large-fruited species are of both the red— and white-oak groups,—the former in Guatemala and Chiapas, and the latter in the last-named state of Mexico and along the flanks of the eastern Sierre Madre range above Vera Cruz. In contrast with these, which may reach a diameter of 50 or even 60 mm., the smallest acorns, also Guatemalan and Mexican, and of the group of red oaks, scarcely measure 5 mm. in diameter.[3]

Some two years ago, while looking over a series of type photographs that I had made in the course of a systematic revision of the oaks of tropical America, Mr. Walter Swingle, of the National Department of Agriculture, expressed considerable interest in some of the east-Mexican large-fruited white oaks as affording a hopeful field for experimentation both in direct propagation and hybridization, with reference to our own tropical and subtropical regions, and Mr. David Fairchild, of the same government department, considered the matter of sufficient interest to undertake importations through the interest of Dr. C. A. Purpus, whose collections in the southern republic have done much of recent years to make its vegetable wealth known. The purpose of the present communica-

[3] *Quercus parviglans* n. nom.—*Q. microcarpa* Liebmann, Overs. Dansk. Vidensk. Selsk. Forhandl. 1854: 184.—Liebmann-Oersted, Chênes Amer. Trop. 26. *pl. 6.*—Not *Q. microcarpa* Lapeyrouse, Hist. Abr. Pl. Pyren. 582. 1813. Equally small are the racemed acorns of an as yet unpublished group of west- and south-Mexican species of the red oaks; and the east-Mexican white oak, *Q. glabrescens,* possesses an equally small-fruited variety.

tion is to give a connected account of these large-fruited species, because of this popular interest that they possess.

The first of the large-fruited tropical American species to be made known is *Quercus Skinneri*[4] collected by Hartweg at Quezaltenango, Guatemala, which Bentham noted and illustrated in 1841 and described the following year. Skinner's oak is a large tree with long-petioled, ovate, acute, rather blunt-based aristately toothed glabrous leaves about 5 × 9 cm., producing solitary or paired short-stalked fruit resembling that of our common red oak but on a much larger scale, the shallow cup 50 mm. in diameter and the short-ovoid acorn of about that length. It is a red oak with the usual characters of apical abortive ovules and tomentose interior of the shell, but the latter is thicker than usual and with the septa intruded into the kernel so as to make the latter somewhat three-lobed. Of recent years *Q. Skinneri* has been collected only by Cook and Griggs, at the Finca Sepacuite, Guatemala. A similar, if not the same, red oak, but with larger duller winter buds and lance-elliptical leaves as much as 20 cm. long, was collected at Chinantla, in the Mexican State of Oaxaca, by Liebmann in the fall of 1842, but of it nothing else is known: as with *Furcræa longæva,* the species seems to range extensively through the Cordillera.

Closely related to Skinner's oak is a species recently collected by Dr. Purpus in the Mexican state of Chiapas, the similar acorns of which may reach a diameter of over 35 mm., their very shallow cups, with thickened scales, sometimes measuring 45 mm. across. This, which differs markedly from *Quercus Skinneri* in having acutely lanceolate very short-stalked leaves about 5 × 12 cm., may be called *Q. chiapasensis.*[5] Like the other oaks here under consideration, it appears to become a tree of very large size.

The year following the full description of *Quercus Skinneri,* the Belgian botanists Martens and Galeotti described under the name

[4] *Quercus Skinneri* Bentham, Gard. Chron. 1841: 16. *f.*; Pl. Hartweg. 90. 1842.—Hooker, Icones Plant. 5. *pl. 402.*—Liebmann-Oersted, *l. c. pl. B, 3.*

[5] *Quercus chiapasensis* n. sp. Arbor grandis: foliis brevipetiolatis, acutis, lanceolatis, aristato-dentatis; fructu magna; cupula plana, glabra, crassa; glande semiglobosa, 35 mm. diametro. *Q. Skinneri* affinis.

Q. insignis[6] what must be regarded as most notable in its genus because of the enormous height of the trees and the production of acorns occasionally fully 60 mm. in diameter and thus out of comparison with those of any black oaks and even with those of such Asiatic Pasanias as *P. cornea*. *Quercus insignis*, which occurs along the upper flanks of Mount Orizaba in eastern Mexico, is a white-oak, with the interior of the acorn shell not woolly, and with deeply lateral ovules. Its short-petioled elliptical-oblanceolate more or less acuminate leaves are sharply low-serrate but without bristle-like tips to the teeth, and measure 5 × 10 cm. or more. The fruit, which matures the first season, as seems to be true of all white oaks, is typically biscuit-shaped and about one-fourth shorter than thick, and the rather shallow cup, which may reach 80 mm. in diameter, is covered with coarse heavy loosely ascending scales.

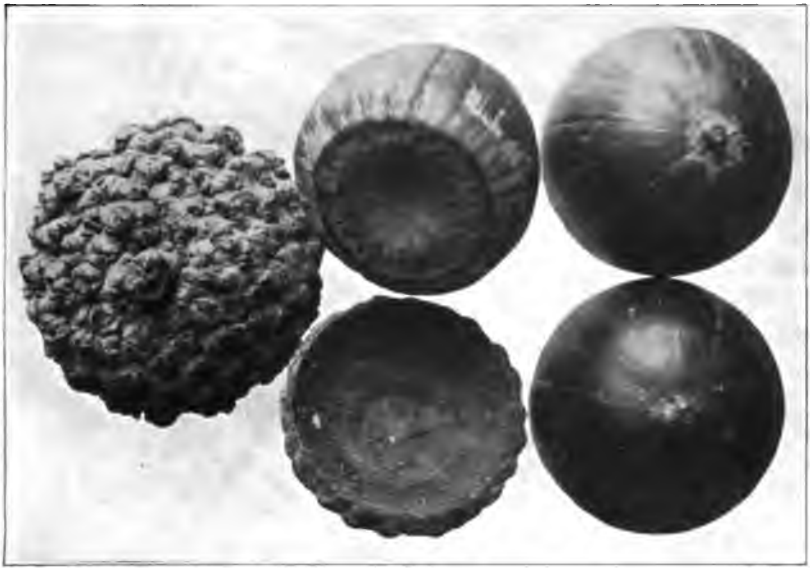
Martens and Galeotti do not appear to have seen more than one form in the oaks of this kind; but the type collection of Galeotti as represented in the museum at Budapest contains acorns of two sorts, one depressed, and the other acuminately conical and about as long as thick. At about the same time that Galeotti's collections were made, the Danish botanist Liebmann collected in the same general region materials of an oak similar to *Q. insignis* but differing in bearing subconical acorns about as long as broad and with more turbinate cups. This was published by its discoverer in 1854 under the name *Q. strombocarpa*.[7] Subsequently when a series of exquisite drawings of Mexican oaks prepared under Liebmann's direction were published by Oersted, the latter added a plate of a very similar form, which he called *Q. insignis strombocarpoides*.[8] It is hard to see how the latter can be distinguished from *Q. strombocarpa*, and the Galeotti collection shows that, different as the fruit extremes are, the discoverer of *Q. insignis* did not separate from its typical form the conical-fruited form to which apparently both of the later names refer.

[6] *Quercus insignis* Martens and Galeotti, Bull. Acad. Brux. 10[2]: 219. 1843. —Liebmann-Oersted, *l. c. pl. K, 29*.

[7] *Quercus strombocarpa* Liebmann, Overs. Dansk. Vidensk. Selsk. Forhandl. 1854: 176.—Liebmann-Oersted, *l. c.* 24. *pl. 27*.—Oersted, Bidrag Kundsk. Egefamilien. 346. *f. E*.

[8] *Q. insignis strombocarpoides* Liebmann-Oersted, *l. c. pl. 28*.

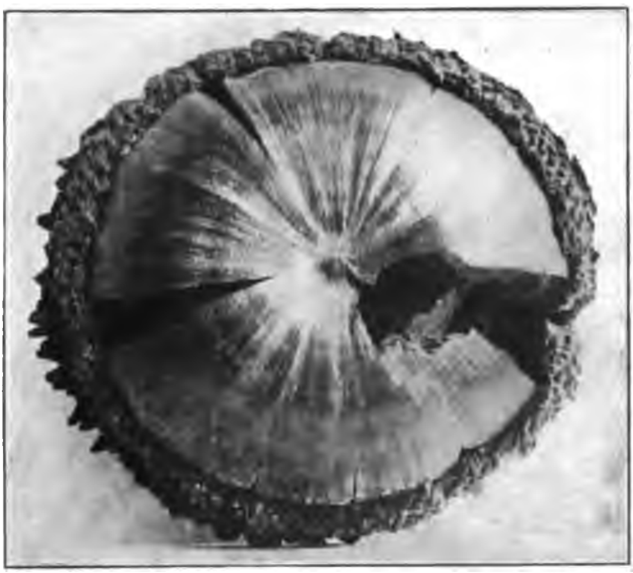
QUERCUS MACROCARPA.



QUERCUS CHIAPASENSIS.

PASANIA CORNEA.

QUERCUS CYCLOBALANOIDES.



QUERCUS INSIGNIS.

Wishing materials that should throw light on this question, I turned in 1912 to my amiable correspondent, Dr. Purpus, who was then in eastern Mexico: but before my letter reached him, Dr. Purpus had gone into southern Mexico. The result of my appeal, therefore, was not further specimens of *Quercus insignis,* but collections of an equally large and almost equally large-fruited white oak which appears to be characteristic of the Chiapas region. With somewhat similar but more deeply toothed leaves about 7 × 20 cm., equally short-petioled, this combines a stoutly stalked turbinate cup as much as 60 mm. in diameter, the scales of which are barely if at all free at tip with their bases connate in zones; and the ovoid pointed acorn measures 40–50 × 50–60 mm. Though closely related to *Q. insignis,* this species is so distinct in its collective characters as to stand as the type of a separable group of white oaks, and it has been called *Q. cyclobalanoides*[9] because of its very peculiar cup-markings.

EXPLANATION OF PLATES.

(All figures are of natural size.)

Plate I.

Quercus macrocarpa. Above, three acorns from a very small-fruited Michigan tree (*Pieters*), partly with and partly without fringe to the cup, and a single fruit of the largest and "mossiest" type from Illinois (*Adams*). Below, two of the more typical acorns of different cup-depth, and a cup showing a not infrequent inrolling of the inner scales, also from Illinois (*Adams*).

Plate II.

Above, two cups and three acorns of the large-fruited Mexican red oak, *Quercus chiapasensis* (*Purpus*). Below, basal and side view of two acorns of the large-fruited Chinese oak, *Pasania cornea* (after photographs by Fairchild).

Plate III.

Above, two fruits of the Mexican ring-scaled white oak, *Quercus cyclobalanoides* (Chiapas, *Purpus*). Below, a fruit of the great-acorned Mexican white oak, *Quercus insignis* (Huatusco, *Purpus*).

The University of Illinois,
March 8, 1915.

[9] *Quercus cyclobalanoides* n. sp. Arbor grandis; foliis brevipetiolatis, acutis, oblanceolatis, mucronato-dentatis: fructu magna; cupula turbinata, luteo-tomentosa, pluriannulata; glande ellipsoidea, sub 50 mm. diametro. *Quercus* Insignes valde affinis, sed sectio distincta, Cyclobalanoideæ, constituens.—*Q. insignis* Journal of Heredity, 5: 407. *f. 12.* 1914—not Martens and Galleotti, *l. c.*

THE SWEDES, GOVERNOR PRINTZ AND THE BEGINNING OF PENNSYLVANIA.

BY THOMAS WILLING BALCH.

(*Read March 5, 1915.*)

Of the original thirteen States, those south of the Middle States as well as those known under the collective name of New England, were settled by men and women of English race. New York, New Jersey and Delaware were first settled by Hollanders. The whole area of the Dutch settlements was known as New Netherland, and the chief city of the Hollanders in the new world was called Amsterdam in New Netherland, though historians afterwards thought fit to change the name into *New* Amsterdam,[1] doubtless because the English had renamed the town *New* York. The settlements in the valley of the Hudson and in what is now New Jersey passed by conquest into the hands of the English. The Dutch settlement in Delaware was destroyed after six months by the Indians. Subsequently, the Swedes took over the inchoate title of the Dutch to present-day Delaware. The Swedes later lost Delaware to the Dutch by conquest, who in their turn were afterwards conquered by the English.

No European Power, however, occupied and took possession of what today constitutes the Commonwealth of Pennsylvania, until Lieutenant-Colonel John Printz, who was the fourth Governor of New Sweden, moved up from Delaware to Great Tinicum Island and there established, in 1643, his seat of government, the first capital placed in the territory of the present State of Pennsylvania. He thereby became the first governor of the territory now known as Pennsylvania.

That Sweden was the first European nation to possess itself of what is present-day Pennsylvania was supported by the International

[1] I have to thank Mr. Robert H. Kelby, the learned librarian of the New York Historical Society, for this information.

Law of the seventeenth century. Towards the end of the sixteenth century there grew up as a rule of international law that, in order that a member of the family of nations could claim as its own a newly discovered and virgin land, it was necessary for that nation to actually occupy and possess that virgin land. The act of merely discovering and christening such an unoccupied land did not give the right of possession. The act of possession must be an actual occupancy through the establishment of forts and settlements in that land. Queen Elizabeth enunciated this principle clearly in 1580 in a notable answer she made at her court to the Spanish Ambassador, Mendoza.[2] It was thus recognized by England through the lips of her sovereign, a sovereign who well knew how to maintain the dignity and interests of her realm abroad. That rule became more and more recognized both by the publicists in their writings and by the nations in their acts, and has remained a rule of international law until the present day.

The sovereignty of Sweden over the land now known as Pennsylvania passed later by conquest to the States General of the United Netherlands, and subsequently again by conquest to the British crown, by whom it was afterwards granted to William Penn.

The fact that the sovereignty of Pennsylvania, alone of the original thirteen, goes back to Sweden for its beginning and that Printz was the first in the line of its governors, is known to only a very few. It would seem well then, that proper monuments to Printz and his Swedish settlement should be erected, so that future generations may know of the beginning of this province and state. And no place would seem more appropriate than the ancient hall of this venerable society of learning, the oldest existing society of learning not only within the bounds of Pennsylvania but also in all of the new world as well, to suggest that, first a bronze tablet should be erected in memory of Governor Printz and his capital called Nya Göteborg on Great Tinicum Island; and second, a bronze statue of Governor Printz, either of life or heroic size, should be placed at some conspicuous place in the city of Philadelphia.

[2] Camden's "Annals," 1580; see translation in Sir Travers Twiss's "Oregon Question."

GENERAL RESULTS OF THE WORK IN ATMOSPHERIC ELECTRICITY ABOARD THE CARNEGIE, 1909–1914.

By L. A. BAUER.

(Read April 24, 1915.)

Notable progress, it is believed, has been achieved by the department of terrestrial magnetism of the Carnegie Institution of Washington during the past year in the perfection of the instrumental appliances for observations in atmospheric electricity. In various articles by Drs. Swann and Hewlett, which have appeared in the *Journal of Terrestrial Magnetism and Atmospheric Electricity,* 1913–1914, new points of theory were brought out, serious errors in certain instruments were made known, and improved methods and instruments were devised. As a result considerable improvement has been made in the work in atmospheric electricity aboard the *Carnegie,* especially on her present cruise.

It is now deemed worth while to expand the work of the department in atmospheric electricity in two directions: (*a*) Continuous observations, by self-recording means, at the department's laboratory in Washington and at such observatories elsewhere as the department may be able to establish in the near future. (*b*) A general electric survey of the globe, implying observations at points distributed over the earth's surface, somewhat as in a magnetic survey.

Probably the late Professor Rowland was one of the first, in his address before the Congress of Electricians, held at Paris, September, 1881, to point out the need in atmospheric electricity "of a series of general and accurate experiments performed simultaneously on a portion of the earth's surface as extended as possible."[1] He says that "the principal aim of scientific investigation is to be able to understand more completely the laws of nature, and we generally succeed in doing this by bringing together observation and theory."

[1] Physical Papers of Henry A. Rowland, Baltimore, 1902, p. 212 et seq.

On Professor Rowland's motion the Congress resolved "that an international commission be charged with determining the precise methods of observation for atmospheric electricity, in order to generalize this study on the surface of the globe."

Unfortunately, in the past, the observations in atmospheric electricity have often been found to be counterfeits of nature because of the errors inherent in the instruments and methods used. Accordingly the much-desired discovery of nature's laws by "bringing together observation and theory" has not been effected in the measure desired. None of the proposals for a general electric survey of the earth which have been made repeatedly to learned academies, one of the last having been presented to the International Association of Academies, has been put into effect, doubtless because of the discouraging experiences encountered.

In spite of the vast work already done by notable investigators, we still have no generally accepted theory of the origin of atmospheric electricity.

Probably one of the most important of recent contributions to the observational data is the series of observations obtained on the past cruises of the *Galilee* and the *Carnegie.* A report giving the results up to the end of 1913, obtained by the department observers and others, was prepared by Dr. Hewlett and published in the September, 1914, issue of *Terrestrial Magnetism and Atmospheric Electricity.* The observations comprised, in addition to the usual meteorological measurements, those of the potential gradient, atmospheric conductivity and radioactive content of the atmosphere. Perhaps the most important result was a confirmation of the somewhat striking phenomenon, that while the conductivity over the ocean is, on the average, at least as great as over land, the radioactive content is much smaller. The values of the potential gradient obtained at sea were of the same order of magnitude as those on land.

Dr. Swann has just completed a report on the atmospheric-electric observations taken during the third cruise of the *Carnegie* while under the command of Mr. J. T. Ault, in 1914. The general course of the *Carnegie* during this cruise was as follows: Leaving Brooklyn on June 8, 1914, she arrived at Hammerfest on July 3.

Sailing again from Hammerfest on July 25 she entered the harbor of Reykjavik, Iceland, on August 24, having reached the latitude of 79° 52′ North, off the northwest coast of Spitzbergen. Leaving Reykjavik on September 15, the *Carnegie* arrived at Greenport, Long Island, on October 12, returning to Brooklyn on October 21, 1914.

The observations in 1914, comprised, in addition to the magnetic and meterological data, measurements of the potential-gradient, the conductivities for the positive and negative ions, and the radioactive content. Measurements of the ionic numbers were also made during the passage from Greenport, through Long Island Sound to New York. The whole of the observations, with the exception of a few measurements in Long Island Sound by Dr. Swann, were taken by Observer H. F. Johnston.

The average values of the potential-gradient, atmospheric conductivity, and radioactive content for the whole cruise were, respectively, 93 volts per meter, 2.52×10^{-4} E. S. U., and 23, the last number being expressed in Elster and Geitel units. The average value of the earth-air current for the whole cruise was 7.7×10^{-7} E. S. U. per sq. cm.

The atmospheric-electric elements were measured daily between the hours of 9 A. M. and 12 noon. The observations as far as they go indicate a general increase of the potential-gradient from summer to winter, which is in accord with land observations for the daily mean values. The conductivity also shows a general increase from the beginning of the cruise (June 8, 1914) to about the end of September, when a maximum occurs, after which the conductivity falls.

No marked variation of the atmospheric-electric elements with temperature or humidity was found. However, an indication is shown of a variation of the conductivity with latitude; a maximum for the latitudes involved occurring in the neighborhood of 50° North. These conclusions with regard to the variation of the elements with season, latitude, etc., must be looked upon as tentative owing to the small number of data involved.

The conductivity appears to have an especially low value in the neighborhood of the American coast. In Long Island Sound,

measurements were made of the ionic numbers n_+ and n_-, and the results indicate that the low values of the conductivity referred to above are to be attributed partly to a low value of the specific velocities of the ions (v_+ and v_-). The mean values of v_+ and v_- for observations on three days in Long Island Sound are respectively 0.77 and 0.83 cm./sec. per volt per cm. The average value of n_+ and n_- for observations taken on three days in Long Island Sound are 340 and 280 ions per c.c. respectively.

By making use of the value (23) given above for the radioactive content, and of the empirical relation obtained by Kurz, for the reduction of the Elster and Geitel unit to absolute value, it turns out that the average radioactive content for the whole cruise amounts to about 12 curies of radium emanation per cubic meter as against 80 curies per cubic meter which is about the average value found over land. The emanation content is thus too small to account for the conductivity observed over the sea, which conductivity is as great or greater than that measured over land.

A criticism of the ordinary method of drawing conclusions as to the nature of the radioactive products in the atmosphere, by comparing the decay curve with one obtained by a wire exposed in a closed vessel, is given in Dr. Swann's report. The activity curves are analyzed in the report mathematically, use being made of the theory of radioactive disintegration, and it is found that while some of the curves can be explained by radium emanation alone, others require the presence of a product of longer decay period than radium *A, B* or *C*. The possibility of this extra product being a product of thorium emanation, as is generally assumed to be the case on land, is discussed by Dr. Swann.

An attempt to calculate the actual amount of radium emanation in the air directly from the theory of the Elster and Geitel method, without assuming any empirical relation results in a much smaller value for the radium-emanation content than that given by the empirical relation unless it is assumed that the average specific velocities of the active carriers are much smaller than is generally supposed.

THE RIGHTS AND DUTIES OF NEUTRALIZED TERRITORY.

By CHARLEMAGNE TOWER.

(*Read April 23, 1915.*)

Although the growing importance of the United States and their extended influence as a world power have made the subject one of prime interest to them in many respects heretofore, there has probably never been a time when the principle of neutrality has had for us in America the same weighty consideration that it has under the existing circumstances in the world today.

Never, probably, have the rights and duties of neutrals been so carefully scrutinized by American public opinion, or so sensitively tested by the responsible authorities of our Government. And very justly so, because, with almost the whole of Europe inflamed before us in this great war, there is scarcely a day in which some serious question does not present itself in the maintenance of our public policy, some delicate situation which affects our national honor,—both in our character of neutrals and our relations with the belligerent powers, and in their dealings with us in return.

It may be of interest, therefore, to consider one or two of the underlying principles of the rights and duties of neutral nations; not the less so, perhaps, because of the fact that neutrality, in its present recognized form, is the most recent and most modern of the effective rules of international law.

Indeed, the nations of antiquity had not only no conception of what we call neutrality, but they had not even a name by which to convey our meaning of the term. The Romans alluded to those not engaged in the war as *medii, amici* or *pacati;* and their dealings with them were regulated, as far as we can judge, by the feeling that they were peaceful and friendly; at all events that they were not openly to be regarded as the enemy. And this appears to have been the view of their position throughout the Middle Ages. It

was only in the seventeenth century that the term *neutralis,*—meaning to the minds of the people of that day, *non hostis,*—was brought into general use by the publicists, and that since then the condition of the neutral has been established, somewhat artificially, as is considered by some writers,[1] under the process of which the term *neutral* has been extended to the flag of a nation which chooses to take no part in the war, to its ships, its commerce and its citizens.

From this point of view it has been declared that neutrality is "the continuation of a previously existing state." That is to say: Powers which go to war and become belligerents alter their condition,—whilst those which choose to be neutral remain as they were before. Consequently, in their case, their international rights are unchanged; and "neutral states and their citizens are free to do in time of war between other states what they were free to do in time of peace."[2]

But, under the rules of international law, the state of neutrality carries with it certain rights and obligations which do not exist when there is no war. It has been settled that neutral governments may regulate the furnishing of certain articles to belligerent cruisers that seek hospitality in their ports, though they are bound to prohibit the supply of certain other articles, as, for instance, arms and ammunition. They have the right to enforce the respect for the neutrality of their waters, though they must not allow their territory to be used for fitting out or equipping armed expeditions against any belligerent. So also, the commerce of neutral individuals is subject to certain restrictions, as, in the matter of contraband of war, which do not exist in time of peace.

But the theory of the law is that these are merely the changes in certain details produced by common consent of the nations,—by the condition of war; though the principle remains permanently fixed, that the rights of a neutral continue, uninterrupted, in time of war precisely as in time of peace,—his rights of trade and commerce, his rights of free intercourse with either belligerent, or with anyone else; and that every restriction upon these activities that

[1] Holland, *Fortnightly Review,* July, 1883.

[2] Lawrence, "International Law," par. 243.

are lawful in a state of general peace must be based upon a clear and unquestioned rule of international law; the burden of proof being upon him who seeks to enforce the restraint.

As a general statement, the obligations of all neutral states are the same, so also are their rights, as non-belligerents and non-participants in the war; they decide by their own motion to occupy a neutral position, aside from and between the belligerents, with all of whom they voluntarily remain at peace. This is called "perfect neutrality," and is accepted by all the powers. But there are two classes of neutrals into which the whole body of neutral nations is divided, whose relations to the war are different in this respect: that, one set of them abstain by their own free will from entering the war; whilst the others are restrained from taking part in the hostilities and are obliged to remain out of it by the conditions of their existence. This difference between them marks the difference between neutrality and neutralization; between neutral and neutralized territory. And it is to this latter that I beg leave for a moment to direct attention.

A neutralized state, then, is one which is and must remain neutral under all circumstances. Its independent existence rests upon that condition. It is a state which has been constituted by common consent of the great powers, which has received from the powers the right to subsist, provided that it take no part whatever in any conflict that may arise between its neighbors and shall have no right of its own to take up arms except to repel attack or to defend its territory. Thus a neutralized state is, in fact, allowed to exist because the operative forces of self-interest of its neighbors find sufficient benefit accruing to themselves,—as, for instance, that it forms an intervening space between themselves and their own powerful neighbors whose proximity threatens their peace,—to induce them to agree to its existence. There are neutralized states, under international law, and neutralized individuals; and this character may be extended also to seas and waterways, to buildings, ambulances and ships.

A distinguished authority (Professor Holland) defines the process of neutralization as "the bestowing by convention of a neutral character upon states, persons and things which might otherwise

bear a belligerent character." But, "so great a change in their legal position cannot be made without the consent of all the parties affected by it. It must be made as the result of international agreement, in order to be valid, and must be accepted by all the important states."[3]

Neutralized states, therefore, are those which, whilst remaining politically independent, have yielded up a part of their sovereignty as the price of their existence, and are dependent upon the powers to protect them,—though they do not belong to the councils of the great powers, nor have they the right to discuss questions of policy which may ultimately lead to the employment of force, except in defence of their own frontiers.

The two conspicuous examples of this kind are Switzerland and Belgium. The cases are similar; each forms with its intervening territory a barrier between the threatened conflicts of powerful neighbors. Switzerland, lying as it does, between Germany, Italy and France, is so situated that if the passage through its territory were open, the Austrians might proceed freely from the valley of the Danube to the Rhone and the Po, and menace the western boundary of France throughout its entire length; and, indeed, that is what happened during the French Revolution, when the neutrality of Switzerland was disregarded and her territory invaded by all the contending parties, whilst the French, Austrians and Russians used her soil for their hostilities against each other. Again, in 1813, the Austrian army passed through Switzerland and crossed the Rhine at three places, in its campaign against France.

A short time later, the perpetual neutrality of Switzerland was recognized by the Congress of Vienna, in 1815; but, upon the return of Napoleon from Elba, the Allies called upon the Swiss Confederation to join in the general coalition against France, in order to assist them in promoting the common welfare of Europe and prevent the reestablishment of the revolutionary authority in France. They declared that they knew the importance attached by Switzerland to the maintenance of the principle of her authority, and that they did not intend to violate that principle; but with the view of accelerating the time when it might be made permanent

[3] Lawrence, ubi supra, paragr. 245.

and advantageous, they called upon the Swiss to assume an attitude and to take such measures as might be in proportion to the extraordinary circumstances of the moment, without forming a rule in this respect for the future. That is to say, the allied forces claimed the right to pass through Switzerland, recognizing her neutrality but agreeing that if it were violated by them they should not regard their act as a rule in the future. In truth, her neutrality was violated during the war by the contending parties on both sides.

But, after the reëstablishment of the general peace in Europe, a declaration was finally made, at Paris, in 1815, which fixed the political status of the Swiss Confederation, and upon that foundation it has rested ever since. By that declaration, both France on the one side and the allies on the other, Great Britain, Austria, Prussia and Russia, formally recognized the perpetual neutrality of Switzerland and guaranteed the integrity and inviolability of her territory. They declared also that the neutrality of Switzerland, and her independence of all foreign influence, were conformable to the true interests of the policy of all Europe.

The situation of Belgium renders it in this respect similar geographically to that of Switzerland; for it is the barrier which lies interposed between Holland and Germany on the one side and France on the other, and by means of its territory the boundary lines of these great powers are separated from each other in such a manner as to remove the menace of irritation which is always present in Europe where the common frontier is marked by a single line. With this barrier maintained, also, both France and Germany are protected from immediate attack at several of the most vulnerable points in the territory of each; as has been made evident by the conflicts that have taken place between the rival powers on the continent for hundreds of years, which have made Flanders and the low countries the battleground of Europe.

The territory of the present kingdom of Belgium was incorporated with that of Holland, in 1815, by the Congress of Vienna, in order to form the kingdom of the Netherlands, and for the distinct purpose of placing a barrier between the territories of Germany and France. But, quarrels of a domestic character having

broken out in the low countries, Belgium separated itself from the kingdom of the Netherlands, in 1831, the outcome of which was that a treaty was made, on the 19th of April, 1839, establishing peace between Belgium, as an independent kingdom, and Holland; and, on the same date, in 1839, another treaty was entered into by Great Britain, Austria, France, Prussia and Russia with the king of the Netherlands, recognizing that the union between Holland and Belgium, in virtue of the Treaty of 1815, is dissolved, and that Belgium, which is to be composed of certain provinces specifically delimited and set forth, shall become an independent state.[4]

This, then, is the origin and constitution of the kingdom of Belgium as we know it today. The powers agreed that, within certain boundary lines, it should be allowed to exist as a separate kingdom. They went further than that, and agreed also, by Article VII. of that Treaty, that:

We have in this a well-defined example of neutralized territory, as we are considering it today. Belgium was granted all the privileges of independence, with the right to make her own laws, regulate her own domestic affairs and administer her own government; always provided, however, that she should maintain, in her foreign relations, the strictest neutrality toward all other states. And this, it is believed, she has faithfully performed.

But, it will be observed that, whilst Belgium is thus bound to the great powers as to her neutrality, there is no agreement for specific performance upon their part in this respect, beyond their ratification of the convention itself and their general undertaking to carry out all of its provisions, in which the powers themselves had not entire confidence. It was evidently not regarded by them as a sufficient safeguard in the event of war, for when Germany and France declared war upon each other, in 1870, there was such grave danger that both the independence and the neutrality of Belgium would be disregarded in the course of the conflict, that it was considered necessary to assure her safety by special agreement having regard to the circumstances of that time.

[4] Hertslet, "The Map of Europe by Treaty," II., p. 984.

"Belgium, within the limits specified, shall form an independent and perpetually neutral state. It shall be bound to observe such neutrality towards all other states."

Therefore, Great Britain entered into a separate treaty with Prussia, in August, 1870, by which it was agreed that:[5]

> "If during the hostilities the armies of France should violate the neutrality of Belgium, Great Britain would be prepared to coöperate with Prussia for the defence of the same in such manner as may be mutually agreed upon, employing for that purpose her naval and military forces to insure its observance, and to maintain, in conjunction with Prussia, the independence and neutrality of Belgium."

And Great Britain entered into a separate treaty with France, at the same time, making provision in the same terms for the coöperation with her for the defence of Belgium in case that Belgian territory should be invaded by the armies of Prussia. These separate treaties were made binding in each case upon the parties during the continuance of the War of 1870, and for twelve months after the ratification of the treaty of peace. Thus Belgium was protected against invasion or disturbance during the Franco-Prussian War; though since that time both her independence and her neutrality depend upon the old agreement between the five powers, made in 1839.

But, as an old French writer has well said: "With such neighbors there is always a chance for trouble." The unfortunate situation of Belgium leaves her always open to danger when her powerful neighbors begin to fight over her head. She has her defence in the old agreement of the powers, it is true. But will that be a sufficient defence when either or all of the powers, engaged in a desperate conflict amongst themselves, find that their own self-interest, then of prime importance to each of them, places the consideration of Belgium in the background? Evidently not; and in this respect all the powers appear to be alike.

For instance, Sir Edward Grey in his great speech in Parliament, on the 3d of August, 1914, whilst advocating the neutrality of Belgium in the present war, pointed to the *interests* of Great Britain as the determining factor in the observance of the guarantee entered into by the powers, in 1839.[6] He quoted to the House the speech which Mr. Gladstone had made in Parliament, upon the same subject, in 1870, when he said, in regard to Belgian neutrality:

[5] Hertslet, "Map of Europe," III., p. 1886.

[6] *The Times*, London, August 4, 1914.

"There is, I admit, an obligation of the treaty. It is not necessary, nor would time permit me to enter into the complicated question of the nature of the obligation under that treaty. But I am not able to subscribe to the doctrine of those who have held in this House what plainly amounts to the assertion that the simple fact of the existence of a guarantee is binding on every party to-day irrespectively altogether of the particular position in which it may find itself at the time when the occasion for acting on the guarantee arises. The great authorities upon foreign policy to whom I have been accustomed to listen, such as Lord Aberdeen and Lord Palmerston, never to my knowledge took that rigid, and if I may venture to say so, that impracticable view of the guarantee. The circumstance that there is already an existing guarantee in force is, of necessity, an important fact, and a weighty element in the case to which we are bound to give full and ample consideration."

Sir Edward Grey added to this his own statement, that:

"The treaty is an old treaty—1839. It is one of those treaties which are founded not only on consideration for Belgium which benefits under the treaty, but in the interests of those who guarantee the neutrality of Belgium."

Unfortunately this is true. That treaty is evidently an obligation of convenience. Germany, upon her side, took the same view. The German Chancellor in his speech before the German Parliament alluded in this connection to "the wrong which we were doing in marching through Belgium." The German government declared that "it had in view no act of hostility against Belgium." It expected the Belgians to maintain an attitude of friendly neutrality toward Germany,—in return for which it undertook, at the conclusion of peace, to guarantee the independence of the Belgian kingdom in full. The Chancellor hoped that the Belgian authorities would yield to the inevitable and "retire to Antwerp under protest."

I do not intend to pursue this inquiry in the direction in which it has given rise to the controversy on both sides, and possibly the world over, as to whether the Allies were ready to pass through Belgium if the Germans had not done so. We are concerned merely with the law. Of course, if Belgium had taken the slightest step toward uniting her forces with either of the belligerents as against the others, she would have forfeited her attitude of neutrality and become herself a belligerent, subject to be treated as an enemy. And this would be the end of her independent existence; for that is based upon the neutrality which the convenience of the great powers has determined upon as the condition precedent of her national life.

But, assuming that she committed no breach of neutrality,—what rights has Belgium or Switzerland or any other neutralized territory? It has the right to defend itself, as Belgium has done. She is not obliged to defend herself, but may choose whether she will do so or not. For, if she yield to superior force, that can not be looked upon as an un-neutral act; though it may place her during the war upon the side of one of the belligerents, as is the case of Belgium today in consequence of her defence. Still, Belgium had undoubtedly the right to defend her soil. The law is on her side in that regard.

But, on the other hand, what protection has she? Evidently nothing but the agreement under which she lives,—and that depends either upon the "interests" of the powers who made the agreement, as Sir Edward Grey said, or upon the convenience of respecting it, as the advance of the German army has proved.

In the heat of a savage conflict, the reasons for the agreement are destroyed and the agreement itself is torn to shreds; for there is no one to enforce it. The only force that exists is being exhausted in the war. The neutralized territory has rights that are not only recognized but also defined by international law. It has its guarantees as well,—equally recognized and defined, though, as in the present case, the authority of the law is gone, and how shall a method be found by which to guarantee the guarantees?

PHILADELPHIA,
April, 1915.

THE PRONOUNS AND VERBS OF SUMERIAN.

By J. DYNELEY PRINCE.

(Read April 23, 1915.)

The pronouns of a language are relics of its earliest demonstratives. The first desire of the primitive speaker must have been to indicate objects. So soon as nouns had evolved themselves in his mind, the next step was the development of an abbreviated form which could indicate substantives without repeating the noun itself, and these abbreviations or indicators were nothing more than pronouns. It is possible that there existed originally in primitive speech only a single impersonal element of this character, which was at first used, supplemented by gestures, to indicate objects of all three persons. Subsequently, the same syllable may have been tonally differentiated to indicate the 'I, thou, that' idea and still later, additional syllables were called into play to aid in differentiating the first, second and third persons. It is interesting to observe that in the very evidently extremely primitive system of Sumerian pronouns, all the personal particles contain the common demonstrative element *e*, which appears most prominently in the third personal *ene*.

The object of the present paper is to present in a concise form the results of grammatical investigations regarding the Sumerian pronominal particles and also to weigh these theories and conclusions from a philological point of view, especially in connection with the incorporation of the pronominal elements in the verbal structure. It is interesting to note that the distinction between the nearer and farther subject-object, herein noted in connection respectively with the *b* and *n* particles, is a most natural linguistic phenomenon which would have followed almost arbitrarily the evolution of the general demonstrative idea.

The material used in this treatise has been taken partly from the

new vocabularies published in Arno Poebel's "Grammatical Texts,"[1] with the main conclusions of which the present writer is forced to disagree, as the material offered by Delitzsch, Langdon and Prince seems to disprove Poebel's chief thesis of the hidden vowel of the first person.

I.

Separable Pronouns.

Mà-e, 'I,' according to Delitzsch, § 28 = *ma* + demonstrative *-e*. Langdon, p. 102, thinks that *mà-e* was pronounced *mö,* as he regards *a-e* as a diphthong, indicating an *Umlaut.* This is possible, especially as the writing *me-a* = *anaku,* 'I,' also occurs. The pronunciation was more likely *mǒ* than *mö.* The form *mà-e* was invariably used for the *status rectus;* note that in such cases as IV. R. 17, 40–41; *mà-e mu-un-ši-in-gi-en* = *iaši išpuranni,* 'he has sent me,' the *mà-e* is really a *status rectus* in prolepsis and not an accusative, which would be regularly represented by the oblique *ma* (see just below). It is interesting to notice that Delitzsch gives *me-e* instead of *mà-e* as the usual form, which is again an indication that *mà-e* was not pronounced in two syllables, but really indicated an *Umlaut.* Delitzsch is, therefore, probably right in supposing that the writing *mà-e* really indicated original *ma,* the element of the first person, + the indicative *e.* All authorities are agreed that *a-e* may represent *e* or *ö* (cf. Delitzsch, § 18b).

The oblique form of *mà-e* is generally *ma,* as Poebel: gen. *ma-a-(k)*; cf. *ma-a-kam,* 'it is mine,' Poebel, p. 43; *ma-a-ge-eš ǧe-ti* = *aššumia liblut,* 'for my sake may he live.' The Dative is regular: *ma-a-ra, ma-ra, ma-a-ar* (*passim*). In the locative, Poebel finds

[1] The following abbreviations have been used: AJSL: "American Journal of Semitic Languages"; ASKT. = Paul Haupt, "*Akkadische und Sumerische Keilschrifttexte*"; Br. = R. Brünnow, "Classified List of Cuneiform Ideograms," Leyden, 1887; Del. = Delitzsch: Friedrich Delitzsch, "*Sumerische Grammatik,*" Leipzig, 1914; EK. = *Eme-ku* dialect; ES. = *Eme-sal* dialect; HT. = ASKT.; JRAS. XVII. = "Journal of the Royal Asiatic Society," quoted Poebel, pp. 63 ff; Langdon = Stephen Henry Langdon, "Sumerian Grammar," Paris, 1911; MSL. = J. D. Prince, "Materials for a Sumerian Lexicon," Leipzig, 1908; P. = Poebel: Arno Poebel, "Grammatical Texts," Philadelphia, 1914; P. AO. 5403: quoted, Poebel, pp. 62–63; P. 142: quoted, Poebel, pp. 57 ff; PSBA. = "Proceedings of the Society of Biblical Archaeology"; Sfg. = Paul Haupt, "*Sumerische Familiengesetze.*"

ma-a, 'on me' (not in Delitzsch). The regular accusative is also *ma-a*.

Poebel (p. 42) gives *mu-me-en* as the full separate form of *ma-e*, which clearly contains the first personal element *m(u)* + *me-en* of the verb 'to be' = 'it is I who am' (cf. *s.v. me-ne*, 'we').

The regular suffix of the first person is *-mu*, not to be confused with the third personal *-mu* referred to below. It is now practically established that the first and second persons suffered a change of vowel in the oblique relation, and that the *-mu* in such cases became *-ma;* as *e-ma*, 'in my temple'; *uru-ma*, 'in my city'; *lugal-ma*, 'for my king,' etc. The difficulty in establishing any definite rule in this connection lies in the fact that both *mu* and *ma* appear indiscriminately for both *status rectus* and oblique (see both Langdon and Delitzsch for numerous examples). The probability is that the original usage of the earlier language was *mu* for *rectus* and *ma* for oblique, but, even in the early documents, we find the confusion of forms so evident, as to make it impossible to come to a definite conclusion. The former theory that *-ma* was the ES. form for EK. *mu* is undoubtedly incorrect. On *-ni* = 1 p. suffix, cf. below, *s.v. e-ne*, 'he, she, it.'

Za-e, 'thou,' according to Delitzsch, § 29 = *za* + demonstrative *e*, as in the case of *mà-e*, 'I.' Similarly Langdon, p. 102, thinks that *za-e* represented *zö*, but this, like *mà-e* = *mö*, was probably pronounced *zȯ*. (*ȯ* = *a-e*). *Za-e*, like *mà-e*, was the invariable form of the *status rectus*. In such phrases as *kâtu amâtka* = *za-e e-nim -zu*, 'thy word,' *kâtu* is really the separable pronoun in nominative apposition. Cf. the remarks above on *mà-e* = *i̯aši*. Note that the second personal pronoun is also given as *ze*, in *ze-me*, 'thou art' (*passim*) and occasionally *zi-me*, Br., 3387.

The oblique of *za-e* is generally *za;* note Poebel: gen. *za-a(k)*, *za-a-a(k)*; *za-a-ge;* dat. *za-ra*, *za-ar;* *za-a-šu* (KU), 'unto thee' and pure locative *za-a*, 'on thee' (Poebel), a case not in Delitzsch. The oblique *za* is always used with the postposition as *za-da*, 'with thee, from thee,' etc.

Poebel gives also the separable *ze-me-en*, corresponding to *mu-me-en*, 'I' (see, however, *s.v. za-e-me-en*, *s.v.* the second person plural).

The regular suffix of the second person is *-zu,* with usually oblique *-za,* as in the case of *-me, -ma* (see just above). But here also *-zu* is found as both *rectus* and oblique, although *-za* seems to have been the original oblique form. Cf. *mà-e eri-za,* 'I am thy servant' (*-za* for *-zu,* L., § 158); *ga-zu-ta,* 'at thy command' (probably should be *ga-za-ta,* etc.). It is not possible to predicate a regular usage for *-zu; -za.*

E-ne, 'he, she, it'; according to Delitzsch, § 30 = demonstrative *e* + demonstrative *ne* = *nê,* 'this.' This is clearly the same *ne,* seen in the plural of nouns and verbs. Langdon (p. 107) thinks that *e-ne* = a reduplicated *ni* with apocope of the first *n; i. e.,* a sort of plural form. This idea has little foundation, as the demonstrative *e*-element is well established in other forms (as, for ex., *mà-e, za-e, lugal-e,* the king, etc.) Poebel gives no separate form for *e-ne,* the probability being that *e-ne* itself served as such. There is no distinct oblique form of *e-ne* which is declined like a noun: gen. *e-ne-ge* (KIT); dat. *e-ne-ra, e-ne-ir;* loc. *e-ne-a,* 'upon him' (Poebel).

The suffix of the third person has a twofold aspect; viz., 1) -(*a*)-*ni* and *-ni,* the former being rarer in occurrence than the latter; the oblique of this form is *-na;* and 2) -(*a*)-*bi* and *-bi,* the former being rarer than the latter; the oblique form of this is *-ba* (Delitzsch, § 37). The same confusion of usage is seen here as that between *-mu, -ma* and *-zu, -za,* fully pointed out by Delitzsch, § 38; *ki-ba,* 'in its place'; *šu-na,* 'into his hand,' regularly oblique, but *a-na* = *abušu,* 'his father' (for *a-ni*) and *dam šà-ga-a-ni,* 'the man of his heart,' instead of *-a-na,* etc. As to the meaning of the *-n-* and *-b-* suffixes, Langdon (p. 105) believed that *-ni, -na* as both noun suffixes and verbal elements, originally denoted animate beings, while *-bi, -ba* indicated inanimates, but the logical continuance of this theory is not borne out by the facts. We may note that in one of Langdon's own examples *bi-e-nad-di-en,* 'he slumbers,' *bi-,* here as verbal prefix, represents an *animate* subject (cf. my review, AJSL. XXVIII. p. 73). Note also HT., p. 76, 1 and 9: *su-mu-ug-ga-ni* and *su-mu-ug-ga-bi,* 'his suffering,' in both cases animate. Delitzsch, § 40, also gives many examples. The suffix *-ni* is used for the first person in Br., 5334: *i-de tum-a-ni* = *ublim pâniya; ud tur-ra-ni-ta* = *ultu ûm cixriku,* 'from the days of my youth'; *lal-a-ni* = *candaku,* 'I am

yoked.' The only possible explanation is that the translator deliberately transferred the persons. The possibility that the *-n-* and *-b-* elements were originally used to denote the remote and nearer subject or object respectively, has already been pointed out by Thureau-Dangin, ZA. XX., pp. 380–404, and fully discussed by Poebel (ZA. XXI., pp. 218–230; Prince, AJSL., pp. 364–365). This theory, although not yet capable of entirely satisfactory demonstration, lends itself more readily to credence than the animate-inanimate idea. In the later language, which represents a period of grammatical decay, the *n* and *b*-suffixes appear to be used arbitrarily. It is probable, however, that in the earlier phases of Sumerian, these endings must have had the force of remote and nearer demonstratives respectively.

Me-ne, me-en-ne, 'we.' Poebel gives *me-en-de, me-de, me-en-de-en,* which, however, should be read *me-en-ne, me-ne, me-en-ne-en.* He uses the *d*-element, because he finds the oblique form *me-en-da-na,* 'without us' (p. 47) and also *nam-da-me-en-da-na,* 'without us'; viz., *nam* negative + prep. *-da* + first person plural *me-en* + prep. *da* repeated + *-na,* probably negative, repeated. Poebel's own form *me-da-nu* (p. 34, line 34), 'without us' clearly shows that the *me-en* in these *me-en-da*-forms is the *me-en* of the first person. Thus, *me-en-da-nu* = *men* first person + prep. *da* + negative *nu.* A form *me-en-de* eliminates the evident combination of *me* = first person + plural *-ne.* Similarly, Poebel's separate forms *me-de-en-de* and *me-de-en-de-en* must be read *me-ne-en-ne* and *me-ne-en-ne-en,* respectively; *me-en-ne* = 'we' + *en,* element of the verb to be; lit. 'it is we who are' (cf. *mu-me-en, s.v. mà-e* above).

According to Delitzsch, *-me-ne,* etc. = *ma* + *ene,* 'I and he,' a sort of exclusive 'we.' But if this were the case, we should expect to find also an inclusive 'we' = 'I and thou,' which would have the form *me-en-zi-en* (or *me-ze*), but this form actually occurs with the equation *attunu* 'you,' plural (just below). It is much more likely to suppose that *me-en-ne, me-en-ne-en* represent a pure plural of the first personal *mê(n)*; *i. e., mê(n)* + *ne* or *ne-ne* + the verbal *-en,* when the form ends in *-n.* The pluralizing of the first person singular occurs for example in Central American Tule *an-mala* = *an* 'I' + the collective *-mala.* Indeed, the form *men-men* is actually

a reduplication of the first personal singular = *me-* + verbal *-n*. We find the reduplicated suffix *-mu-mu* 'our' (see below), which confirms this view.

Me-en-ne declines regularly, although no genitive has been found as yet; probably = *me-en-ne-ge* (KIT); dat. *me-en-ne-ra, me-en-ne-ir;* loc. *me-en-ne-a,* these last two cases being given by Poebel.

The suffix of the first person plural appears as 1) *-mu-mu,* Langdon, p. 109, n. 1, although this is rare; 2) Clay, Miscellaneous Tablets, has found: *dumu-mu-meš* 'our child,' a direct plural of *-mu;* 3) as *-men* : *en-men* 'our lord,' Langdon, p. 103 (Delitzsch, § 33, gives *-men* as frequent in this sense); 4) the common suffix is *-me: ad-da-me* 'our father'; *ama-me* 'our mother'; *ki-me-ta* = *ittini* 'with us,' etc. The curious form *ki-me-ne-ne* = *ittišunu* 'with them,' Delitzsch, § 43, probably was wrongly translated and means 'with us'; *i. e., ki* 'with' + *me-ne-ne,* a pluralized form of the usual *-me.* There seems to be no distinction in these suffixes between *rectus* and oblique. This is clearly indicated by the series of suffixes *an-ne-en, en-ne-en, in-ne-en, me-en-ne-en, un-ne-en,* all which are used for the first person plural (MSL., p. xxii, § 5) and are not honorifics as I thought (AJSL., XXVIII., p. 73). These are merely plural first personal suffixes with possible connecters (cf. just below *s.v. me-en-zi-en*). The *-nen* element which appears in all of them must represent *-me-n.*

Me-en-zi-en = *attûnu* 'you' (given by all sources) and also *zi-ne* 'you,' a real plural of the second personal element *zi* (*ze* = *za-e*), Langdon, p. 104. Note the parallel *me-ne* 'we.' In view of the fact than *za-e-me-en* also = *attûnu,* IV. R. 21, 1 B. rev. 3, clearly = *za-e* + *men* = 'thou and I,' it is probable that *me-en-zi-en* also = 'I and thou' (*me,* 'I' + verbal *-en* + *zi*(*ze*), 'thou + verbal *-en*). But this *za-e-me-en* is equivalent to Poebel's full form of *za-e* (see above *s.v. za-e*). It is impossible that *za-e-me-en* could have been a second personal singular separate form and at the same time a second person plural! If it were really used in both senses there must have been a different tone for each usage of *men* = respectively the verb 'to be' and the first person. Note that the odd reading NI-*e-me-en,* HT. 139, § 7, clearly = *za*(*l*)*-e-me-en.*

Of *me-en-zi-en* no genitive has been discovered, but it probably

was *me-ne-zi-en-ge* (KIT); dat. *me-en-zi-en-ra* and *za-ra-an-zi-en* (!); loc. *za-a-an-zi-en.* In these two latter forms, we have a reduplication of the second person; *i. e.*, *za* + dat. *-ra* + verbal (*a*)*n* + the second personal *zi* with verbal *en* = *zaranzen* and *za-a* loc. + (*a*)*n* = second person + *zi* with verbal *en* = *zânzen.*

The suffix of the second plural is *-zu-ne,* as *mu-lu-zu-ne,* 'your lord,' Langdon, p. 104. Note that in Delitzsch, "*Sumerisch-Akkadisch-Hettitische Vokabularfragmente,*" p. 19, the form *á-zu-šú-ne-a-áš* = *ana ittikunu,* 'for your wage' = the suffix *-zu-ne,* with infixed preposition *šú* (KU) + directive *a-áš,* an unusual and interesting example of infixation. The suffix *zu-ne-ne* often occurs, Delitzsch, § 42: *u-gu-zu-ne-ne* = *elikunu,* 'upon you'; *nam-en-un-un-zu-ne-ne* = *macartikunu,* etc. Here we have plainly the pure plural of the second personal element and no indication of 'thou and I.'

As in the case of the first person plural, there seems to be no distinction between *rectus* and oblique. This is indicated by the series of suffixes similar to those just cited in connection with the first person plural; *-ab-ci-en, -an-ci-en, -en-ci-en, -ib-ci-en, -íb-ci-en, in-ci-en, -me-ci-en, -me-en-ci-en* and *-un-ci-en.* The forms *-me-ci-en* and *-me-en-ci-en* may contain an 'I and thou' element. These all represent the second personal suffix with possible connecters.

E-ne-ne-ne, 'they'; according to Delitzsch, § 32; *ene* + *enene,* 'he and they,' but this form is more likely to be *ene,* 'he, she, it' + the reduplicated plural element *-ne,* as in the case of *me-ne* and *-zu-ne-ne,* cited above. The short form *e-ne-ne* is also common. Poebel gives *e-ne-ne-ne* as the full separate form, but without sufficient foundation, as either *e-ne-ne* or *e-ne-ne-ne* might have served in this capacity, as in the case of the singular *e-ne.*

The third person declines regularly; gen. *e-ne-ne-ge* (KIT); dat. *e-ne-ne-ra, e-ne-ne-ir;* loc. *e-ne-ne-a.*

The third plural suffix, as in the case of the third person singular, is twofold; 1) (*a*)*-ne-ne,* the *a* not being always present, in fact it is usually part of the prolonged root, as *dug-ga-nene.* It appears regularly *šu-ne-ne,* 'their hand'; *gir-ne-ne,* 'their foot; *ki-ne-ne-ta* = *ittišunu,* 'with them' (on *ki-me-ne-ne* = *ittišunu,* Delitzsch, § 43, see above *s.v. me-en-ne*). 2) The endings with the *b*-element: *bi-e-ne-ne, bi-e-ne,* Delitzsch, § 43; *be-ne-ne,* Langdon, p. 108, and

be-ne, Langdon, p. 108, are also common: *sib-bi-ne,* 'their shepherd'; *muǧ-bi-ne-ne* = *elišunu* (Langdon, p. 108). The distinction between remote and nearer subject and object, respectively *-ne-ne* and *-bi-ne,* is no more logically carried out in the later language than in the case of *-ni, -bi* of the third person singular (*q.v.*), but their original remote and nearer force seems just as probable.

The third person plural possessive is frequently expressed by the singular suffixes of the third person: *-ni, -bi,* a phenomenon which

TABLE OF PRONOUNS.

	I	*Thou*	*He, She, It*
Nom.	*ma-e* Separate: *mu-me-en*	*za-e* Separate: *za-e-me-en*	*e-ne*
Gen.	*ma-a-(k)* *ma-a-ge*	*za-a(k)* *za-a-a(k)* *za-a-ge*	*e-ne-ge*
Dat.	*ma-a-ar* *ma-ra* *ma-a-ra*	*za-ra* *za-ar*	*e-ne-ra*
Loc.	*ma-a*	*za-a* (*za-a-šù*, 'to thee')	*e-ne-a*
	SUFFIXES	SUFFIXES	SUFFIXES
Rectus[2]	*-mu*	*-zu*[2]	(*a*)*-ni, -ni*[4] (*a*)*-na, -na*
Oblique	*-ma* (*-ni* very rare and probably an error)	*-za*	(*a*)*-bi, -bi*[4] (*a*)*-ba, -ba* *-mu*[5]

	We	*You*	*They*
Nom.	*me-ne, me-en-ne, me-en-ne-en*	*me-en-zi-en*	*e-ne-ne* *e-ne-ne-ne*
Gen.	[1]*me-en-ne-ge*	[1]*me-en-zi-en-ge*	*e-ne-ne-ge*
Dat.	*me-en-ne-ra* *me-en-ne-ir*	*me-en-zi-en-ra* *za-ra-an-zi-en*	*e-ne-ne-ra* *e-ne-ne-ir*
Loc.	*me-en-ne-a*	*za-a-an-zi-en*	*e-ne-ne-a*
	SUFFIXES[3]	SUFFIXES[3]	SUFFIXES[3]
	-me-en *-me* *-mu-mu* } rare *-mu-meš* }	*-zu-ne* *-zu-ne-ne*	*-*(*a*)*-ne-ne, -ne-ne* *-be-e-ne-ne*[3] *-be-ne-ne,* etc.

CONNECTING SUFFIXES
an-ne-en, en-ne-en,
in-ne-en, me-en-ne-en,
un-ne-en

CONNECTING SUFFIXES
ab-ci-en, an-ci-en,
en-ci-en, ib-ci-en,
ib-ci-en, in-ci-en,
me-ci-en, me-en-ci-en,
un-ci-en

[2] Confused usage.

[3] No distinction between *rectus* and oblique.

[4] Probable distinction between nearer and farther subject and object.

[5] Used only with participles, so far as is known. See below.

is seen in other languages, as, for example, in Central American Tule, where *a'ti, i'ti* can indicate both 'he, she, it' and also 'they' (Prince, Amer. Anthropologist, XV., p. 484; the *a'ti, i'ti* -element may be pluralized by the collective suffix *-mala,* which, however, is often omitted).

II.

SUMERIAN VERB WITH PRONOUNS, WITH REFERENCES TO FOLLOWING COMMENTARY.

I-THEE

GA-CLASS: *ga-mu-ra-ab-dím* = *lu-bu-ša-ku-um,* 'I will (let me) make for thee,' P., No. 142, rev. 2, 10.

MU-CLASS: *nu-mu-ra-te-mà-dè(ne)-en* = *u-la e-ṭe-xi-a-kum,* 'I shall not go to thee,' P. AO. 5403, 6. *xu-mu-ra-ab-gă(r),* 'I gave thee as a present,' P., p. 103.

MI-CLASS: *mi-ni-max-en,* 'I made thee great therein (for it),' P., p. 112.

I-HIM

GA-CLASS: *ga-an-na-dím* = *lu-bu-su-um,* 'I will make it for him,' P., No. 142, rev. 2, 15.

MU-CLASS: *xu-mu-na-du,* 'I built for him,' P., p. 102. *mu-na-du,* 'I built for him,' P., p. 102. *xu-mu-ni-max* = *lu-ša-ti-ir,* 'I will increase for him' (or) 'therein,' P., p. 102.

MI-CLASS: *mi-ni-í* = *a-na-ku šu-a-ti šu-a-ti,* 'I it for him,' P. JRAS., XVII., 65, 4, 23. *mi-ni-du* = *al-bi-in,* 'I moulded it for him' (or) 'therein,' P., p. 102. *xu-mi-ni-in-tax* = *lu-um-mi-su,* 'I supported it,' P., p. 102.

NE-CLASS: *ne-gí-a,* 'I restored it (Clay).'

BA-CLASS: *ba-a* = *a-na-ku šu-a-ti*(+ *-ti;* probably = *šuati šuati*), P. JRAS., XVII., p. 65, 4, 19. *ba-ni-í* = *a-na-ku šua-ti šu-a-ti,* 'I it for him,' P. JRAS., XVII., p. 65, 4, 28 (also *ba-ni-e* ditto). *ba-an-na-te-en e-te-xi-šum,* 'I went to him,' P. AO., 5403, 8.

BI-CLASS: *bí* = *a-na-ku šu-a-ti,* 'I it (or) him,' P. JRAS., XVII., p. 65, 4, 13. *bi-i* = *a-na-ku šu-a-ti,* 'I it (or) him,' P. JRAS., XVII., p. 65, 4, 14.

I(N)-CLASS: *i-ni-í* = *a-na-ku šu-a-ti šu-a-ti,* 'I it for him,' P. JRAS., XVII., p. 65, 4, 22. *in-na-ni-í* = *a-na-ku šu-a-ti šua-ti u*

a-na-ku šu-a-šum, 'I it for him and I to him it,' P. JRAS., XVII., p. 65, 4, 30.

I-HIM

sag-túm-ma i-ni-in-ga = *ma-gi-ir-tam ak-bi-šum,* 'I spoke favorably to him,' P. No. 142, rev. 3, 21. *in-na-te-en* = *e-iṭ-xi-šum,* 'I have gone to him,' P. AO. 5403, 2.

IM-CLASS: *ù-gul im-ma-an-mà-mà,* 'I asked him,' P. p. 102.

THOU-ME

MA-CLASS: *nam-ma-te-má-dé (ne)-en* = *la ta-ṭe-xi-a-am,* 'do not come to me,' P. AO. 5403, 5.

MU-CLASS: *nam-mu-un-xa-xa-en* = *la tu-te-bi-da-(an-ni),* 'mayst thou not be lost to me,' P. No. 142, rev. 3, 8.

THOU-HIM

POSTPOSITIVE CLASS: *dím-(ma)-na-ab* = *e-bu-su-um,* 'make for him,' P. No. 142, rev. 2, 14. *gur-an-ši-ib,* 'turn to him,' P. No. 142, rev. 2, 16. *te-a-na* = *ṭe-xi-šum,* 'go to him,' P. AO. 5403, 1. *na-an-na-te-mà-dè(ne)-en* = *(la) te-ṭe-(xi)-šu-um,* 'do not go to him,' P. AO. 5403, 4.

MI-CLASS: *mi-ni-e* = *at-ta šu-a-ti- šu-a-ti,* 'thou it it,' P. JRAS. XVII. p. 65, 4, 25.

BA-CLASS: *ba-e* = *at-ta-ku* (*sic* !) *šu-a-ti,* 'thou it,' P. JRAS. XVII. p. 65, 4, 20.

BI-CLASS: *bi-*NE = *at-ta šu-a-ti,* 'thou it,' P. JRAS. XVII. p. 65, 15. *bi-e* = *at-ta šu-a-ti,* 'thou it,' P. JRAS. XVII. p. 65, 16.

I(N)-CLASS: *i-ni-e* = *at-ta šu-a-ti šu-a-ti,* 'thou it it' or 'it for him,' P. JRAS. XVII. p. 65, 4, 24. *in-na-ni-e* = *at-ta šua-ti šu-a-ti,* 'thou it for him,' P. JRAS. XVII. p. 65, 4, 32. *in-na-te-e-en* = *te-iṭ-xi-šum,* 'thou hast gone to him,' P. AO. 5403, 9, also 'go to him,' ditto, 2. *in-da-má-e-en* = *ta-ša-(ka)-aš-(šu)-mu,* 'thou shalt place it upon him,' P. AO. 5403, 10.

HE-ME

MA-CLASS: *ma-an-si* = *i-din-nam,* 'he gave it to me,' P. p. 110. *igi . . . ma-ni-in-du-a,* 'when he looked upon me,' P. p. 104. *igi . . . mu-ši-in-bar-ra,* 'when he looked upon me,' P. p. 102. *ma-a-ar ma-an-du-ga,* 'when he commanded me,' P. p. 104. *ma-ra ma-an-du-ga,* 'when (to build) she ordered me,' P. p. 105. *xa-ma-ab-*

dím-e = *li-bu-ša-am,* 'may he make for me,' P. No. 142, rev. 2, 22.

MU-CLASS: *mu-ub-dim-e* = *i-bu(pi)-ša-am,* 'he made for me'; P. p. 57, rev. 3, 19, renders 'makes'(?). *nu-mu-ub-dím-e* = *u-la i-bu-ša-am,* 'he did not make for me,' P. No. 142, rev. 3, 20. *sag-túm-ma mu-un-ga* = *ma-gi-ir-tam ik-bi-a-am,* 'he spoke favorably to me,' P. No. 142, rev. 3, 21. *sag-ki . . . mu-ši-in-bar,* 'he looked on me,' P. p. 105. *mu-na-an-si,* 'he has given to me,' P. p. 110. *nam mu-un-tar,* 'he determined fate for me,' P. p. 105.

MI-CLASS: *nam-mu mi-ni-in-tar-ra,* 'after he had determined fate for me,' P. p. 105.

HE-HIM

MU-CLASS: *mu-na-ni-in-gi-gi,* 'he replied to me,' P. p. 93. *u . . . mu-na-an-si-ma-ta,* 'after he had given to him,' P. p. 105. *mu-na-ni-in-du,* 'he had built for him therein,' P. p. 105. *mu-na-an-ši-in-gar,* 'he made it for him,' P. p. 106.

MI-CLASS: *mi-ni-in* = *šu-u šu-a-ti šu-a-ti,* 'it for him,' P. JRAS. XVII. 65, 4, 27. *mi-ni-in-tar-ra,* 'when he had determined it for him (it),' P. p. 112.

BA-CLASS: *ba-on* = *šu-u šu-a-ti,* 'he it,' P. JRAS. XVII. p. 65, 4, 21. *ba-an-na-te* = *i-ṭe-xi-šum,* 'he went to him,' P. AO. 5403, 7.

BI-CLASS: *bi-in* = *šu-u šu-a-ti,* 'he it,' P. JRAS. XVII. p. 65, 4, 17–18. *šu-ni bi-in-si-a,* 'after he had placed it in his hand,' P. p. 105.

IB-CLASS: *ib-ri-tuk,* 'he shall receive for it,' (Clay).

I(N)-CLASS: *i-ni-in* = *šu-u šu-a-ti,* 'he it it,' P. JRAS. XVII. p. 65, 4, 26. *in-na-ab-si-mu* = *in-na-din-šu,* 'he shall give it to him,' P. p. 94. *in-na-ab-gi-gi* = *ip-pa-alšu,* 'he shall answer it to him,' P. p. 94. *in-na-ab-gur-ri* = *u-ta-ar-šu,* 'he shall return it to him,' P. p. 94.

THEY-ME

MU-CLASS: *xu-mu-ši-in-bar-ri-eš* = *lu-ip-pa-al-su-um,* 'they have looked upon me,' P. p. 104. *sag-e-eš xu-mu*-PA-TUG-DU-*eš,* 'they have given it to me as a gift,' P. p. 104.

BI-CLASS: *šu-mu-šù bi-in-si-eš-a,* 'when they gave it into my hands,' P. p. 104.

THEY-THEE

PRECATIVE CLASS: KA *xa-ra-ab-ša-ša-gi-ne* = *li-iš-te-mi-ga-kum*, 'may show reverence unto thee,' P. p. 110.

THEY-HIM

PRECATIVE CLASS: *xe-e-en-na-ab-dím-e-ne* = *li-bu-šu-šum*, 'may they make for him,' P. No. 142, rev. 2, 17.

MU-CLASS: *mu-un-ni-in*-PA-TUG-DU-*a*, 'when they had given to her as a gift,' P. p. 112. *mu-na-an-si-mu-uš-a* = *i-ti-nu-šum*, 'when they had given it or gave it to him,' P. p. 104. *mu-na-an-gi-ni-eš-a* = *u-ki-in-nu-šum*, 'when they had made secure for him,' P. p. 104.

I(N)-CLASS: *in-na-ab-ka-la-gí-ne* = *u-dan-ni-nu-šum*, 'they shall pay him,' P. p. 104. *na-an-na-ab-dím-e-ne* = *la i-pi-šu-šum*, 'may they not make for him,' P. No. 142, rev. 2, 18.

COMMENTARY.

1. *Ba-*, 'I' (*s.v.* I-HIM); cf. also IV. R. 14, obv. 20 a: *ki-bi-gar-ra ba-ni-ib-dur-ru* = *ina tâkulti lûšešib*, 'I will invite (them) to a feast'; probably first person, but the text is broken.

2. *Ba-*, 'thou' (*s.v.* THOU-HIM); not an uncommon usage. Cf. AJSL. XIX., § 20; IV. R. 17, 45 a; in IV. R. 30 nr. 3, rev. 15, there occurs a series of *ba——ne* forms all = 2 p. It is possible that the Assyrian scribe changed them from a 3 p. which perhaps was used for a general "you" like German *man;* French *on.*

3. *Ba-*, 'third person' (*s.v.* HE-HIM); occurs *passim.*

4. *Bi-*, 'I' (*s.v.* I-HIM); an unusual prefix in this sense.

5. *Bi-*, 'thou' (*s.v.* THOU-HIM); an unusual prefix in this sense. *Bi-*, as a *prefix,* is unusual in any case, even as the third person, as it is a common third personal *suffix.*

6. *Bi-*, . . . *-eš*, 'they' (*s.v.* THEY-ME); not common, although, if *bi-* is used in the singular 3 p., it is natural to find *bi-*, . . . *eš* for the plural.

7. *Ga-*, 'I' (*s.v.* I-THEE; I-HIM); a very common first personal prefix, probably from the cohortative *ga*; in fact, *ga-* was really cohortative originally, although it is frequently used in the sense "I will." Cf. Del., § 157, and AJSL. XIX., §23. It also = 1 p. in HT. 119, obv. 22: *ga-nu ga-ni-lax-en* = *alkam î nillikšu*, 'come let us go.' Note that is 1 p. plural here. On the other hand, *ga-* is used for the 2 p., IV. R. 11, 45 b: *en-nun ga-ne-tuš* (KU) = *ana maccarti tûšêšib*, 'thou shalt sit in the watch'; cf. AL.[3] 134, obv. 1: *an-sud ud-ag bil-gim sar-ki-ta za-e ši-in-ga-me-en bil* = *nûr šamê ša kîma išatim ina mâtim napxat attîma*, 'the light of the heaven which like fire in the land shines art thou, fire.' *Ga-* also may be used for the 3 p.; IV. R. 11, 19–20 b: *mu-uš-ku-pi azag-ga-na-ta a-an ga-mu-ri-a-bi* = *ina uznišu elliti minam ixsusa*, 'what has he planned with his brilliant ears.'

8. *Im-*, 'I' (*s.v.* I-HIM); cf. also IV. R. 6, 41 b: *ki-ta im-mi-in-ri*, 'I

placed it at the bottom,' but used with a preceding *mà-e*, 'I.' *Im-*, however, can mean "thou"; II. R. 16, 16 e: *er* (A-SI) *im-ma-an-šeš-šeš* = *tabakki*, 'thou weepest.' *Im-* is very common as a third personal prefix.

9. *In-*, 'I' (*s.v.* I-HIM). Very rare. Poebel's examples are the best instances of this use.

10. *In-*, 'thou' (*s.v.* THOU-HIM); IV. R. 7, 30 a: *nin ma-e nin-zu-a-mu za-e in-ma-e-zu* = *ša anâku idû atta tîdi*, 'what I know thou shalt know' (= 'for me' = *-ma-e-*?); cf. AJSL. XIX., § 28. *In-* is commonly used with the third person.

11. *In-*, . . . *-ne*, 'they' (*sv.* THEY-HIM); a logical third person plural.

12. *Ma-*, 'me, to me' (*s.v.* HE-ME); for this usage Poebel's examples are best. Note also *ma-an-se* = *iddinšu*, 'he gave it to him' with the third person, Br. 4418.

13. *Mi-*, 'I' (*s.v.* I-THEE). Poebel's example is the only one known to me.

14. *Mi-*, 'me' (*s.v.* HE-ME; HE-HIM). Note that the *-nin-* here = '*me*.' *Mi* is most common with the third personal sense, Br., p. 546.

15. *Mi-*, 'thou' (*s.v.* THOU-HIM); cf. also IV. R. 24, nr. 3, 6–7: *tul-tul-aš mi-ni-in-šid* = *tilâniš tamnu*, 'thou regardest it as a ruin,' but points back to a 2 p. *-zu* in line 3.

16. *Mu-*, 'I' (*s.v.* I-THEE; I-HIM); very common use (see AJSL. XIX., § 32).

17. *Mu-*, 'thou' (*s.v.* THOU-ME); only in Poebel, so far as I have met it.

18. *Mu-*, 'he' (*s.v.* HE-ME; HE-HIM); *passim;* AJSL. XIX., pp. 217–218.

19. *Mu-* . . . *-eš*, 'they' (*s.v.* THEY-ME); a natural plural of *mu-*, 'he.' Note *mu-* . . . *-uš*, the same plural, as *mu-* . . . *eš* with vowel harmony; *uš* for *eš*.

III.

Analysis of Material.

The prefixes, infixes and suffixes shown by the above table may be grouped alphabetically as follows:

an-ši-ib, '(turn thou) it to him' = *šib*.

ba- = 1, 2 and 3 p. subject.

ba-an-, 'he it.'

ba-an-na-, 'he it; he for him.'

bi- = 1, 2 and 3 p. subject.

bi- . . . *-eš* = 3 pl. subject.

ga- = 1 p. subject.

ga-mu- = 1 p. subject: 'I will.'

ĝen- . . . *-e-ne* = 3 p. pl. precative subject.

i- = 1 p. subject; cf. *i-ni-in*.

i(*b*)*-* = 3 p. subject.

im- = 1, 2 and 3 p. subject.

i-ni-in-, 'I to him.'
in- = 1, 2 and 3 p. subject.
in-na-, 'I to (for) him.'
in-na-ab-, 'he (they) to him.'
ma- = 2 p. subject; 3 p. subject: 'he to me.'
ma-ab-, 'he it for me.'
ma-an-, 'she (he) . . . me' (acc.).
ma-ni-in-, 'he upon me.'
mi- = 1, 2 and 3 p. subject.
mi-ni, 'I thee therein; I it for him; he it for me.'
mu- = 1, 2 and 3 p. subject. Note also the following:
mu- . . . *-eš* = 3 p. subject.
mu-na-an-, 'he to (for) him; he it to me; they it for (to) him.'
mu-na-ni-in-, 'he it for me.'
mu-ni-, 'I for him.'
mu-ši-in-, 'he for me.'
mu-un-, 'he for (to) me.'
mu-un-ni-in-, 'they it for (to) her (him).'
-na-, 'for (to) him.'
-na-ab-, 'it for (to) him.'
-ni-i-, 'for him'; *mi-ni-i-*, 'I for him; thou for him'; *ba-ni-e-*, 'I it for him'; *i-ni-i*, 'I it for him.'
-ra-, 'to thee.'
-ra-ab-, 'it for thee.'

Analyzing the above elements stlil further, we observe that the first personal subject may be denoted by: *ba-; ga-; ga-mu-; im; in-; i-; mi-; mu-*.

The second personal subject may be denoted by: *ba-; bi-; in-; ma-; mi-; mu-; mu--eš* (pl.).

The third personal subject may be denoted by: *ba-; bi-eš* (3 pl.); *i(b)-; in-; ma-; mi-; mu-*.

In other words *ba-, im-, in-, mi-*, and *mu-*, may indicate the 1, 2 and 3 persons indiscriminately, and that *ma-* = 2 and 3 persons, while *ga-* is almost always used for the 1 p.

Nor is the problem made easier by the tabulation of the 1, 2 and 3 personal objective infixes; viz., 1 p. object: *ma-*, 'he to me'; *ma-ab-*, 'he it for me'; *ma-an-*, 'she (he) . . . me' (acc.); *ma-ni-in-*,

'he upon me'; *mi-ni-*, 'he it for me'; *mu-na-an-*, 'he it for me'; *mu-ši-in-*, 'he upon me'; *mu-ub-*, 'he for me'; *mu-un-*, 'he for (to) me.'

The second personal object shows: *mi-ni-*, 'I thee therein,' but consistently *-ra-*, 'to thee; thee'; *-ra-ab-*, 'it for thee.'

The third personal object is seen in *ba-an-*, 'he it'; *ba-an-na*, 'he it; he it for him'; *in-na*, 'I to him'; *mu-na-an-*, 'he to (for) him; they it for him'; *mu-un-ni-in-*, 'they it for her (him).' The element *-na* clearly = 'to him,' as *na-ab* = 'it for him'; *-ni-i-*, 'for him,' as *ba-ni-i*, 'I it for him'; *i-ni-i*, 'I it for him'; *mi-ni-i-*, 'I for him; thou for him.'

We find in these forms the duplicate *mi-ni-i-*, 'I thee therein' and 'I for him' = 1 and 3 object; *mu-un-*, 'he for me,' but *mu-na-an-*, 'he it for me' and 'he it for him.'

Poebel's table of pronominal elements as used by the verb (p. 45) is most ingenious, but not satisfactory, as will be shown. His classification is as follows:

	Infixed.	Enclitic.	Absolute.	Suffix.
1 p.	ʿ	*m*	*m* (*de* and *en*)	*en*
2 p.	*e*	*z*	*z* (and *en*)	*en*
3 p.	*n*	*n*	*e*	*e*
Collective	*b*	*b*		

This he has elaborated from his Paradigms (pp. 70 ff); thus: *ni-la-en*, 'I pay'; *n* is preformative + the *i* which contains the 1 p. ʿ; *la* = *root* + *en*-suffix of the 1 and 2 p.; *ni-la-en* also = 'thou payest'; only here, he thinks, that his 2 personal *e* is contained in the *i* of *n-i*. *Ni-la-e* also means 'he pays,' where the *n* = preformative of the third person + connecting vowel *i* + root *la* + 3 personal suffix *-e*. The analysis of the forms, just given is my own, made from what I believe to be his theory. The ʿ-vowel for the first person again appears in the simple forms *i-dim*, 'I made'; the *e*-vowel of the second person in *e-dim*, 'thou madest' and the *n* of the 3 p. in *indim*, 'he made' (p. 78). Similarly *a-tum*, 'I brought' (*a* = *a*ʿ); *a-mên*, 'I am' (Clay); *e-tum*, 'thou didst bring' and *an-tum*, 'he brought' (p. 80) seem to indicate the correctness of his idea. But, without entering more deeply into this ingenious and carefully thought out theory, it may be demolished by the simple fact that *a-* (= *a*ʿ), *e-* and *n-* do not always mean the 1, 2 and 3

persons, although *a-* and *e-* *usually* occur in this sense. Thus, *a-* indicates the 3 p.: *a-rim-rim-ne*=*iṭ-ṭi-bu-(u)*, 'they have been immersed'; *kas-? a-ab-du*(KAK) =*ši-ka-ra i-ba-ba-di* and *a-ne-in-gi*=*ik-?-šu*, both clearly third persons, although the meaning of the stems is unknown (cf. Br., p. 548). On the other hand, *a-* generally indicates the 1 p. (Prince, AJSL. XIX., p. 211). The prefix *e-* is frequently used of the 3 p. as: *e-ag*, 'he made'; *e-gaz*, 'he killed'; *e-gen*, 'he went' (Delitzsch, § 135 and § 184a). As for *n*(*i*), Poebel himself gives examples cited above of *n*(*i*) used for both the 1 and 2 persons, while for the 3 personal use, cf. Br., p. 543: *ni-zu*=both *i-du-u*, 'they know' and *ti-di*, 'thou knowest'; *ni-gal* (IK) =*i-ba-aš-ši*, 'it is,' etc. *ad nauseam.* In other words, *a*, *e* and *n*(*i*) appear used for all three persons indiscriminately with a preference in favor of the 1, 2 and 3 persons respectively. On the other hand, is Poebel correct in supposing that the suffixed forms *-en* attached to verbs are characteristic of the 1 and 2 persons only? As in the case of *a*, *e* and *n* they appear indiscriminately for all three persons: *ni-la-en*, 'I pay'; 'thou payest,' as cited above, but *mu-un-tag-en*=*in-naq-qu-u*, IV. R. 19, 48 b; *mu-un-ši-in-gí-en*=*iš-pu-ra-an-ni*, 'they have sent me,' Br., p. 560. As to Poebel's 3 p. *-e*, it, of course, occurs often with the 3 p.; cf. *til*(BE)*-e*=*ig-da-mar*, Br., 1499, etc., but also *an-na-ab-uš-e*=*tu-ša-ax-xa-za-šu*, 'thou shalt cause him to seize it'; it is also a frequent sign of the imperative, as *kú-e*=*akul*, 'eat thou'; *uš-e*=*ri-da-an-ni*, 'have connection with me,' Br., 553.

There can be no doubt that Poebel is right in giving *m-z* and *n-b* as the respective characteristics of the endings of the postpositive conjugation, as *-mu-*, *-zu-* and *-ni*, *-na-* and *-bi-*, *-ba-* are the ordinary 1, 2 and 3 personal suffixes, respectively, of the postpositive *ḥâl*-clause; yet even here we find a variation, as the third person also appears with the ordinarily first personal *-mu* in relative clauses. This is the so-called *-mu* of the third person which I believe I was the first to call attention to (MSL. XXIX, § 32). The best illustration of it will be seen in the following phrases from IV. R. 27, No. 1, 4–11:

šinig-ga šar-šar a nu-nag-a-mu
bi-i-nu ša ina mu-sa-ri-e me-e la iš-tu-u

the grain which hath drunk no water in its bed;
suǧur edin-na pa nu-sìg-ga-mu
kim-mat-su ina ci-e-ri ar-ta la ib-nu-u
whose bud in the field no shoot has borne;
GIŠ-A-AM *šita*(RAT)*-na ba-nu-su*(*g*)*-ga-mu*
il-daq-qu ša ina ra-ṭi-šu la i-ri-šu
the sprout which in its water-ditch is not planted;
GIŠ-A-AM *ur-ra ba-ab-bu-ra-mu*
(*il-daq-qu*) *ša iš-da-nu-uš in-na-as-xu*
the sprout whose roots have been torn away;
gu šar-šar-ra a nu-nag-a-mu
qu-u ša ina mu-sa-ri-e me-e la iš-tu-u
the vegetation which in its bed has drunk no water;

A similar construction to the above is undoubtedly that in ASKT., p. 122, 16: *eri-zu-ka ág-gig-gá ak-a-mu* = *ana ar-di-ki ša ma-ru-uš-tum ep-šu,* 'unto thy servant (fem.) who has (lit. 'makes' = *ak*) sickness.'

It is perfectly evident from the above examples that we have here a purely relative *-mu* used with participles. This is probably identical in derivation with the demonstrative *mu-* in the regular relative pronoun *mu-lu* and also with the common *mu*-prefix of verbs. It is quite possible that this relative *-mu* was used to indicate all three persons, like the *mu*-prefix in verbs.

What then are we to conclude as to the pronominal use with the Sumerian verb? Is it possible to imagine a verbal system with no fixed method of expressing the pronouns? The existence of the practically fixed second personal value of the infix *-ra-* and of the very common use of *ga-* as a first person would lead us to suppose that the verbal prefixes were really not indeterminate pronominally, even though Delitzsch lays down the rule that there is no second personal conjugation in Sumerian (p. 102).

The existence of third personal elements has long been recognized. The difficulty lies in the apparently indiscriminate use of many verbal prefixes for all three persons and the fixation of this usage by Poebel's undoubtedly valuable equations. The question now is whether Poebel is right in supposing that there underlies in every case of a first personal usage the '-vowel, *i. e.*, that *mu-*, 'I'

stood for *mu-'*, while *mu*, 'he' did not contain this element. This is equally true of the *e*-prefix of the second person varying with *i*, cited by Poebel as characteristic. Are we to understand an *e*-element hidden in every second personal equation; *ba-*, *im-*, *in-*, *mi-*, *ma-*, *mu-*? The latter question must be answered in the negative, because, as just shown, *e* was not used exclusively of the second person. An examination of the paradigms as given by me in this paper will show the improbability of such a proposition.

The first thought which strikes the philologist studying this maze of apparently contradictory forms suggests the theory that in Sumerian, as in other languages, person in the verb must have expressed by the tone. This idea I suggested in AJSL. XIX., pp. 205–206, but no Sumerologist has ever gone into the matter. All scholars in this line have preferred, either to deny the distinction of pronouns by the verbal prefixes or else to suggest a difference in quantity (Paul Haupt, Sfg., p. 19, n. 6; Bertin, PSBA. V (1882–3), pp. 19 ff). But a difference in quantity or "accent," as some call it, would have been indicated at least by a prolongation of the vowel of the prefix. Real voice-tone would not have been so designated, any more than it is Chinese Wen-Li to-day. Grammatical tones actually exist in African Yóruba, as *ile ṛe*, 'thy house' but *ile ṛê* (another tone), 'his house'; in this language *o*='thou' but *ô* (another tone) ='he, she, it.' Nothing could be more suggestive than this parallel (cf. S. Crowther, "Grammar of the Yóruba Language" (London, 1852), p. 12). I cite it, not of course with the intention of connecting Sumerian with Yóruba, but simply to demonstrate the possibility of toned grammatical elements which do not occur in Chinese. The three persons expressed by *ba-*, *im-*, *in-*, *mi-* and *mu-*, the two persons by *ma-* and the similar apparently indiscriminate use of the infixes, noted above, all point only to such a solution, which is far more reasonable than the idea that hidden vowels exist in such prefixes and infixes. If these vowels were present, how were they distinguished? There is nothing in the inscriptions to betray their existence. The Chinese do not indicate tones in their writing, because they are as readily understood by the reader of a living language, as an English reader understands the distinction between words of identical sound and difference of mean-

ing such as 'so, sow' and 'sew'; 'low'='below' and the verb 'low,' etc. Similarly, the Babylonian priest to whom Sumerian was, if not in later times actually a living tongue, at least a pronounced idiom, would never have thought of indicating the tonal differentiation of the grammatical verbal elements. The very poverty of Sumerian phonetically and the apparent monotony of its consonantal elements go to show the necessity of supposing some special unindicated means of differentiation. There seems to be every reason to suppose that such elements cited above as *ba-*, *im-*, *in-*, *mi-*, *mu-* indicating the first, second and third persons in the verbal scheme must have been tonally differentiated.

There are only about eleven distinguishing consonantal elements in the language; viz., *b*, probably = near object and near demonstrative; *d* = partitive; locative; means; *g* = precative and intentional; hence, future (= also *ng* = *n*); *ğ* = pure precative optative, indicated herein by *x*; *l* (rare) = *n*; *m* = demonstrative and relating; *n*, probably = remote object and demonstrative; *r* = ethical dative; motion, direction towards, and perhaps rhotacism for *z* in the second person *-ra*; *š* = direction towards, similar to *r*, with which it may be connected etymologically; *t* = 'in' or 'out of'; location in general; *z* = pure second person, the only fixed consonontal grammatical value. Combine with these elements the vowels *a* = direction and *i* (*e*) = completed action, past and future, having a force like the Slavonic "perfective" forms, not forgetting that *i* may be the harmonic equivalent of *e* and *u* of *a*, and we get a reasonable explanation of most of the prefixes and suffixes of the language, particularly of the verbal prefixes treated above. See for a full discussion of these points, Prince AJSL. XXIV. pp. 354–365, and also in Encyclopaedia Britannica, XXVI, p. 77.

Poebel's infixes (pp. 70 ff), all which are, of course, well known, I will amplify by the following examples for the sake of clearness: *na*, 'to him'; *in-na-an-ba-e* = *uqassu*, 'he gives to him' = *na*, 'to him' + *n*, 'it' remote; *in-ne-la-e*, 'he will pay to them' (*-ne*); cf. *mu-ne-gen*, 'he went to him'; note that *šin* = 'them,' HT., p. 46, 25; *in-ši-in-se*, 'he gave to them'; *ma-la-e*, 'he will pay me'; here the *m* stands for the 1 p. + the directive *a*; *mu-ra-la-e*, 'he will pay thee'; the *mu* contains the demonstrative *m* + the tonal vowel of the

third person + the 2 p. *-ra-; in-ši-la-e,* 'he will pay to him': *i*, the perfective vowel + *n* = remote object 'it' + *ši* directive; ***mu-ši-la-e,*** 'he will pay to me'; the tonal ***mu*** of the 1 p. + directive *ši; **mu-e-**ši-la-e,* 'he will pay for thee': demonstrative *m* + tonal *u* of the second person + perfective *e; i(n)-ni-la-e,* 'he will pay upon (= for) it' = perfective *i* + remote object *n* + perfective *i* again + *ni,* really = 'therein'; *i(n)-na-ni-la-e,* 'he will pay to him upon it': same as the above with the directive *na*-insert; *ib-ta-la-e,* 'he will pay from it (out of it)'; perfective *i* + nearer object *b; i(n)-na (b)-ta-la-e,* 'he will pay to him from (out of) it'; same as above with directive *na* + nearer object *b; ib-da-la-e,* 'he will pay together with it'; better 'by means of it'; *ib* as above with the *da* of means; *mu-e-da-la-e,* 'he will pay together with thee' demonstrative *m* with tonal *u* of the second person + perfective *e* + *da* as above. Poebel's whole set of infix-paradigms may be explained satisfactorily following this system.

THE HALL AND CORBINO EFFECTS.

By E. P. ADAMS.

(*Read April 22, 1915.*)

About four years ago Professor Corbino[1] described some effects which are closely related to the Hall effect. These new effects all have to do with the production of a secondary circular current in a metallic disk when a primary radial current is sent through it, and the disk placed in a magnetic field perpendicular to its plane. The only metal in which Corbino was able to detect any of these effects was bismuth, perhaps owing to lack of sensitiveness in his methods, but more probably because he seems to have neglected to take the precaution of preventing circular currents in a parallel disk used to lead the radial current into the disk under investigation. These circular currents would produce an effect which would largely balance the effect sought.

Last year Mr. Chapman and I[2] measured this Corbino effect in twelve different metals. In two other metals, tin and zinc, the effect was too small to measure. The method of measurement consisted in measuring the current induced in a coil of wire placed parallel to the disk when the radial current was reversed about twenty times a second by a rotating commutator.

The result of these measurements showed that the circular current C, produced, was proportional to the magnetic field, H, and to the radial current, I, or

$$C = aHI.$$

In the magnetic metals and in bismuth a is not constant but it depends on the magnetic force. In all the other metals tried a appears to be constant.

In order to compare this effect with the Hall effect, we may

[1] *Physikalische Zeitschrift*, XII., pp. 561, 842, 1911.
[2] *Philosophical Magazine*, XXVIII., p. 692, 1914.

assume that the effect of the magnetic field is to produce an electric intensity at right angles to both the magnetic force and to the primary electric intensity, and proportional to their product and the sine of the angle between them. This we may take to be:

$$E' = cVHE,$$

where V stands for the vector product. Applying this to the Corbino effect in a circular disk where r_2 is the external radius and r_1 the internal radius we find:

$$E = \frac{I}{2\pi ktr},$$

where I is the whole radial current, k, the specific conductivity, and t the thickness. Then the transverse electric intensity is

$$E' = \frac{cIH}{2\pi ktr}$$

and the whole circular current:

$$C = \frac{c}{2\pi} \log \frac{r_2}{r_1} \cdot IH.$$

Therefore the constant a is equal to $(c/2\pi) \log (r_2/r_1)$

$$c = \frac{C}{IH} \cdot \frac{2\pi}{\log \frac{r_2}{r_1}}.$$

We may now make the same hypothesis about the Hall effect. Here it is known that if a current I flows through a rectangular sheet metal of length l, breadth b, and thickness t, there is a transverse difference of potential given by

$$e = R\frac{HI}{t},$$

R being the Hall constant. The transverse electric intensity is now

$$E' = \frac{cHI}{kbt},$$

and thus the transverse difference of potential is

$$e = \frac{c}{k}\frac{HI}{t}.$$

Thus

$$R = \frac{c}{k}.$$

The constant c may be determined from both the Hall and Corbino effects. Experiments that Mr. Chapman has recently been making show that c is the same when measured by the two effects.

Now it is known that the Hall effect varies in sign from metal to metal. This change in sign may be introduced in the hypothesis by supposing that the constant c varies in sign for different metals. The experiments that have been made show that the Corbino effect changes in sign with the Hall effect. Thus there can be little doubt that these two effects are essentially the same, and that any explanation of one effect will explain the other.

Corbino also showed that when a disk carrying a radial current was placed in a magnetic field so that the normal to the disk made an acute angle with the direction of the field, a torque was brought into play tending to make the disk parallel to the field. If ϕ is the angle between the normal to the disk and the magnetic force, the mutual energy of the circular current and the magnetic field is

$$W = \frac{c}{8\pi} \cdot IH^2S \cos^2 \phi,$$

where S is the area of the disk. Thus the torque tending to increase ϕ is

$$-\frac{\partial W}{\partial \phi} = \frac{c}{8\pi} IH^2S \sin 2\phi.$$

Mr. Smith has succeeded in measuring this torque in four or five different metals, including bismuth, and the values of c calculated from his results are in good agreement with those obtained from the measurement of the circular current.

The production of a circular current in a disk by a magnetic field acting on a radial current implies an increase in its resistance.

This increase may be readily calculated. The rate of heat production by the radial current is

$$\frac{I^2 \log \dfrac{r_2}{r_1}}{2\pi kt}.$$

By the circular current it is

$$\frac{2\pi C^2}{kt \log \dfrac{r_2}{r_1}} = \frac{c^2 I^2 H^2 \log \dfrac{r_2}{r_1}}{2\pi kt};$$

the total rate of heat production is thus

$$\frac{I^2 \log \dfrac{r_2}{r_1}}{2\pi kt}(1 + c^2 H^2).$$

If k' is the conductivity of the disk in the magnetic field we may write the total rate of heat production when a radial current I is sent through it

$$\frac{I^2 \log \dfrac{r_2}{r_1}}{2\pi k' t}.$$

Thus

$$\frac{k}{k'} = \frac{\sigma'}{\sigma} = 1 + c^2 H^2 \quad \text{or} \quad \frac{\partial \sigma}{\sigma} = c^2 H^2.$$

In this expression σ is the specific resistance of the metal and σ' its effective specific resistance in the transverse magnetic field. Now according to this view the resistance of a conductor should always be increased by a magnetic field. It is known, however, that with some metals notably iron and nickel, the resistance is decreased in a transverse magnetic field. Furthermore, the increase of resistance calculated from this formula is very much less than the increase actually observed. In the thought that the change of resistance might be dependent on the geometrical form of the metal Mr. Lester has measured the effect of a transverse magnetic field on the resistance of a number of metals, using disks with a radial current.

His results are in good agreement with previous measurements made with wires and strips. For example, in the case of bismuth, using the same disk that was employed to measure the circular current he found, for a field $H=5{,}000$, $\delta\sigma/\sigma=0.16$. Now $c=2 10^{-5}$ for $H=5{,}000$, so that $c^2H^4=.01$. It is thus certain that some other influence is effective in causing the main part of the change of resistance of a metal in a magnetic field; it is very probable that the field affects the molecular structure of the metal.

The interpretation of these results from the point of view of the electron theory of metallic conduction is unsatisfactory. I have worked out their theory[3] assuming free electrons in the metal that collide with the metallic atoms and obtained very simple expressions for the number of electrons in unit volume and their time between collisions. The numbers so obtained are of the same order of magnitude as have been obtained by other methods. But the difficulty of accounting for the difference in sign of the effect for different metals on any such simple theory indicates that if we are to hold to the electron theory of metallic conduction other forces than those resulting from collisions like those between hard elastic spheres must be supposed to act upon the electrons. The surprising thing is that so much can be explained by the simple theory of electrons when all such forces are neglected.

We have seen that the Corbino effect is, essentially, the same as the Hall effect. In its measurement and interpretation the Corbino effect has some important advantages over the Hall effect. In the first place it is not necessary to use the very thin films that are required to produce measurable Hall effects. And in the second place the absence of the free transverse boundaries render the interpretation of the Corbino effect simpler than that of the Hall effect.

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[3] *Philosophical Magazine*, XXVII., p. 244, 1914.

SOME RESULTS FROM THE OBSERVATION OF ECLIPSING VARIABLES.

By RAYMOND S. DUGAN, Ph.D.

(*Read April 24, 1915.*)

At Princeton we have been using for over ten years a stellar photometer devised by Pickering and similar to the one used for so many years to such good purpose by Wendell. This photometer has many virtues and but few vices. Its construction is such that the observer has the very comfortable conviction that nearly all the sources of systematic error he can think of are being rendered innocuous by the program of observation. In accuracy it is apparently excelled by the electrical photometers alone.

A large part of our researches at Princeton has for some time been the observational and theoretical study of eclipsing variables. From the beginning we were of the opinion that the patient distribution of observations repeatedly throughout the entire period of light-variation might very possibly bring us several facts to repay us for the great labor involved.

The first star subjected to this process was *RT* Persei.[1] 14,464 measures made during the years 1905–8, combined into a mean curve, showed at once the existence of a secondary minimum, 0.13 of a magnitude in depth, and with satisfactory distinctness a slight change in brightness between eclipses. This latter change was interpreted to mean that the two stars are ellipsoidal and brighter on the sides facing each other. An asymmetry, established with considerable certainty in the curve of primary eclipse, was found to prevail throughout the entire curve. Combining this with the knowledge of one component of the eccentricity derived from the observed displacement of secondary minimum toward the preceding

[1] See Contributions from the Princeton Univ. Obs., No. 1.

primary minimum, Shapley has recently explained this asymmetry as a Periastron effect—an increased brightness of the stars when nearest each other. There is another possible explanation which I shall consider in connection with the third star.

In 1909 the series of 18,384 measures of *z* Draconis was completed.[2] Evidence of a very shallow secondary minimum, only six or seven hundredths of a magnitude in depth, displaced slightly toward the following primary minimum, was clearly found. Ellipticity of figure and exchange of radiation were again demonstrated. These two effects have been abundantly verified in other cases.

The theoretical representation of the observations of both *RT* Persei and *z* Draconis just at the beginning and end of eclipse is a little unsatisfactory—the theoretical curve starts to drop more rapidly than the observed curve. This is possibly evidence that the stars are darkened toward the limb—like the sun.

It was hoped that the observations of *RV* Ophiuchi would not only demonstrate the existence of darkening toward the limb but would determine approximately the degree of darkening. But darkening seems to be very elusive sometimes. The observed secondary minimum is barely deep enough for the uniform solution, and a darkened solution requires a still greater depth. The well-determined primary minimum is strongly asymmetrical. The inter-radiation effect is about the same as in the other two stars, while the ellipticity is much greater. This latter fact when considered with the anticipation that the ellipticity effect would in this case be too small to detect—on account of the large distance between the component stars—is somewhat disconcerting. Then too there are several well-defined hollows in the curve—a conspicuous one just after primary and another just after secondary—and a long stretch before secondary when the star is much brighter than before primary. The only satisfying method of solving the curve seemed, therefore, to be a least square solution of the whole curve—a procedure hitherto avoided. The final theoretical curve, representing our present knowledge of the causes of light variation of these stars, is the result of the solution of forty-two equations with seven

[2] See Contributions from the Princeton Univ. Obs., No. 2.

unknowns. The probable errors are of satisfactory smallness, considering the amount of asymmetry.

It seems necessary in this case to face the fact of asymmetry unflinchingly. The secondary minimum gives no evidence of an eccentric orbit and the consequent possibility of a magnified periastron effect. The most conspicuous asymmetry is a greater brightness from middle of primary eclipse through to secondary than on the other side. A sine term of the first order with an amplitude of three hundredths of a magnitude takes care of the greater part of this. The existence of this sine term would probably indicate that the advancing side of the bright star is brighter than the following. If such were the case, then the loss of light during the early stages of eclipse—when the brightest part of the disk is being covered—and the gain of light right after totality would both be more rapid than would be the case if the disk were uniformly bright, as was assumed for the least square solution. The divergence from the theoretical curve of the well observed branches of primary minimum is in this anticipated direction and it is of about the right amount. A similar asymmetry has already been remarked in the curve of *RT* Persei and is probably also, at least in part, due to this cause. This suggested explanation is of course not new, but the evidence is apparently strong that the advancing side of the brighter component of some Algol variables is brighter than the following side. After this sine term is removed from the curve of *RV* Ophiuchi, there seem to be other changes in brightness of an amount small but seemingly guaranteed by the probable error.

In the system *RT* Persei, one star is one and one third times as large and five times as bright as the other; in *z* Draconis, one star is thirteen times as bright as the other but just about equal to it in size. The eclipse is very deep and nearly total. In *RV* Ophiuchi one star is twelve times as bright as the other but smaller. The brighter star is entirely hidden behind the fainter for about an hour. The two stars are farther apart than in the other two systems but they are apparently much more elongated.

During the seven years since completing the light-curve of *RT* Persei I have observed through an occasional primary minimum both visually and photographically. Recently I have observed two

secondary minima. The eclipses are now coming over forty minutes earlier than they are predicted by the elements determined from the original series of observations. This is very surprising in view of the accuracy with which the elements were determined. These elements were determined mainly from a large number of well observed minima grouped quite closely about two epochs about 700 periods apart. If the time of eclipse is fixed by the observations within a half-miniute in each of these two regions, then the period is known to one-tenth of a second. In cases where both branches of the minimum were continuously observed it seems hardly possible to change the observed time of any individual minimum by more than one or two minutes, and still represent the observations closely.

The photographic record of *RT* Persei, generously furnished me by Professor Pickering, showed that at —7,500 *P*, or 17½ years before the zero epoch, the eclipses were about 100 minutes late. The average period, then, is nearly a whole second shorter than the period determined from my original series of observations. This shorter period should have caused my observed minima to run off ten minutes from the predictions during the interval of 700 periods. This is intolerable. Beside my own observations, there are a good many observations by Wendell and several by Graff available. Of course a single estimated photographic magnitude is not nearly as accurate for determining the time of minimum as a series of photometric observations right through the eclipse.

Making now the correction to the shorter, average period, I have plotted the new residuals, and find that two periodic terms, one running its course in 12,000 eclipse-periods, or 27½ years, the other in one-third this time, or 4,000 periods, with coefficients of twelve and five minutes respectively, going through their zero values on the up grade together, fit the observations pretty well. The smaller period is of the order of magnitude of that to be expected from the revolution of the line of apsides caused by the observed prolateness of homogeneous stars. The amplitude is of the size to be expected from the smallest value of the eccentricity in accord with the observed shift of secondary minimum.

The important question, and one which is difficult to answer, is whether the time of the secondary eclipse shifts in either of these

periods. The revolution of the line of apsides in the shorter period would cause the time of secondary minimum to oscillate back and forth to an extreme of ten minutes before and after the midway point between successive primaries.

The evidence is both scarce and somewhat uncertain. The entire secondary eclipse amounts to a drop of but little over a tenth of a magnitude. An isolated photographic estimate of brightness is of little value here. Even from a continuous series of photometric observations, it is difficult to fix on the time of mid-eclipse within several minutes. My own observations, weighted according to the apparent certainty with which they determine the time of mid-eclipse, indicate rather strongly the shift in the time of secondary eclipse in the shorter of the two periods. The few observations by Wendell do not furnish any very strong evidence for or against this result. In no case were his observations carried through both branches of the eclipse, and consequently they do not determine the time of the eclipse with much accuracy. What disagreement there is, is in the same direction as in the observations of primary eclipse—for some reason the Wendell times of eclipse are nearly all earlier than mine. The evidence in hand at present points to a revolution of the line of apsides of *RT* Persei. I hope to observe occasional primary and secondary minima of this star during the next few years.

As a bi-product, I have determined the photographic curve from the Harvard observations and compared it with the visual curve of primary minimum. According to the observations, the eclipses in the two regions of the spectrum are of the same duration and the same depth, but the curves follow different paths. This is too strange a result to be taken very seriously, considering the paucity of the photographic material. I have, however, one minimum which I observed through a blue color screen made for the purpose. Through this filter the minimum was observed about 0.15 magnitude deeper than without the filter. So I conclude that the observations at the bottom of the photographic curve, which are few in number, are to be disregarded and the curve extended to the greater depth given by the color-screen observations. The difference in the character of the two curves indicates that when one star is in great part covered up, the light of the system is more reddish than when

they both shine undimmed. Either one star is redder than the other, or the eclipsed star is redder toward the limb than at the center.

z Draconis has been an equally interesting surprise. The average period is quite a little longer than was supposed. Some time ago it shortened up with great rapidity. The top and bottom of this sharp decline are well determined by observations from quite a variety of sources. The two sine terms combined in this case have periods of 7,200 and 2,880 eclipse-periods and nearly the same coefficients as in *RT* Persei—ten and four minutes respectively. The prolateness and eccentricity of *z* Draconis are about the same as those determined for *RT* Persei.

The secondary minimum of *z* Draconis is only 0.06 magnitude—half as deep as that of *RT* Persei; the observations of the secondary minimum are few and were all taken within a brief interval, and they furnish very little evidence. This star must also be kept under frequent observation. When observing this star with the 23-inch it requires a determined effort to see it at all when in the middle of its deep eclipse.

The greater depth of the photographic eclipse comes out very nicely in the case of *z* Draconis. As the eclipse increases, the light from the star becomes redder and redder. At deepest phase more than half the light is known to come from the fainter star. It is apparently much redder than its far brighter companion, a fact which is doubtless to be expected.

Lastly, in reducing the observations of *RV* Ophiuchi I found it necessary to predict the minima with a sine term of about 1,600 periods, and of small amplitude. The photometric history of *RV* Ophiuchi is short and incomplete compared with that of *RT* Persei and *z* Draconis, and no conclusions can safely be drawn from this result.

PRINCETON UNIVERSITY,
April, 1915.

SOME PRESENT NEEDS IN SYSTEMATIC BOTANY.

BY L. H. BAILEY.

(*Read April 23, 1915.*)

If an editor were to survey the families and genera and species of the vegetable kingdom, he would find himself making comparisons and drifting to conclusions respecting the character of the systematic work and the worth of various contributions. Many of these conclusions he might not be able to analyze. They might be very much in the nature of impressions, and yet they might be felt so strongly as to be convictions. It is a vast field that his oversight would cover, and the bases of comparisons would be of the most various kinds, yet the convictions in very many cases would be concrete. It may be well to consider for the moment some of these possible convictions, of course in no spirit of captiousness, but to bring other points of view on some of our common problems, even though these points of view may not always be capable of direct application.

Very likely, his first feeling would be a consciousness of the great variety in the methods of the monographs. The systematic work is rapidly specializing, and the specialists make their own criteria. The result is a marked diversity in the work, which all the efforts at standardization do not very much control. Probably, Bentham and Hooker's "Genera Plantarum" is the last of the comprehensive works to be brought to a completion by a single person or by two or three persons working as one. This is succeeded by the editorial work of Engler and Prantl in "Die Natürlichen Pflanzenfamilien," and later in more detail by Engler in "Das Pflanzenreich." Floras of countries and regions tend more and more to be constructed editorially, with contributions by specialists. All this results perhaps in closer work in the specialties and the details, but it may lack in coordination and in the balancing of the parts.

Probably all the larger conclusions by our hypothetical editor would be derived from this general situation. No longer do we have the controlling authority of one man, holding the work steady and maintaining a homogeneous method. I well remember a remark that Asa Gray made about his Compositæ, on which he had worked so long and so lovingly, seeing the end of his time and foreseeing the change of his method. I remember also that in those days I was somewhat violently interested in nomenclature and I proposed to publish on it; but Gray gently dissuaded me: it was some years before I understood why.

A Situation in Nomenclature.

In proportion as we lose the influence of a single controlling personality, or of a few personalities working in an understood harmony, do we resort to arbitrary and conventional methods of codification. This is well illustrated in the convulsions in nomenclature in recent years. In this country, for example, with the passing of Gray, we began to give up the combination of two words as the name of a plant, and to substitute the oldest specific name brought down through any number of genera. Intrinsically, one method is as good as the other, but we sought to arrive at uniformity by rigidly adopting one of them. A train of difficulties has followed this and other innovations, and instead of finding ourselves in full harmony of action, with one uniform practice in nomenclature, we have two or three or several practices, and to a considerable extent each worker making his own. The present situation in nomenclature is a vivid illustration of the failure of arbitrary means of standardization. The situation also has a social significance, as I shall attempt to suggest.

The probability is that we should have arrived at our destination sooner and with no greater confusion if we had allowed the situation to work itself out without formal regulation, recognizing more fully the principle of usage which in the end controls all language. We have probably made a mistake in endeavoring to substitute arbitrary priority for stability; at all events, we might have saved ourselves the very amusing exercise of upsetting a well established name for the purpose of substituting an older name in order that

we might make the name stable. It looks now as if usage were after all to control in the end, and in some regards quite independently of arbitrary regulations. The principle of undeviating priority has not yet controlled for any length of time in the development of language. It is a false premise.

I am not now arguing for a return to any older or prior method, nor in challenge of any current practice, and certainly not in criticism of any group of workers, for we shall probably outgrow our conventionalities sooner by working with them rather than against them. But I must protest, as I have protested many times before, against the assumption that the names of plants belong to botanists to do with them as they will. This is only another way of saying that these latinized names of plants are rightfully a part of language and are not mere formulæ or symbols to be used only by insiders. We desire that the public shall use this language. We publish our manuals with this purpose. We try to make plant books simple, that they may be popular. We take pains to spread the knowledge of plants and thereby to promote the love of nature. There are thousands of persons who sell plants, and the names become established in trade and represent commercial values. These values cannot be shifted readily from name to name; and if one makes a plea for correct nomenclature in plantsmen's catalogues and lists, one receives the reply that it is scarcely worth the while seeing that the names change so frequently. The custom of shifting the names is undoubtedly directly responsible for much of the disregard of new nomenclature on the part of dealers; and we must remember that the use of these kinds of names among the people is probably promoted more by the plant dealers than by the botanists. I judge that the botanists have not yet succeeded in securing the active and free cooperation of this great class of people.

Of course we are to recognize that much of the change is inevitable, that, in fact, it is a consequence of new and closer studies of the groups, resulting in a clearer understanding of generic and specific limitations. This is a contribution to knowledge which everyone must accept. But there is a class of changes which does not have this justification. I am conscious, in making inquiries,

that the first thought of some particularists appears to be a desire to see whether it is possible to change the names.

Nor am I yet ready to leave this subject. From a successful and sincere public lecturer, who is trying to lead the people to a knowledge of animals and plants, I had a request for aid containing the statement that he could devote only a little time daily "to the study of Latin and I want to get only a sufficient knowledge of it to enable me to know why the gipsy moth is called (*Porthetria dispar* L.) and whether *Raphamus raphanistrum* means a plant, an insect or a tribe of elephants." This person, of course, had not had a college training in these particular subjects, but he is not ignorant or inattentive. He writes that he has about 2,000 bulletins, many bound volumes and a special cyclopedia, nearly all of which material is classified, using a card-index. "It has taken a lot of work to do this but as I can spare from farm labor only about an hour each day for study I find the index is a great time saver by showing me just where to find what I want." This man will accomplish much with his methods of contact. But consider the position of this man if to a complicated system of nomenclature we add a continuous tendency to change; and I think it is fairly our obligation to consider his position.

When we feel within us the desire to change the names of genera and groups, let us think well of this man and his carefully considered hour,—what it would mean to him in cross-referencing, in indexing, in the readjusting of his work. If it is to bring new knowledge that we cannot so well record otherwise or indispensable definitions, very good; but the burden of proof always rests on the new name. The work with names is fascinating, even captivating, and every change identifies the worker with it; but we are not to forget that some of this work is likely to be of the kind that, in other fields, might be called pedantry.

Bear with me further while I call your attention to the fact that we are not only changing our plant names with apparent disregard of the users of them, but that we are also making them more complicated. To the name of the plant,—genus and species,—we add the authority. We now omit the punctuation and thereby make the

author in effect a part of the name. When the combination of two words was held to constitute the name of a plant, the author of the combination was sufficient for identification; but with the single-word system we carry the author of both the original specific name and of the new combination, and the whole becomes something like a complicated formula. This is a convenience to the worker with plant names, but he is not the only party concerned; his needs may be served in the citation of the synonomy. His obligation to the public is to present the simplest possible name and the least involved. If the history is to be retained in the name-compound, where may we not stop and how complicated may our formulæ finally become? We may in time evolve a phraseology, or an algebraic form, as complicated as some of the pre-Linnæan customs. We are really confusing two things,—nomenclature and bibliography. We should separate citation from nomenclature. We have no right to inflict the public with our taxonomic book-keeping.

There are three pressing needs in our present systematic botany, as I see it. One of these needs I have now tried to suggest, which is the urgency to subordinate the nomenclature question. This is specially important in a democracy, where we desire to give all qualified persons equal chance, where we are supposed to remove hindrances and arbitrary domination by central authorities and to allow the people to express themselves freely. The public has real rights in the names of plants. Soon we must stop playing with names.

A Situation as to Species and Genera.

The oversight that we assumed in the beginning would undoubtedly discover other interesting situations in our systematic work. What these comparisons might be would depend, of course, on the particular person who made them; but in respect to the American work, with which at the moment we are mostly concerned, any person could not fail to admire the quality of the monographs and lesser contributions. Although systematic botany may occupy a subordinate place in our teaching, it is receiving extensive and very expert attention both from amateurs and from those attached officially to the great collections, and the published work is such as to

give us much pride. Ability of a high order continues to express itself in this field.

We have noted the tendency to specialize. Persons become expert in certain detached groups of plants. We become most skillful in detecting the differences that may distinguish species, but it may be doubted whether we are equally skillful in bringing together the agreements that may formulate genera. We seem now to be discovering separateness. It does not follow that one who has nice judgment on species necessarily has equal authority on genera. The tendency to break up our old groups into many genera, is apparently the result of the application of the species-habit. It is a great question whether the method of separation is the proper one to apply equally in these two kinds of cases.

Perhaps we cannot hope for much result in the standardizing of the species-conception by our methods of herbarium work, but it ought not to be difficult to arrive at some kind of an agreement on genera. We may well consider the advisability of being progressive in searching out the ultimate specific units—so far as there are such units—at the same time that we hold a conservative attitude on genera, for we can scarcely assume that there are ultimate generic lines. Thereby we might make a truthful presentation of the vegetable kingdom at the same time that we avoid vast changes in nomenclature.

A Situation as to the Living Material.

With the needful specialization of the systematic work, we find ourselves with very unequal treatment in the different groups. This inequality is perhaps the most outstanding characteristic of our present phytographical publication. It is impossible at present to compile a general work with any clear approach to uniformity of handling in the different genera and families. This is due in part to the fact that some of the groups have been recently worked over whereas others still retain a traditional treatment. Nor is it desirable that there shall be rigid codification on genera, for we need the judgment of different workers and this necessarily leads to non-uniformity; the specialist is entitled to his method; and yet the inequalities in interpretation appear to be so great in many cases as to amount to inharmony and even to confusion.

While there is more hope in the standardizing of genera than of species, it is within the possibilities to arrive at some kind of agreement on specific values, but this is not to be expected as a result of codification or regulation: it must be a real agreement by men who are brought together on a new kind of study of a common line of problems.

As I have already indicated, I would not expect or even desire a dead uniformity of treatment in any range of systematic work, and least of all in species. It would be a great misfortune to lose the expression of personality in even such formal work as this. But there is need of a closer understanding as to the essential facts in the treatment of the members of a genus. If one were to look over *Erythrina,* for example, one would find about 50 species recognized, native in warm countries in the two hemispheres; and while there is much uncertainty as to the characters of given species, one would not find very wide disagreement between the different authors. If next one were to look on *Eschscholtzia,* one would find a wholly different state of things, notwithstanding the fact that this genus is confined to western North America. Gray saw about a dozen species in this genus; Greene, with more material to work on, saw 112 species; and Fedde sees 123. Jepson, who has studied them with care in the field, is not able to see a great number of species, although he finds numberless seasonal and other forms; and he does not see much hope in solving the *Eschscholtzia* puzzle by the usual study of herbarium material but rather by "combined field and cultural studies."

And here is the particular suggestion I desired to make in the writing of this paper,—that a few groups be worked out very carefully by growing the plants under observation and as far as possible under conditions of control and always, of course, in comparison with living feral material. Such studies might require some years, even in a relatively small group: very good—the results would be all the more convincing. If a half dozen groups could be worked over in this way, with discussion of the living material by standing committees of some recognized association, we should very likely arrive at a basis of judgment such as the present collecting and inci-

dental field notation and indoor study of dried material can never give us. The conclusions,—or the points of view, if conclusions were impossible,—would be invaluable in bringing us to an understanding and therefore to a substantial agreement on some of the matters that are now most perplexing us. This is now the greatest need in systematic botany.

This means that we should now study life histories with the purpose to apply the knowledge in systematic work. We shall come to the end in due time of the inventory process in describing new species. After a time we shall consider it to be scarcely worth the while to carry the separative process very much farther, and we shall then undertake a synthetic process of building up the forms into species-values. The current studies of variation and of plant-breeding are bringing us to a new point of view: it is now time that we begin the incorporation of these methods into our systematic work.

ITHACA, N. Y.,
April 23, 1915.

THE VARIABLE STARS *TV, TW, TX* CASSIOPEIÆ AND *T* LEONIS MINORIS.

By R. J. McDIARMID.

(*Read April 24, 1915.*)

The four Algol variable stars *TV, TW, TX* Cassiopeiæ and *T* Leonis Minoris have been under observation by the writer for the last three years with the polarizing photometer attached to the 23-inch equatorial of the Princeton Observatory.

The total number of measures made on the four systems is over 35,000, distributed among the four stars as follows, *TV* 9,920, *TW* 13,728, *TX* 8,486 and *T* Leonis 3,792. The light curves of the first three are well defined, while the observations on the last system are not so complete. The periods of the light variation with the exception of *TV* Cass. have been determined from my visual observations combined with photographic measures kindly sent me by Professor Pickering, of Harvard.

The systems will be discussed in slightly different order than the above, the more important left to the last.

The system *TW* Cass. has been observed by other astronomers, and notes published pronounced it as irregular in its variation, later Zinner found it regular in its variation and of the Algol type with a period $1^d\ 10^h\ 16.6^m$. From the discussion of the Princeton observations I found that the period was double the published period and instead of one eclipse there were two differing by 0.05 magnitude. The double period is confirmed by the three observed phenomena, 1st, the difference in depth of the two minima of 0.05 mg.; 2d, the interval from primary eclipse to secondary is 7.8 minutes longer than from secondary to the following primary; 3d, the primary eclipse is 36 minutes longer in duration than the secondary. It is from the knowledge of the last two facts that we are able to determine both components of the eccentricity—the quantities *e* and (longitude of periastron). The period is 2 days, 20 hours, and

33.6 minutes and the two eclipses have a depth of 0.62 mg. and 0.57 mg. respectively. From a discussion of the light curve following the theory as outlined by Professor Russell in *A. J.*, 36, 5; 36, 1 results were obtained giving the dimensions of the system in terms of the radius of the orbit. It was found that the two stars were of nearly the same size and had the same surface brightness.

In the case of *T* Leonis Minoris as in *TW* Cass. we have two minima, they are however of very different depth, the primary having a loss of 2.46 magnitudes while the secondary has only 0.05 magnitudes. The period is 3 days, 0 hours, 28 min., and 38.0 sec., and is accurately known. Combined with the visual observations I have used the Harvard photographic measures as far back as 1889, and have been able to establish a definitive period. The observations are not so complete as in the other systems, the length of the period being so nearly three days; also weather conditions at special times have entered largely into this.

From a study of the light curve along the lines of the eclipsing theory it has been found that the stars are of nearly the same size but are very different in surface brightness, the ratio being 1 : 18.

The third system *TV* Cass., whose period, $1^d\ 19^h\ 30^m\ 11.7^s$, has long been known, having been observed by Ashbury and Yendell, was placed under observation in October, 1913, at Dr. Shapley's suggestion. At that time nothing was known about the secondary eclipse. From my observations it was found that a secondary eclipse of 0.09 magnitude did really exist, coming 21 minutes before the time of mid period. The orbit of the system like that of *TW* Cass. is eccentric, but in this case the components of the eccentricity can not be separated.

In the two previous stars it was found from the light curve that the stars were of constant brightness between eclipses. The light curve of *TV* Cass. is somewhat different as there seems to be a gradual rise in the curve between primary and secondary eclipse which corresponds to an increase in brightness of the system. The explanation is, that the radiation of the bright star on the side of the fainter one as they approach the time of secondary eclipse tends to brighten its surface and thus give rise to the phenomenon observed in the light curve. From a study of the light curve it was found

that the fainter star was twice as bright on one side as the other. The stars are nearly equal in size with a ratio of surface brightness of 1 : 5.5. In this system the depth of the primary eclipse is 1.05 magnitudes and its duration a little over 6 hours.

The last system, *TX* Cass., is the most interesting of the four stars treated here. It was announced some years ago as being an irregular variable; later its period was given by Zinner with the note, that the period was probably changing. It was partly on account of these published notes that a thorough study of the light curve of the star was carried out. Owing to the nature of the variation the star has proved to be a difficult system for photometric study. The eclipses, primary and secondary, which undergo a loss of light of 0.54 and 0.33 magnitudes respectively and last over 18 hours, are difficult to observe, in fact it is impossible to obtain a complete minimum on any one night even during the long nights of winter. The Harvard photogarphic measures have again proved to be of extreme value and by combining them with the visual observations I was able to establish a definitive peroid. The period is 2 days, 22 hours, 14 min., and 41.7 sec.

In the systems so far discussed the stellar disc was considered of uniform surface brightness. Assuming this to be the case with the system *TX* Cass. it was found that the observations could not be represented at all satisfactorily; the deviations were in many cases three times the probable error. On the other hand assuming the stellar discs to be similar to the sun bright at the center and decreasing in brightness toward the edge, a very satisfactory representation of the observations was found. The hypothesis of darkened discs seems to be the correct one as it is confirmed by the nature of the eclipses; the secondary eclipse is total with a constant phase of six hours while the primary eclipse has no constant phase and the curve is distinctly round bottomed showing that the variation is continuous. This condition would exist with darkened discs in case of an annular eclipse, and since the secondary is total our natural and legitimate conclusion is that the primary is annular. The system *TX* Cass. seems to offer very strong evidence in support of darkening toward the limb in stellar systems.

The light between eclipses does not remain constant; the light curve is distinctly bowed up, showing the stars are elliptical in shape and have their greatest brightness when we see them broad side on. This is the condition midway between eclipses. From the study of the light curve it was found that the stars are very different in size, with the stars nearly of the same brightness having a ratio of 1 : 1.5. The ellipticity of the stars can best be shown by giving their dimensions expressed in terms of the radius of the orbit.

	Major Axis.	Minor Axis.
Big star	$a_1 = 0.567$	$b_1 = 0.519$
Small star	$a_2 = 0.295$	$b_2 = 0.270$

The stars in this system are very close together.

AN INTERPRETATION OF STERILITY IN CERTAIN PLANTS.[1]

By E. M. EAST.

(Read April 23, 1915.)

It is obvious that it is impossible to investigate the cause of sterility in hybrids by the pedigree culture method when such sterility is complete. Occasionally, however, one finds hybrids which are not wholly sterile. Such is the case in the historic cross, *Nicotiana rustica* L. × *Nicotiana paniculata* L. This hybrid holds an enviable position in experimental botany, since it was the first artificial hybrid to be studied. It was made by Kölreuter in 1760 and was studied by him for several years by means of back crosses with each parent.

This cross I repeated in 1909, using as the *N. rustica* parent a small variety *N. rustica humilis* Comes obtained from Dr. Comes through the kindness of Dr. D. G. Fairchild. It has now been studied through five generations both in the field (general morphology) and in the laboratory (histology and cytology). The essential points noted, as I see them, are as follows:

Two species giving extremely uniform progeny when selfed have, when crossed, given an intermediate F_1 population as uniform as themselves, and an inordinately variable F_2 population.

The germination of F_2 seeds varies in different samples from 20 to 60 per cent.

Practically no two F_2 plants are alike, and the parental forms are recovered once in every 100 to 200 F_2 plants.

In F_1, from 1 to 6 per cent. of the ♀ gametes are functional. It is impossible to determine the percentage of viable ♂ gametes formed from the pollen mother cells, but from 2 to 6 per cent. of the

[1] It is impossible to reproduce the photographs shown by means of lantern slides, but an illustrated paper giving the details of the investigation is to be published shortly.

pollen found is morphologically perfect. The maturation difficulty in spermatogenesis is largely at the first spermatocyte division.

F_1 plants are as fertile *inter se* as in back crosses with either parent.

Segregation of determiners for fertility occurs in F_1, so that by recombination some perfectly fertile plants are obtained in F_2.

Nearly all fertile F_2 plants selfed give only fertile progeny. Occasionally a fertile F_2 plant selfed may give a slightly non-fertile daughter.

Numerous combinations that should be possible in F_2 are omitted in the population obtained. Combinations approaching *N. rustica* seem to be more frequent than those approaching *N. paniculata.* Many more homozygous combinations occur in F_2 than might be expected.

Perfectly fertile plants giving perfectly fertile progeny, heterozygous for many allelomorphs, do occur in F_2.

No more than a very general formal interpretation of these facts can be made at present, but assuming that the chromosomes carry the hereditary character determiners, and that these react with the cytoplasm under proper environmental conditions to build up the soma, attention is called to the following possibilities of satisfying the conditions imposed by the data.

1. There is selective elimination of F_2 zygotes.

2. There is no evidence of selective fertilization. (I infer this from the fact that F_1 plants are as fertile *inter se* as in backcrosses.)

3. The selective elimination of non-functional gametes that must occur in F_1 and the recombinations of functional gametes that give different grades of fertility in F_2 cannot be interpreted by a Mendelian factorial notation without subsidiary assumptions, but possibly may be the result of one of the two following hypotheses:

(*A*) Through multipolar spindles, mating of non-homologous chromosome pairs at synapsis, or other mitotic aberrations at the reduction division, the 24 chromosomes characterizing each of the two species may be irregularly distributed at gametogenesis. If some of these irregular gametes may function, the majority of the experimental data are satisfied, but there are reasons which there is not time to consider which make this scheme improbable.

(*B*) On the other hand the facts may be interpreted without assuming irregularities of chromosome distribution if (1) there is a group of chromosomes in each parent that cannot be replaced by chromosomes from the other parent; if (2) there is a group of chromosomes from each parent, a percentage of which may be replaced by chromosomes from the other parent, but where functional perfection of the gametes varies as their constitution approaches that of the parental forms; if (3) there are other chromosomes that have no effect on fertility and therefore can promote recombinations of characters in the progeny of fertile F_2 plants; if (4) a naked male nucleus entering the normal cytoplasm of the egg in the immediate cross can cause changes in the cytoplasm that will affect future reduction divisions; if (5) this abnormally formed cytoplasm is not equitably distributed in the dichotomies of gametogenesis in the F_1 generation; if (6) it follows from (4) and (5) that F_2 zygotes may be formed which are less perfect in their gamete forming mechanism than those of the F_1 generation; and if (7) the heterotypic division of gametogenesis does not necessarily form two cells alike in their viability.

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AMERICAN PHILOSOPHICAL SOCIETY

104 SOUTH FIFTH STREET

PHILADELPHIA, U. S. A.

PROCEEDINGS

OF THE

AMERICAN PHILOSOPHICAL SOCIETY

HELD AT PHILADELPHIA

FOR PROMOTING USEFUL KNOWLEDGE

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ADDITIONS TO THE FAUNA OF THE LOWER PLIOCENE SNAKE CREEK BEDS (RESULTS OF THE PRINCETON UNIVERSITY 1914 EXPEDITION TO NEBRASKA).

BY WILLIAM J. SINCLAIR.

(Read April 24, 1915.)

One of the objects of the Princeton University 1914 Geological Expedition to Nebraska was to acquire, if possible, fossil bones from the Lower Pliocene Snake Creek beds of Sioux County, partly to fill out the exhibition and study collections of the Department of Geology, which were lacking in Pliocene vertebrates, and, partly, to obtain some additional light on the fauna of the Great Plains region in Lower Pliocene time, with the purpose of establishing a broader basis for the correlation of Continental Interior and Pacific Coast Tertiary deposits. In both respects the expedition was thoroughly successful, which I attribute, in large part, to the enthusiastic support of my assistants, Messrs. A. C. Whitford, of Lincoln, Nebraska, and Mr. Charles Barner, of Agate, and to the kindness of our temporary neighbors, the various ranchmen on whose ranges the bonebearing deposits lie.

The Snake Creek beds were named and described by Matthew and Cook,[1] and reference should be made to their paper for details not brought out in the pages which follow. The four exposures worked by the Princeton party lie within the limits of the Whistle

[1] "A Pliocene Fauna from Western Nebraska," *Bull. Am. Mus. Nat., Hist.*, N. Y., Vol. XXVI., Art. XXVII., pp. 361–414, 1909.

Creek quadrangle, south of the sandhills on the divide between the Niobrara and the North Platte rivers, in draws at or near the heads of Dry Spotted Tail Creek, Spotted Tail Creek and Snake Creek, as follows: Loc. 1000A, T. 26 N., R. 55 W., Sec. 31 (N. E. ¼); Loc. 1000B, same township and range, but in the southeast quarter of Sec. 33; Loc. 1000C, T. 25 N., R. 55 W., Sec. 3 (S. E. ¼ to middle of section); Loc. 1000D, T. 25 N., R. 54 W., Sec. 2 (N. E. ¼). Of the four, Loc. 1000C, to which the attention of Messrs. Whitford and Barner was called by Mr. John Weir, before my arrival in the field, was particularly productive, yielding some of our best material.

The Snake Creek beds comprise unconsolidated, water-worn gravels, clean, cross-bedded, round-grained sands sometimes streaked with magnetic, and a mortar-like, gray-white material, sometimes in angular fragments and sometimes in cobbles or boulder-like masses, resting with marked erosional unconformity on the Middle Miocene Sheep Creek beds. Rolled pebbles of granite, quartzite, etc., indicate water transportation from the crystalline rocks of the mountains farther west, probably some of the sand is windborne, but a large part of the Snake Creek matrix has not been transported far and consists, sometimes, of subangular fragments resembling in appearance dried mortar, and, sometimes, of gravels, cobbles, and large masses of more or less indurated clay or silt, evidently represented the harder portions of the Sheep beds through which the Snake Creek channels were cut. Many large, slightly rounded masses of Sheep Creek sediment incorporated in the Snake Creek sands and gravels are quite incoherent and could not have stood thorough saturation with water, not to mention transportation to any considerable distance. I think they were derived from the caving of undercut banks along channels incised in the Sheep Creek. Water-worn fragments of silicified wood are common, but are not necessarily remains of a forest contemporary with the Lower Pliocene fauna. Most of it, if not all, is *remanie* material.

The stratification is lenticular, water-worn gravels giving place laterally to cross-bedded sands and jumbled masses of clay boulders. Either gravels, sands or mortar-like fragments may rest with clean sharp contact on the eroded surface of the Sheep Creek, the irregu-

larity of which is increased by land sliding occuring along the sides of the draws where the exposures are found, but much of it is due to changes in the slope of the channel-beds in which the Snake Creek deposits accumulated. Upward, the formation merges into wind-blown sands and silts which cover the prairie top, and it is not always possible to distinguish between them, as bones sometimes occur in the lower layers of the sand above the level of the typical Snake Creek gravels.

Exposures, when found, are along the sides of the draws which have cut down through the Snake Creek beds into the underlying Sheep Creek, and are usually more or less obscured by wind-blown sand overgrown with grass and weeds, so that little in the way of fossils can be seen at the surface except an occasional weathered bone fragment on the bare spots between grass clumps. Occasionally, a larger ungrassed area of sand and pebbles may show a few horse teeth, a jaw fragment or two or the ends of some broken limb bones. All collecting was done by stripping off the surface sod and exposing the Snake Creek-Sheep Creek contact wherever the greater abundance of gravel and bone fragments suggested the presence of a productive "pocket" or lens of bone-bearing gravel. If the preliminary prospecting seemed to warrant further excavation, a large area was cleared and the bank cut back to a vertical face which was worked by undercutting at the level of the contact just mentioned. This was kept up until the productive gravel was exhausted or the repeating caving of the heavy top burden of sand made further work both laborious and dangerous.

The bones are remarkably well preserved, mostly black or of a dark color, and occur in both the gravels, sands and mortar-like conglomerate, becoming scarce as the sand gets clean or the number of clay boulders and cobbles increases. They are all more or less abraded, sometimes by water wear, at other times manifestly by wind-blown sand,[2] and vary in character from rolled bone pebbles to complete skulls. Hardly ever is there association of adjacent parts. Occasionally a *remanie* fossil, washed out of the Sheep

[2] The type skull of *Protolabis princetonianus* sp. nov. was found in soft sand, lying on the left side with the front of the skull tilted downward. The arch and back of the skull on the upper (right) side are pared down to a common level in a manner suggesting sand-blasting.

Creek, is found, but with this exception the bones seem to have been introduced directly into the streams which transported the Snake Creek gravel and, apparently, represent the fauna of the immediate vicinity, as frail teeth and delicate skull and jaw processes remain unbroken, suggesting that the bones have not been moved far. As will be seen by an examination of the Whistle Creek Quadrangle, our collecting localities are somewhat widely scattered and may not all represent the deposits of a single stream, possibly are not all strictly contemporaneous, but as our large collection from locality 1000C contains practically the same forms as are found in the remaining less fossiliferous localities, there is every reason to regard the fauna as a unit. So far as determined, the Snake Creek beds have yielded the following association of forms, those marked (*A*) being preserved in the American Museum, New York, (*P*) in the Geological Museum of Princeton University and (*C*) in the private collection of Mr. H. J. Cook, of Agate, Nebraska.

Dogs.

Amphicyon amnicola (*A*).
Amphicyon sp. indet. (*A*).
?*Amphicyon* sp. indesc. (*P*).
Aelurodon haydeni validus (*A*).
Aelurodon saevus secundus (*A*).
Aelurodon cf. *wheelerianus* (*P*).
Aelurodon sp. div. indet. (*A, P*).
Tephrocyon hippophagus (*A, P*).
Tephrocyon cf. *temerarius* (*A*).
Tephrocyon cf. *vafer* (*A, P*).
Tephrocyon mortifer (*C*).
Tephrocyon sp. maj. (*A, P*).
?*Cyon* sp. (*A*).

Civet-cat.

Bassariscus antiquus (*A*).

Mustelines.

Brachypsalis pachycephalus (*P*).
Brachypsalis obliquidens sp. nov. (*P*).
Martes glareæ sp. nov. (*P*).

Cats.

Pseudælurus near *intrepidus* (*P*).
Cat, non-machærodont (*P*).
Machærodont cat, gen. indet. (*A*).
?*Felis* cf. *maxima* (*A*).

Rodents.

Mylagaulus cf. *monodon* (*A*).
Dipoides curtus (*A, P*).
Dipoides tortus (*A*).
Hystricops cf. *venustus* (*A, P?*).
Geomys cf. *bisulcatus* (*A*).

Edentates.

Megalonychid, gen. et. sp. indet. (*A, P*).

Rhinoceroses.

Teleoceras sp. (*A, P*).
Aphelops sp. (*A, P*).
?*Cænopus* sp. (*A*).

Horses.

Archæohippus sp. (*P*).
Parahippus cf. *cognatus* (*A, P*).
Hypohippus cf. *affinis* (*A*).
Hypohippus sp. (*P*).
Merychippus cf. *insignis* (*A, P*).
Merychippus close to *calimarius* (*P*).
Hipparion cf. *occidentale* (*A, P*).

Hipparion gratum (*A, P*).
Hipparion cf. *affine* (*A, P*).
Protohippus cf. *placidus* (*P*, probably *A*).
Protohippus near *perditus* (*P*, probably *A*).
Pliohippus cf. *mirabilis* (*P*).
Pliohippus sp. div. (*A*).

PECCARIES.

Prosthennops cf. *crassigenis* (*A*).
Prosthennops sp. (*A, P*).

OREODONTS.

Merychyus (*Metoreodon*) *relictus* (*A*).
Merychyus (*Metoreodon*) *profectus* (*A, P*).
Merychyus (*Metoreodon*) sp. (*A, P*).
Pronomotherium siouense sp. nov. (*P*).

CAMELS.

Protolabis princetonianus sp. nov. (*P*).
Pliauchenia (*Megatylopus*) *gigas* (*A, P*).
Alticamelus procerus (*A, P*).
Alticamelus sp. div. (*A, P*).
?*Procamelus* sp. div. (*A*).

ANTELOPES AND DEER.

Dromomeryx whitfordi sp. nov. (*P, A*).[3]
Drepanomeryx falciformis gen. et sp. nov. (*P*).
Cervus sp. (*A, P*).
Blastomeryx elegans (*A*).
Blastomeryx cf. *wellsi* (*A*).
Merycodus necatus sabulonis (*A, P*).
Merycodus cf. *necatus* (*A, P*).
Merycodus sp. div. (*A, P*).

BOVIDS.

Neotragocerus improvisus (*A, P*).
Bovid gen. indet. (*A*).
Bison sp. (*A*).

MASTODONS.

Gomphotherium sp. (*P*).
?*Mastodon* sp. (*P*).

BIRDS.

Aquila danana? (*P*).[4]
Buteo near *borealis* (*P*).[4]

REPTILES.

Crocodile vertebra (*P*).
Lizard jaws (*P*).
Huge land tortoise (*A, P*).

OF UNCERTAIN POSITION.

Part of large mammal jaw (*P*).

The collections obtained by the Princeton expedition have greatly increased the number of Miocene genera represented in the Snake Creek fauna. *Archæohippus* excepted, *Brachypsalis, Pseudælurus, Pronomotherium, Protolabis* and *Dromomeryx* have species in the Upper Miocene, distinct, but not strikingly different from, their Snake Creek successors, rather increasing the close relationship of the fauna with that of the Upper Miocene previously commented on by Matthew and Cook. Additional Pliocene elements are far

[3] *Palæomeryx* sp. of Matthew and Cook.
[4] Represented in the Princeton collection by a fragment of the tarsometatarsus. Determinations by Dr. Loye Holmes Miller.

less abundant. Perhaps the new horned artiodactyl, *Drepanomeryx*, presenting a type of horn-core not hitherto known in North America, and a mastodon apparently allied to *Mastodon americanus*, may be regarded as belonging to this category. The conception of old and new faunal elements should not be unduly emphasized, because, as our exploration of the Snake Creek beds plainly shows, we do not yet know the extreme upward range in time of a number of Upper Miocene genera and can merely say of the new, supposedly Pliocene, forms that this is their first appearance. A suggestion regarding climatic conditions may be found in the presence of crocodiles and huge land tortoises, the latter rivalling in size those of the Galapagos Islands, indicating, perhaps, that the approaching chill of glacial times had not yet exterminated these cold-blooded types.

DESCRIPTIONS OF NEW GENERA AND SPECIES.

Aelurodon sp. compare wheelerianus?

The left ramus of a lower jar with $p_{\overline{4}}$ and $m_{\overline{1}}$ and alveoli for the remaining teeth (No. 12068 Princeton University Geological Museum, collecting locality 1000C) is referable to an *Aelurodon* of about the size of *A. wheelerianus*, from the type of which it differs in the greater length of $p_{\overline{4}}$–$m_{\overline{1}}$, the shorter jaw and the closer crowding of the premolars. It is either too small or too large to be referred definitely to any of the described species of *Aelurodon*, but is hardly complete enough to be made a new specific type.

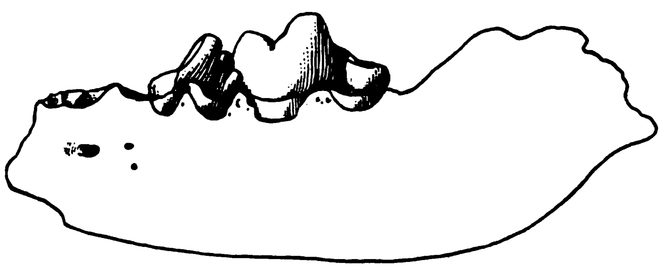

Fig. 1. *Aelurodon* sp., compare *wheelerianus?*, left ramus, side view, No. 12068, two thirds natural size.

?Amphicyon sp. indesc.

A huge canid, possibly an undescribed species of *Amphicyon*, is represented in the Princeton Snake Creek collection by the right

ramus of the lower jaw, an ulna and some other bones, of which the lower jaw (No. 12078 Princeton University Geological Museum, collecting locality 1000C) is here figured to give some idea of its size and proportions. The fragment retains alveoli for the canine, four double-rooted premolars and the sectorial molar. The first and second premolars are separated from each other by a short space, and from the canine and first molar by long diastemata, whlie the rest of the dentition is in close series.

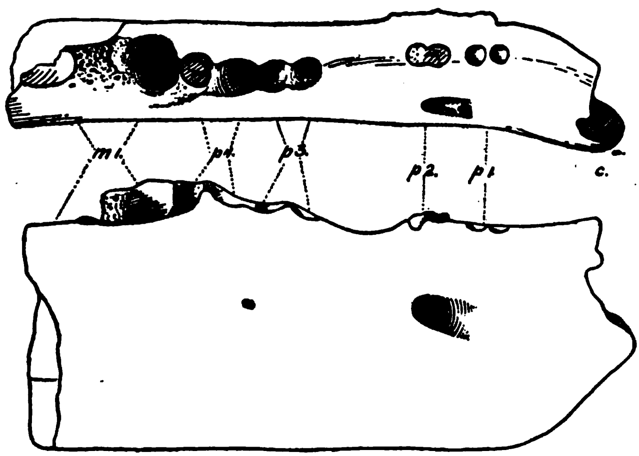

FIG. 2. ?*Amphicyon* sp. indesc., No. 12078, right ramus of the lower jaw, side and top views, one half natural size.

BRACHYPSALIS OBLIQUIDENS sp. nov.

Type No. 12070 Princeton University Geological Museum, collecting locality 1000C, the left ramus of the lower jaw with $p_{\overline{2}}$-$m_{\overline{2}}$ and alveoli of the canine and first premolar (Fig. 3). This is a decidedly larger, deeper-jawed, heavier-toothed species than *Brachypsalis pachycephalus,* with the anterior premolars placed very obliquely to the tooth-row and all the teeth closely crowded. It is of about the same size as *Paroligobunis* (*Brachypsalis*) *simplicidens* from the Lower Harrison, but has a larger second molar, a slightly larger sectorial and more closely crowded, obliquely placed anterior premolars.

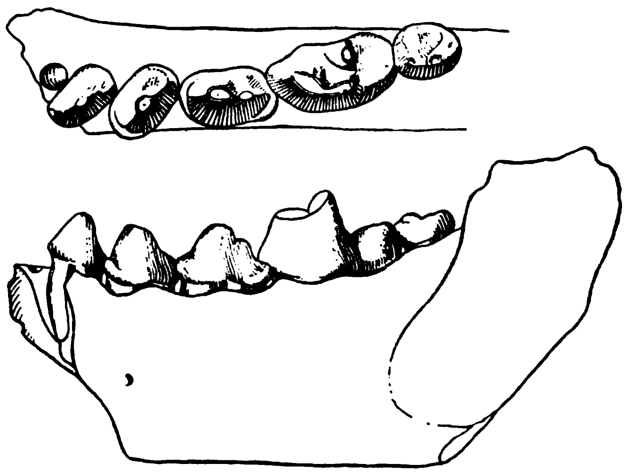

FIG. 3. *Brachypsalis obliquidens,* lower jaw, type specimen, external view, and crown view of the teeth, both natural size, No. 12070.

MEASUREMENTS.

Length, p_1-m_2	56
Length, p_1-p_4	32
Length, m_1-m_2	26
p_2	$9\frac{3}{4} \times 6$
p_3	11×7
p_4	$13 \times 7\frac{3}{4}$
m_1	$17\frac{1}{2} \times 9$
m_2	$9 \times 7\frac{1}{2}$

MARTES GLAREÆ sp. nov.

Type No. 12071 Princeton University Geological Museum, collecting locality 1000C, the left ramus of the lower jaw with p_3, $_4$ and m_1 and alveoli of the canine, p_1, $_2$ and m_2 (Fig. 4). In size, close to the type of *M. ogygia* Matthew from Horizon E of the Upper Miocene of Colorado, but differing in the presence of p_1 (represented by a small alveolus), the slightly larger, more laterally compressed p_3 which lacks a posterior accessory cusp as in *ogygia* and some existing species, the presence of this cusp on p_4 (only slightly less developed than in specimens referred to *M. americana* with which comparison was made), and the larger heel on m_1. In both *M. ogygia* and *M. glareæ* the metaconid or m_1 is more sharply separated than in specimens referred to *M. americana* which I have

examined. *M. minor* Douglass from near the bottom of the Lower Madison Valley Loup Fork beds and *M. furlongi* Merriam from the Thousand Creek beds, Thousand Creek, Nevada, are smaller forms, while *M. parviloba* Cope from the Middle Miocene of Colorado is a larger animal than either *ogygia* or *glareæ,* and *M.* (*Putorius*) *nambianus* from the New Mexican Loup Fork has a shorter jaw than either of the species just mentioned. It is approached in size by specimens in the Princeton University osteological collection referred to *M. americana,* but differs, in addition to the characters cited above, in the larger heels and heavier anterior basal ledges on the premolars and the greater degree of lateral compression of these teeth.

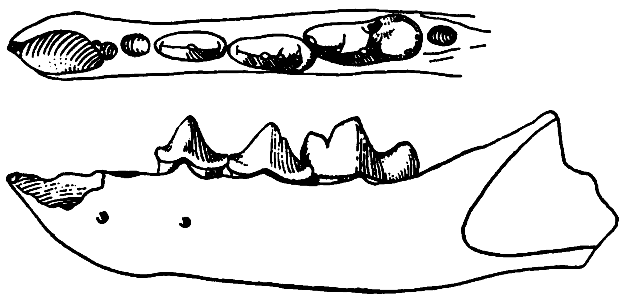

FIG. 4. *Martes glareæ,* lower jaw, type specimen, external view and crown view of the teeth, twice natural size, No. 12071.

MEASUREMENTS.

Length, p_3-m_1	17
p_3	5 × 2
p_4	5.8 × 2.1
m_1	8 × 3

PSEUDAELURUS near INTREPIDUS Leidy.

The presence in the Snake Creek fauna of a cat not far removed from *Pseudaelurus intrepidus* Leidy is indicated by a jaw fragment No. 12081 Princeton University Geological Museum, collecting locality 1000C, which agrees with Leidy's type fairly closely in the dimensions of the jaw, but differs in having the teeth a little smaller and the posterior accessory cusps and heels on the premolars less strongly developed. A further difference, which may be of little importance, is found in the position of the mental foramina which,

in *P. intrepidus*, occur below the alveolus for $p_{\overline{2}}$ and the anterior root of $p_{\overline{3}}$ respectively, while in the Snake Creek form they lie below the posterior root of $p_{\overline{3}}$ and a little in front of its anterior root. The alveolus for $p_{\overline{2}}$ is quite small and must have supported a minute vestigial single-rooted tooth.

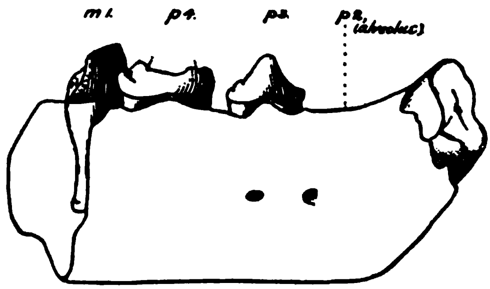

Fig. 5. *Pseudælurus* near *intrepidus*, lower jaw, right side, No. 12081, natural size.

FELID gen. et sp. indet.

A large non-machærodont cat is represented by a fragment of the left mandibular ramus No. 12073 Princeton University Geological Museum, collecting locality 1000A, in which are preserved the alveoli for three incisors, the base of a very large laterally flattened canine and alveoli for two premolars, a very small single-rooted $p_{\overline{2}}$ and a large double-rooted $p_{\overline{3}}$. The chin is not flanged but the symphysial region projects a short distance below the level of the lower border of the jaw.

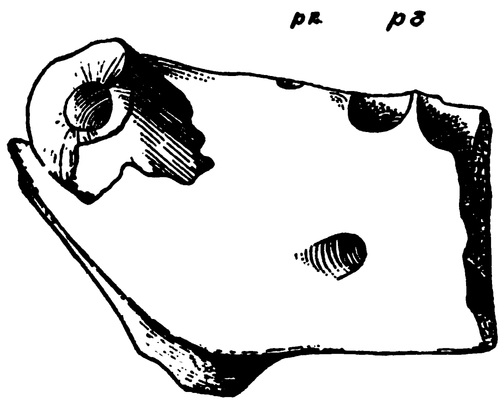

Fig. 6. Indeterminate felid, fragment of the lower jaw, left side, lateral view, No. 12073, natural size.

Edentate (?Megalonychid).

A single imperfect claw, "definitely recognizable as of Gravigrade relationship" and comparable "with some of the smaller Megalonychidæ" is reported by Matthew and Cook from the Snake Creek beds. Further confirmation of the presence of edentates is found in a navicular bone (Fig. 7) unquestionably of a Gravigrade, about two thirds the size of the navicular of *Megalonyx jeffersoni* and of much the same general type, obtained by the Princeton expedition at collecting locality 1000C.

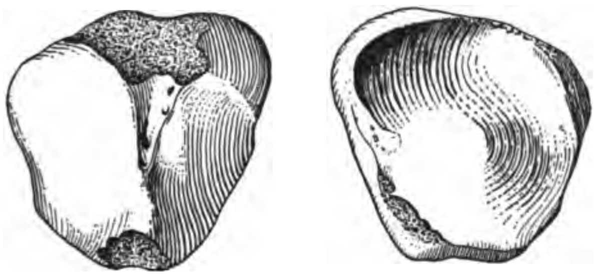

Fig. 7. Navicular bone of gravigrade edentate, upper and lower views, two thirds natural size, No. 12079.

Mastodons.

Mastodons of two types are indicated in the Princeton Snake Creek collection by several complete molars, most of which seem

Fig. 8. *Gomphotherium* sp., right last lower molar, one half natural size. No. 12064 Princeton University Geological Museum, collecting locality 1000 A.

referable to *Gomphotherium,* with a last lower molar carrying four cross-crests and a heel and having the intervening valleys blocked by large accessory tubercles (Fig. 8). A smaller form (Fig. 9), also with four cross-crests and a heel in $m_{\overline{3}}$, has the summits of the crests much more acute than in the *Gomphotherium* type and the valleys as free from accessory tubercles as in the corresponding tooth of *Mastodon americanus* to which the Snake Creek form is, possibly, related. Accessory ridges occur on the front and rear of the external half of each crest, but are no more strongly developed than in *M. americanus.* The last lower molar of the latter does not decrease in width posteriorly as rapidly as does the tooth here considered, but in other respects they closely resemble each other. The crown is unworn and there is no trace of cement.

FIG. 9. ?*Mastodon* sp., left last lower molar, two thirds natural size. No. 12116 Princeton University Geological Museum, collecting locality 1000 A.

INCERTÆ SEDIS.

A fragment of the left ramus of a lower jaw, No. 12091 Princeton University Geological Museum, collecting locality 1000A, has not been determined generically (Fig. 10). The specimen shows alveoli for two incisors and part of the root of a third. The first alveolus is very large and shallow and the second narrow and deep. The fragment of the root of the third incisor is strongly compressed

laterally and almost quadrangular in cross-section. These are followed after an intervening space, throughout which the dental margin of the ramus is broken, by a small, single-rooted, conical

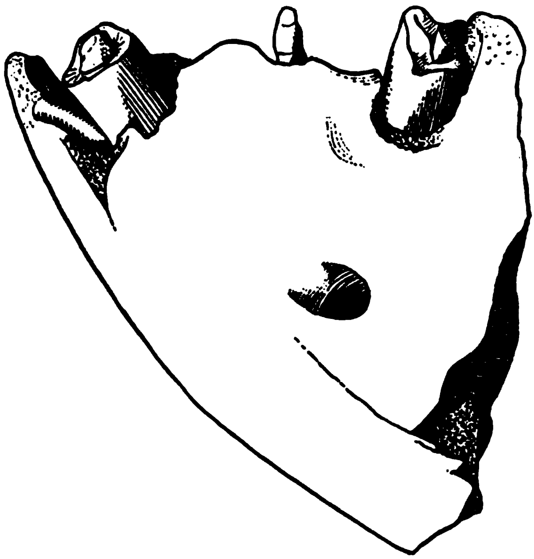

FIG. 10. *Genus incert. sed.* No. 12091, a fragment of the left ramus of the lower jaw, outer side, two thirds natural size.

tooth with enamel-covered crown. A second diastema, with undamaged margin, separates this tooth from the anterior root of a large, evidently deciduous tooth, beneath which, in the jaw, is the cavity for a still larger permanent tooth. The root of $i_{\overline{3}}$ seems to have projected into this cavity where it has been truncated by absorption. The symphysis is firmly fused, a small portion of the right ramus adhering to the left one and showing part of the alveolus for the first incisor of the right side.

MEASUREMENTS.

$i_{\overline{1}}$, anteroposterior diameter of alveolus (approximate)	23
$i_{\overline{1}}$, transverse diameter of alveolus (approximate)	18½
$i_{\overline{2}}$, anteroposterior diameter of alveolus (approximate)	6½
$i_{\overline{2}}$, transverse diameter of alveolus (approximate)	9
$i_{\overline{3}}$, anteroposterior diameter of root	16
$i_{\overline{3}}$, transverse diameter of root	9
?c, anteroposterior and transverse diameters	6

Length of diastema ?c-dp	15
Depth of jaw below middle of dp	107
Thickness of jaw at level of mental foramen	45

ARCHÆOHIPPUS sp.

A small short-crowned $p_{\overline{2}}$ of the right side (No. 12128 Princeton University Geological Museum, colecting locality 1000B) agrees in structure with the upper teeth of *Archæohippus* in the complete union of the metaloph and ectoloph, the distinct protoconule, and open prefossette, there being no anterior median enamel fold on the wall of the metaloph. This horse has not been reported hitherto from any horizon above the Middle Miocene Mascall beds of Oregon.

MEASUREMENTS.

Greatest anteroposterior diameter	13
Greatest transverse diameter	13

PRONOMOTHERIUM SIOUENSE sp. nov.

Type No. 12057 Princeton University Geological Museum, collecting locality 1000C, the right ramus of the lower jaw with $p_{\overline{1}}$-$m_{\overline{3}}$ and alveoli of $i_{\overline{1}}$-c. Tooth crowns worn. A smaller form than

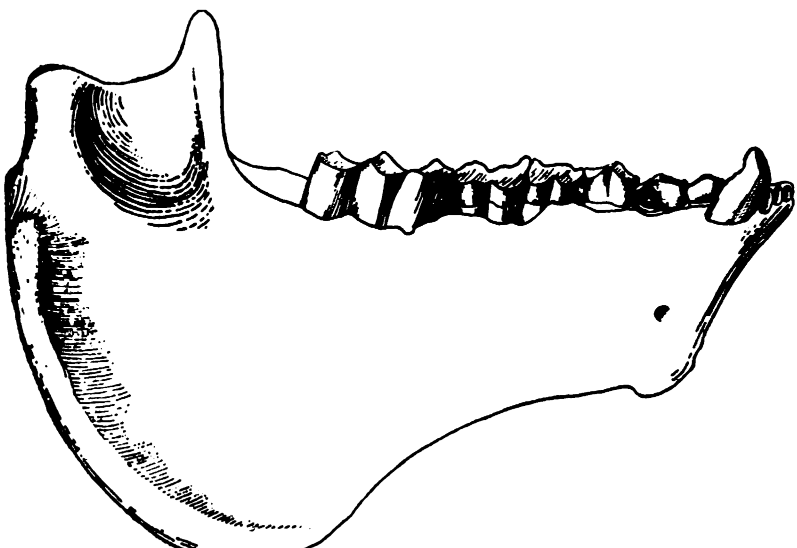

FIG. 11. *Pronomotherium siouense*, lower jaw, type specimen, external view, $\times \frac{1}{2}$.

either of the better known Miocene species (*P. laticeps* and *P. altiramis*) from which it can be separated by differences both in size and proportions.

MEASUREMENTS.

Length of jaw	208
Depth beneath p_3	57
Depth beneath m_1	53
Depth beneath back part of m_2	55
Depth beneath last lobe of m_3	94
Depth coronoid to angle	147
Length lower dental series	132+
Length lower premolar-molar series	125
Length lower premolar series	50
Length lower molar series	75

PROTOLABIS PRINCETONIANUS sp. nov.

Type No. 12053 Princeton University Geological Museum, collecting locality 1000C, an uncrushed skull, sand-worn on the right side which lay uppermost, associated with most of the left ramus of the lower jaw, a fragment of the right ramus and an ulna-radius. The limb bone belongs to a camel but may not pertain to the same individual as the skull. In size, there is close agreement with *Protolabis longiceps* Matthew from the Colorado Loup Fork (Pawnee Creek beds), but a comparison of the two skulls brings out certain minor differences which appear to be of specific value. In *P. princetonianus,* the anterior facial vacuity is far larger than in the Colorado form, with the premaxillæ extending above it and reaching farther back than in that species. Another marked difference appears in the absence of an abrupt constriction of the face in front of p^2 which produces the sudden incurving of the tooth row seen in *longiceps* in contrast with the gradual taper of this region in the Princeton specimen. Various differences in dental structures are also noticeable, as follows: p^2 thicker and heavier and p^3 less reduced and with posteroexternal groove deeper than in *P. longiceps;* p^4, if anything, larger in *longiceps* than in *princetonianus.* Lower premolars somewhat less reduced and molar crowns somewhat higher, and posteroexternal groove in p_T placed nearer hinder end of tooth than in *longiceps;* $p_{\bar{3}}$ with distinct anterior cusp which is absent in the last named form.

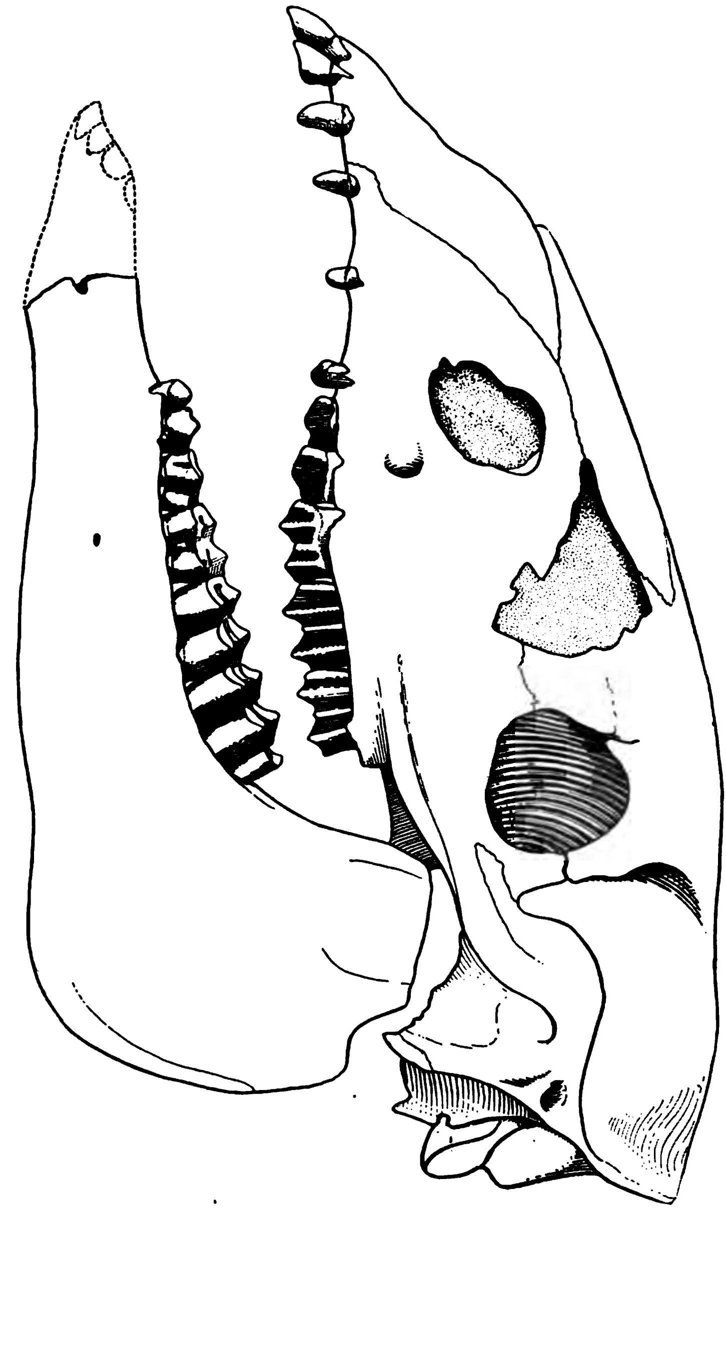

FIG. 12. *Protolabis princetonianus*, type specimen, left side of skull and lower jaw, one half natural size, No 12053. Symphysis of lower jaw restored in outline from another specimen of the same species.

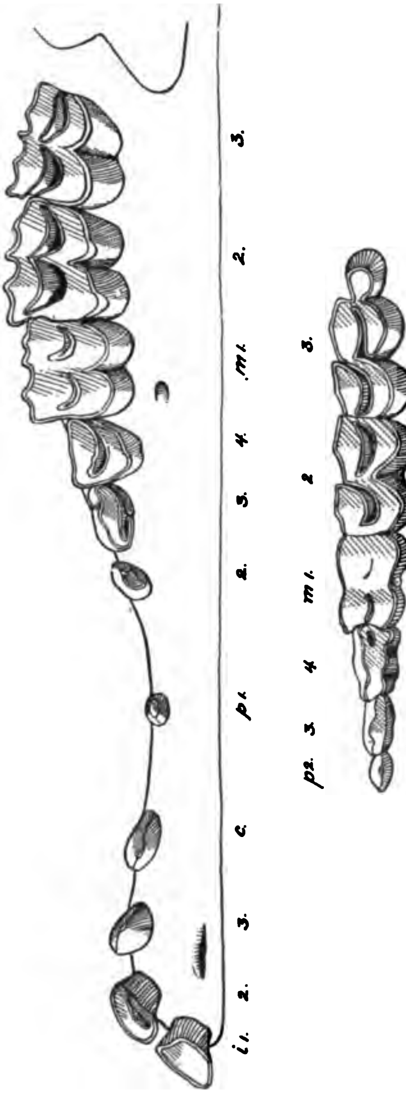

FIG. 13. *Protolabis princetonianus*, type specimen, crown view of the upper and lower teeth, two thirds natural size, No. 12053.

MEASUREMENTS.

Total length of skull (incisors to condyles)		310
Length, i^1-m^3	right 195,	left 199
Length, p^1-m^3	right 121,	left 127
Length, premolar series	right 59½,	left 64
Length, diastema behind i_3	right 11,	left 9
Length, diastema behind c	18	
Length, diastema behind p^1	right 18,	left 20
Length, lower premolar-molar series		106
Length, lower premolars		33
Depth of jaw in front of p_2		32
Depth of jaw below middle of m_3		40
Length of radius		195
Width of radial shaft at middle		24

DREPANOMERYX FALCIFORMIS gen. et sp. nov.

Type No. 12072 Princeton University Geological Museum, collecting locality 1000C, a horn of the left side (lacking tip) and the basal portion of the right horn (Figs. 14, 15).

Frontal not cavernous at base of horns. Horns non-deciduous, rising immediately above upper posterior margin of orbit, sloping backward and upward and at the same time curving inward, at base almost circular, but flattening upward in the transverse plane extending backward and inward from the orbits, producing a scimitar-like structure which curves inward toward its fellow on the opposite side. Horns without any suggestion of twist, proximal half comparatively smooth and free from pits and irregularities, such faint groovings as are present being longitudinal. Distally, and especially toward the outer margin, the surface is rough and pitted, but this seems to be due to sand-blasting or water-wear which has destroyed the outer table of bone. A broad groove is visible throughout the central portion of the shaft on the posterior aspect of the horn. Horns solid throughout, the surface, texture resembling that of the Pronghorn Antelope.

No teeth have been found in the Snake Creek beds which can be referred, even provisionally, to the new form, unless those which have been correlated by Matthew and Cook with their *Neotragocerus improvisus*, and the lower jaw described under that genus in the present paper, should be associated with the curved type of horn found in *Drepanomeryx* rather than with the straight horns of *Neotragocerus*.

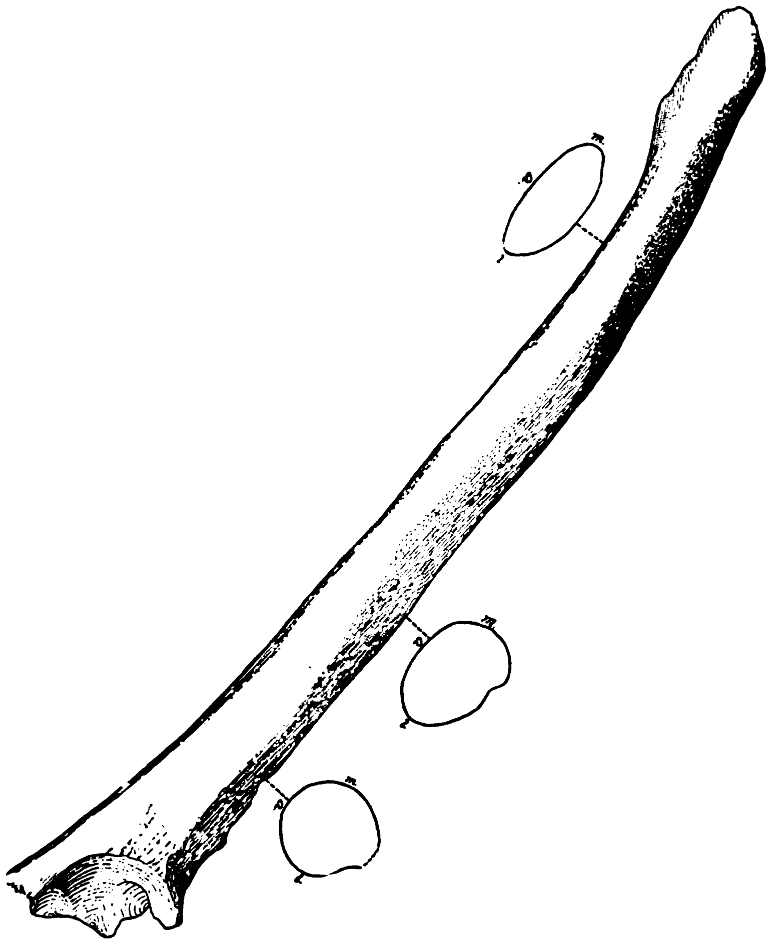

FIG. 14. *Drepanomeryx falciformis*, type specimen, lateral aspect of the left horn, one half natural size, No. 12072. *l, m, a* in cross-sections = lateral, median and anterior margins.

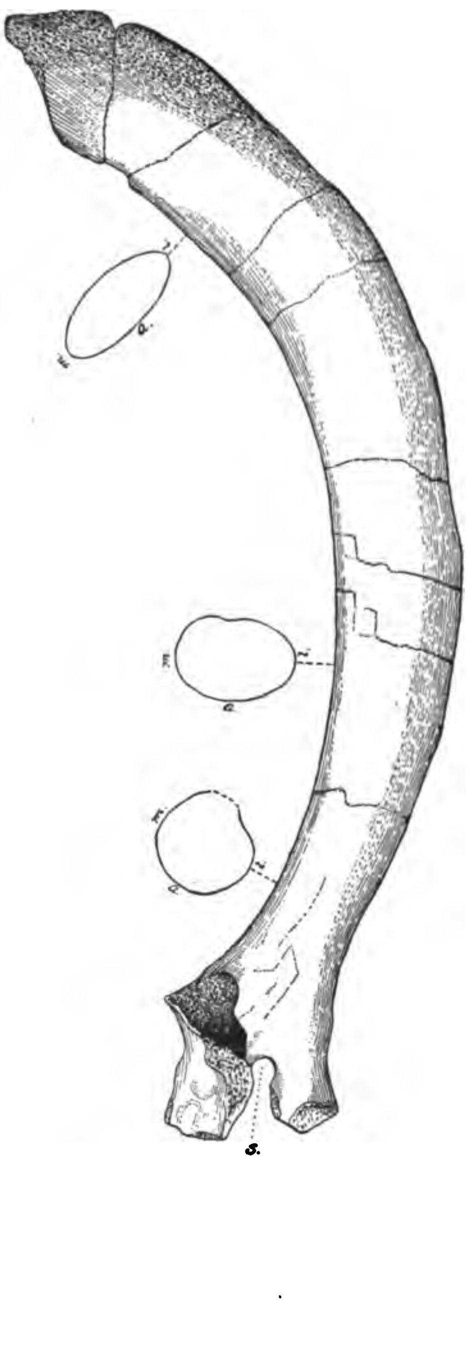

FIG. 15. *Drepanomeryx falciformis*, type specimen, anterior aspect of the left horn, one half natural size, No. 12072. *l*, *m*, *a* in cross-sections = lateral, median and anterior margins; *s*, supraorbital foramen.

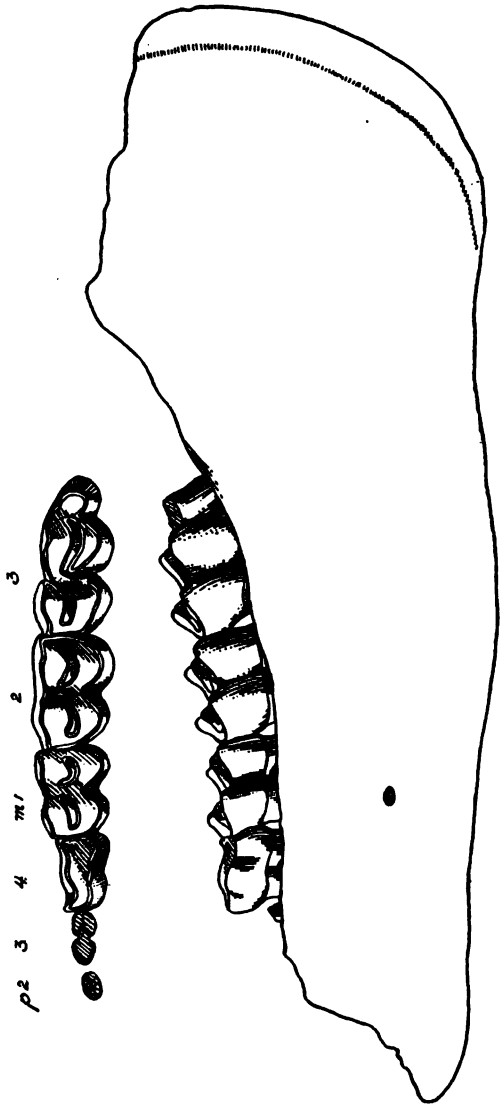

FIG. 16. *?Neotragocerus improvisus*, left ramus of the lower jaw, side view, and crown view of the teeth, two thirds natural size, No. 12106.

NEOTRAGOCERUS IMPROVISUS Matthew and Cook.

The left ramus of a lower jaw (No. 12106 Princeton University Geological Museum, collecting locality 1000C), which is doubtfully referred to this form, supports brachyodont molars which register almost exactly with the upper teeth selected by Matthew and Cook as paratypes of *Neotragocerus improvisus*. With the discovery in the Snake Creek beds of scimitar-shaped horns (*Drepanomeryx* gen. nov.), presumably of antelope-like animals, correlation of the straight *Neotragocerus* type of horn with jaw fragments, both upper and lower, supporting short-crowned teeth becomes even more provisional than it has hitherto been, since either type of horn is large enough to fit an animal of the size of those to which the jaws belonged.

DROMOMERYX WHITFORDI sp. nov.

Type No. 12054 Princeton University Geological Museum, collecting locality 1000C, an associated pair of horn bases (Fig. 17). Paratype No. 12086 Princeton University Geological Museum, the right ramus of a lower jaw, unassociated with the horns but from the same collecting locality (Fig. 18). The species is named in honor of my assistant in the field, Mr. A. C. Whitford. Horn bases about one third wider than in *D. borealis*, with the posterior upper corner of the wing-like expansion at the base of the horn

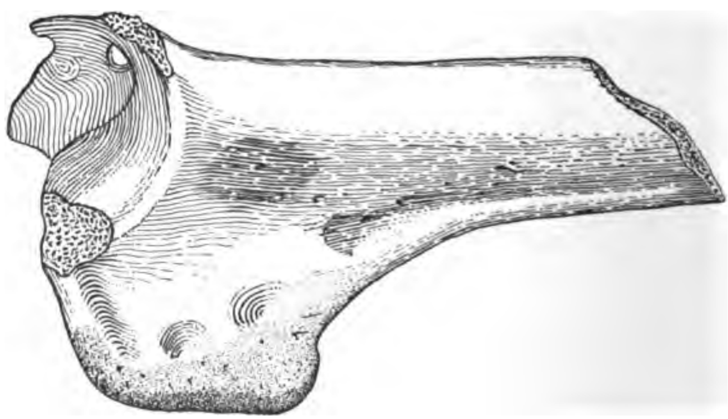

FIG. 17. *Dromomeryx whitfordi*, type specimen, base of left horn, outer side, two thirds natural size. One of an associated pair, No. 12054.

sharply angular instead of a flowing curve as in *D. borealis*. Lower jaw of practically the same size as in that species and dentition not specifically separable therefrom.

The inclusion in the same new species of type material not found associated is most unsafe. In this instance it seems justifiable because the collections made by two parties (American Museum and Princeton) have shown the presence of but one species of *Dromomeryx* in the Snake Creek beds, the so-called *Palæomeryx* of Matthew and Cook being undoubtedly *Dromomeryx* and not separable from the new species here described.

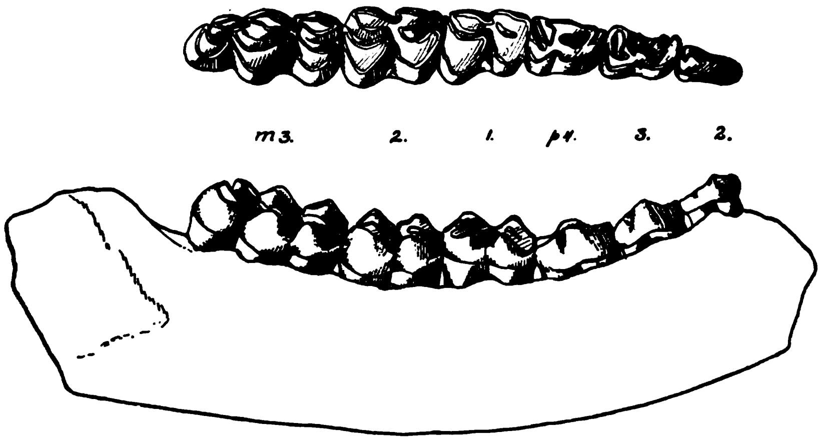

FIG. 18. *Dromomeryx whitfordi*, paratype, right ramus of the lower jaw, side view, and crown view of the teeth, two thirds natural size, No. 12086. The distance from p_2-m_3 is a little greater in the crown view, owing to elimination in the drawing of the fore-shortening due to curvature of dental series.

MEASUREMENTS.

Width of horn-base across middle of wing-like process	73
Anteroposterior diameter of beam three inches above base	30
Transverse diameter of beam three inches above base	25
Legnth, p_2-m_3 measured as chord of arc	109
Length, m_1-m_3	67½
p_2, anteroposterior 12⅓, transverse	6⅔
p_3, anteroposterior 14½, transverse	10
p_4, anteroposterior 15, transverse	10½
m_1' anteroposterior 17, transverse	14
m_2' anteroposterior 19½, transverse	15
m_3, anteroposterior 31, transverse	15
Depth of jaw beneath p_3	31
Depth of jaw beneath m_3	31½

PRINCETON UNIVERSITY, April, 1915.

EXPLORATIONS OVER THE VIBRATING SURFACES OF TELEPHONIC DIAPHRAGMS UNDER SIMPLE IMPRESSED TONES.

By A. E. KENNELLY and H. O. TAYLOR.

(*Read April 22, 1915.*)

The following research was carried on, at the Massachusetts Institute of Technology, under an appropriation from the American Telephone & Telegraph Co. during the year 1914–1915. The experimental work was carried out at Pierce Hall, Harvard University.

The object of the investigation was to explore the amplitude of the small harmonic vibrations of a circular diaphragm of telephonic type, clamped around the edge, and to compare the observed values with those which had been already deduced mathematically. Hitherto, so far as we are aware, the amplitude of vibration of a telephone diaphragm has been determined only at one point on the surface, usually the center,[1] The observations here reported differ from those heretofore obtained, in extending over the entire surface of the diaphragms.

Exploring Apparatus.

The exploring device, or "explorer," devised and constructed for this research, consists of a tiny triangular mirror fastened to a little phosphor-bronze stirrup strip, and having its point applied, by means of torsion in the strip, to the surface of the vibrating diaphragm at the point to be explored. The natural frequency of the mirror being much greater than that impressed on the diaphragm, the mirror is able to follow the vibrations of the latter, without breaking out of contact. The pressure exerted by the mirror on the diaphragm is so small as not materially to affect the diaphragm's vibration. A beam of light, reflected from the mirror on to a translucent scale, was thus set into vibrations synchronous with, and

[1] See Appended Bibliography, Nos. 2, 4, 7, 9 and 10.

proportional to, the vibrations of the diaphragm at the point of contact.

The vibration explorer is shown in side elevation at Fig. 1, in top view at Fig. 2, and in section, through center of the diaphragm, in Fig. 3. A fairly massive rectangular brass frame holds a plate sliding in grooves. The crank at the bottom of Fig. 1 controls this

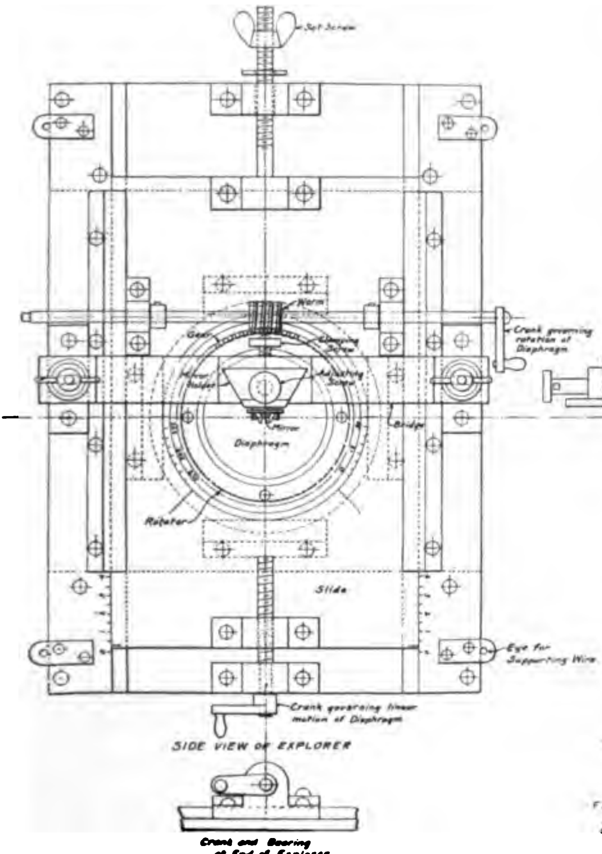

Fig. 1.

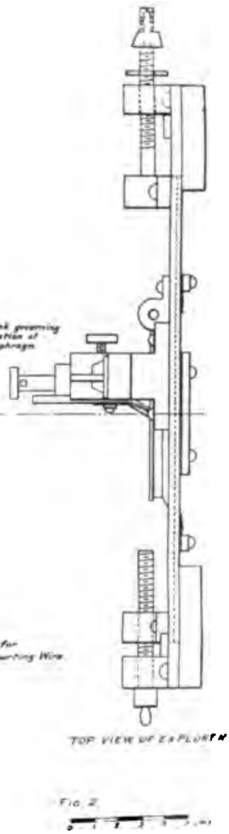

Fig. 2.

sliding motion, with the aid of the set screw at the other end. At the center of the sliding plate is a circular frame, into which is clamped the diaphragm to be tested. The circular frame can be rotated in its own plane by means of the crank at the right hand of Fig. 1.

A stout brass bridge is fastened to the sides of the rectangular frame. At the center of this bridge is the mirror-holder shown in detail at Fig. 3. The mirror-holder slides in a groove provided in the bridge, and is clamped therein by a clamping screw S. A fine-motion screw M is also provided, for adjusting the position of the mirror. One turn of M advances the mirror 0.8 mm. (1/32 inch). By means of an auxiliary mirror fastened beneath the top of the screw M, the angle through which the screw is advanced may be

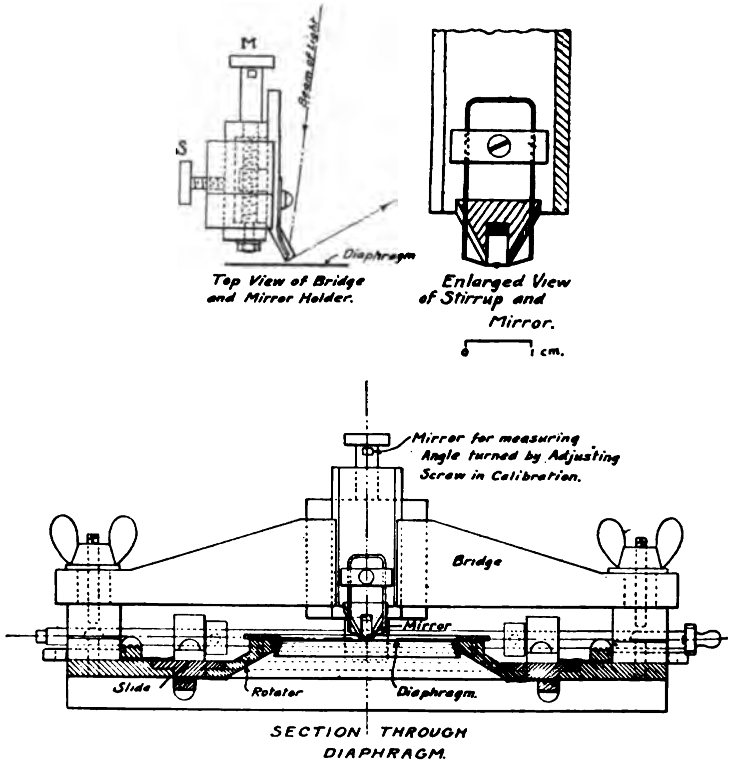

FIG. 3 APPARATUS FOR EXPLORING THE VIBRATION OF DIAPHRAGMS.

0 1 2 3 4 5 cms.

measured, for calibrating the indications of the instrument. Adjustment can be made to 1 deg. of rotation, or 2.2 μ; i. e. $\left(\frac{0.8}{360} \text{ mm.}\right)$ assuming that backlash is guarded against.

The construction of the apparatus is such, that the mirror is held at all times at the center of the brass rectangular frame; while by means of the two crank adjustments, the diaphragm to be explored can be moved so as to bring any part of its surface beneath the mirror. With the aid of the scales of distance and angle shown in Fig. 1, the position of the mirror with respect to the diaphragm can be adjusted and read off to polar coördinates (r, θ). The motion in r is controlled by the crank at the bottom, to 0.1 mm.; while the angular motion in θ is controlled by the crank at the side, to $1°$, or less if desired. The slide is held in position by flat springs, attached to the rectangular frame, so as to keep the motion of the slide confined to its own plane. A similar construction is used with the circular frame. It is important that the plane of the diaphragm shall not be disturbed when either crank is operated. The weight of the whole explorer is 4.63 kgs. (10.2 lbs).

A magnified view of the mirror, and its stirrup frame, is shown at the top of Fig. 3. The mirror, of silvered glass, about 0.1 mm. thick, is cut in the shape of an equilateral triangle, about 1 mm. in length of side. One vertex of the mirror is applied to the surface of the diaphragm, and the mirror is fastened with sealing wax across a thin phosphor-bronze strip. This strip is approximately 3 mm. long between abutments, 0.02 mm. wide, and 0.013 mm. thick. The weight of the mirror is about 1 milligram, without varnish or sealing wax. Its natural frequency of vibration, as obtained photographically, is about 2,500 $\sim$. These little mirrors are apt to break off the stirrup strip; so that they have to be renewed and recalibrated occasionally. The pressure exerted on the diaphragm by the point of the mirror, as measured by an auxiliary test, is approximately 200 dynes (204 mgm. wt.). A pressure of this order seems to be desirable, so as to obtain a natural frequency of 2,500 $\sim$. If, however, explorations are confined to lower diaphragm frequencies, the natural frequency of the explorer mirror, and its pressure on the diaphragm, may be reduced accordingly.

The diaphragm to be explored is 5.4 cm. in diameter, and is placed in the circular frame. It is clamped tightly into this frame, with the ring clamp shown in Fig. 3, which had a radius of 2.62 cm. when no auxiliary clamping rings were used. The vibration explorer

is then suspended on wires from the ceiling, or other convenient support, in order to suppress building vibrations of high frequency; so as to support the explored diaphragm in a vertical plane. The mirror-holder is then advanced towards the diaphragm, and clamped by screw *S*. The mirror is now carefully brought into contact with the surface of the diaphragm by adjusting screw *M*. A picture of the explorer is presented in Fig. 4. The suspension wires *ww*,

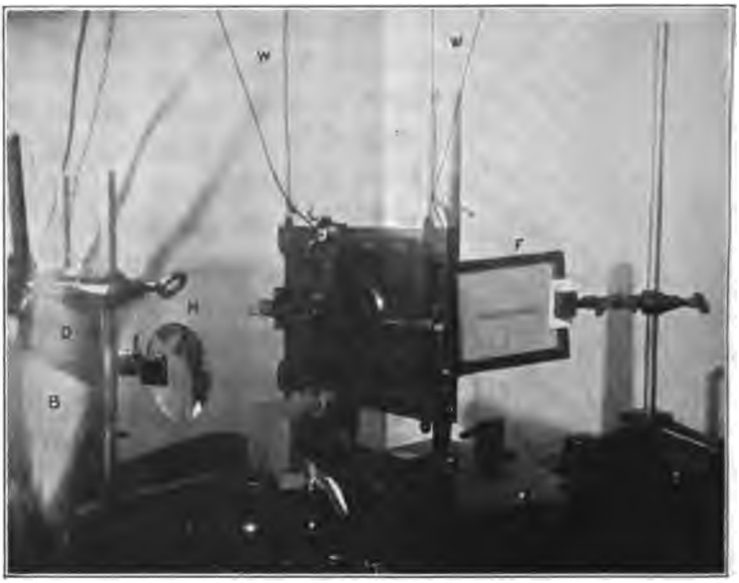

FIG. 4. Vibration Explorer in Booth.

support the instrument. The condensing and focusing lens *H* throws a narrow arc-light beam upon the exploring mirror, which reflects it on to the translucent graduated screen *F*. With the diaphragm at rest, the spot on this screen is a narrow, sharp, vertical, luminous strip. When the diaphragm is set in vibration, the mirror in contact with it vibrates synchronously, and the spot is spread into a luminous band, the limits of which are easily read on the graduated translucent scale. If the motions of diaphragm and mirror are simple harmonic motions, the luminous band shows no discontinuities of intensity. If, however, there is a complex harmonic motion in the diaphragm, the luminous band will show bright and dark

patches, either quiescent, or with beats. By means of the optical magnification of amplitude that can be effected with such an exploring mirror and scale, diaphragm vibrations of amplitude 0.1 μ (*i. e.*, 10^{-5} cm.), or less, can be observed; although the precision of measurement falls off considerably, for a diaphragm amplitude below 0.5 μ (half a micron).

Optical System.

The optical system employed with the vibration explorer is diagrammatically indicated in Fig. 5. The stereopticon arc-lamp *A* throws a powerful condensed beam of light on the pinhole *B*, in a brass vertical screen. A set of small powerful collimating lenses *C*

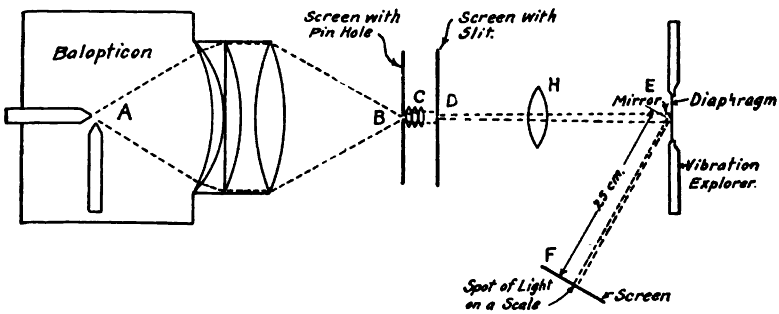

Fig. 5. Diagram of Optical System used with Vibration Explorer.

throws the nearly paralleled beam through the screen and slit *D*, as well as the focussing lens *H*, on the exploring mirror *E*, whence it is reflected to the translucent screen *F*, at a convenient distance, in this case 25 cm. An image of the slit in screen *D* is then sharply focused at *F*. In Fig. 6, it is indicated geometrically that the amplitude e of the diaphragm's displacement is equal to the continued product of the observed amplitude d of the luminous band, the ratio of l (the radius arm of the mirror), to $2L$ the double distance of the mirror from the screen, and the cosine of the angle ϕ between the radius arm of the mirror and the plane of the diaphragm. In order to avoid frequent changes in ϕ, it is desirable to keep constant the zero of the spot at the center of the graduated scale *F*, and with it the contacting angle of the mirror. The numerical

expression $M = 2L/l \cos\phi$ may be called the ***magnification-factor*** of the explorer. As ordinarily employed, $2L = 50$ cm., $l = 0.05$ cm., $\phi = 45°$ approximately, or $\cos\phi = 0.7$; so that $M = 1,400$ approximately, varying in different sets of measurements between

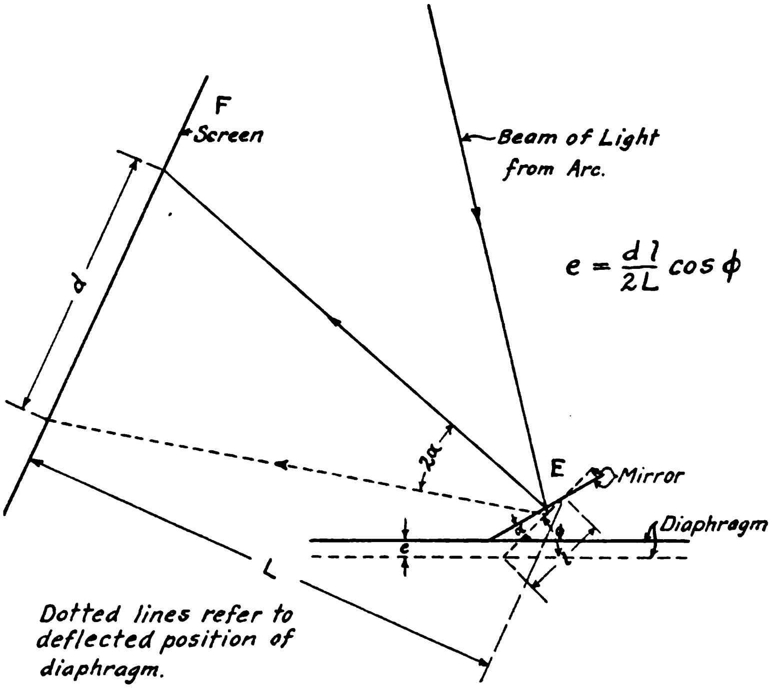

FIG. 6. Diagram Showing Action between Mirror and Diaphragm in Explorer.

800 and 1,500. Had it been necessary, this magnification-factor might have been considerably increased, by increasing the distance L between mirror and scale; although the reduction in luminous-spot intensity, at increasing ranges, prevents the precision of the observations from increasing in the same proportion as the magnification-factor. At $L = 25$ cm., the amplitude of luminous band could be read to 0.1 mm. on the graduated translucent scale F.

SOURCE OF DIAPHRAGM VIBRATIONS.

Two sources of vibrations were used in different series of tests, (1) acoustic, (2) electromagnetic.

(1) The acoustic vibrations were supplied from one of a series of small organ-pipes, giving fairly simple musical tones between C_2 of 128 $\sim$, and C_6 of 2,048 $\sim$. The organ-pipe selected was mounted vertically in a block on the table, at the back of the vibration explorer, and supplied with air at constant pressure (about 18 cm. of water) from a pneumatic tank. The whole apparatus was placed inside a sound-damping wooden-frame booth (274 cm. × 183 cm. × 214 cm. high), lined on the inside with hair-felt, 2.5 cm. thick, surfaced with thin cloth. The observer, after turning on the air to the organ-pipe, observed the amplitude of the luminous band on the translucent screen *F*, Figs. 4 to 6, as the mirror was applied to different successive points on the diaphragm.

(2) The clamping ring of the diaphragm in the explorer was chosen of such dimensions that a standard telephone receiver could be substituted for it. In this case, a steel diaphragm had to be employed. The telephone was then operated by a feeble measured alternating current (2.0 milliamperes) obtained from a Vreeland mercury-arc oscillator having a frequency adjustable, by successive steps, between 430 $\sim$ and 2,500 $\sim$.[2]

Exploration with Diaphragm No. 1.

Diaphragm No. 1 was a telephone-receiver diaphragm of steel, japanned on one side. Its dimensions are given in Table III. The diaphragm was clamped, around the boundary, between opposing circular knife-edges.

TABLE I.

Vibration Amplitudes over Diaphragm No. 1, at Frequency 608 $\sim$, for Nine Different Azimuths θ, and Seven Different Radial Distances r.

Radial Distance, r Cm.	Vibration Amplitude Observed with Explorer (Microns) at Different Azimuths θ.								
	0° μ	40° μ	80° μ	120° μ	160° μ	200° μ	240° μ	280° μ	320° μ
−0.08	13.8	12.1	11.7	14.0	13.4	10.4	10.8	13.3	12.0
+0.31	12.7	10.9	11.3	12.3	12.7	9.7	10.6	12.6	11.6
+0.69	9.7	8.0	8.9	10.4	10.4	8.1	8.8	11.5	9.5
+1.06	6.6	5.9	6.4	6.8	7.0	6.2	6.4	7.1	6.8
+1.44	4.2	3.6	4.2	4.7	4.5	4.1	4.2	4.9	4.3
+1.82	2.5	2.2	2.1	2.5	2.5	2.3	2.4	2.5	2.4
+2.20	0.9	0.8	0.9	0.9	0.9	0.7	0.7	0.9	0.8
+2.54	0	0	0	0	0	0	0	0	0

[2] See Bibliography No. 5.

An organ-pipe of $D_4^\sharp$ (608 ~) was set up with its lip 5 cm. from the back of the diaphragm. An exploration was then made over the surface, at points differing by 40° in azimuth θ, and at successive increases in radius of about 3.3 mm. (7 steps in r, and 9 steps in θ, or 63 observations in all.) The preceding table gives the observed amplitudes of vibration deduced from the scale-deflections, with a magnification factor of $M = 1,180$.

It will be seen from the above table, that at any particular radius r, measured from the center of the diaphragm, the amplitudes at varying azimuths θ are substantially equal. The irregularities are small, but nevertheless seem larger than can be accounted for by errors in observations and are, perhaps, due to irregularities in the diaphragm. Fig. 7 shows the contour lines of vibration-amplitude in microns, the maximum amplitude being at or near the center, and amounting to 14μ. Such vibration amplitudes are larger than were usually obtained, and were specially reinforced in this case, in order to secure large deflections. It will be seen from the contour diagram, that the diaphragm was vibrating with its fundamental or gravest mode of motion; *i. e.*, a motion to-and-fro as a whole, without either nodal diameters or nodal circles. It is known that a circular diaphragm, clamped at the edge, is capable of vibrating in an indefinitely large number of ways, according to the

FIG. 7.

VIBRATION AMPLITUDE IN MICRONS. 608 ~ DIAPHRAGM No. I.

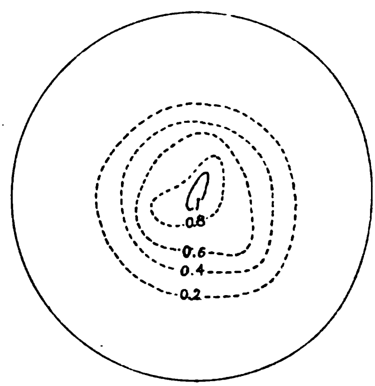

FIG. 8.

VIBRATION AMPLITUDE IN MICRONS. 2100 ~. DIAPHRAGM No. I.

number of nodal circles, and also according to the number of nodal diameters present.[3]

A similar exploration was made over the diaphragm, with acoustic excitation from an organ-pipe giving C_6 (2,100 ~). Here the points of observation were in steps of about 3.3 mm. in r, and in steps of 40° in θ, as before, with magnification-factor, $M = 1{,}265$.

TABLE II.

VIBRATION AMPLITUDES OVER DIAPHRAGM NO. 1, AT FREQUENCY 2,100 ~, FOR NINE DIFFERENT AZIMUTHS θ, AND SEVEN DIFFERENT RADIAL DISTANCES r, FIVE ONLY GIVING READABLE DEFLECTIONS.

Radial Distance, r cm.	Vibration Amplitude Observed, with Explorer, at Different Azimuths θ.								
	0° μ	40° μ	80° μ	120° μ	160° μ	200° μ	240° μ	280° μ	320° μ
−0.08	0.70	0.95	0.95	0.95	0.87	1.02	0.95	0.87	0.95
+0.31	0.70	0.87	0.87	0.78	0.78	1.02	0.87	0.78	0.87
+0.69	0.62	0.78	0.78	0.62	0.62	0.78	0.62	0.62	0.78
+1.06	0.31	0.39	0.39	0.39	0.39	0.39	0.39	0.39	0.62
+1.44	0.16	0.16	0.16	0.08	0.08	0.08	0.08	0.08	0.23
+1.82	—	—	—	—	—	—	—	—	—
+2.20	—	—	—	—	—	—	—	—	—
+2.54	0	0	0	0	0	0	0	0	0

The vibration contour-lines for this case are given in Fig. 8. Here again it is seen that, setting aside irregularities in the diaphragm, and allowing for errors of observation (which are more noticeable with the small amplitudes of higher pitch), the mode of vibration is essentially fundamental, since there are no perceptible nodal circles or nodal diameters.

Having thus ascertained that both at pitch $D_4^{\sharp}$ (608 ~), and at C_6(2,048 ~), the first mode of vibration was presented, a series of careful explorations were made at a number of intermediate pitches. These likewise all showed the first or fundamental mode of vibration. See Table II*A*.

Observations were also made, at organ-pipe frequencies down to 128 ~. Explorations would be very difficult to obtain on this diaphragm at such low frequencies, owing to the small vibration amplitudes produced; but the indications were that the fundamental mode of vibration was maintained throughout.

[3] See Appendix 1.

The conclusion, therefore, seems warranted that, for this particular steel telephone diaphragm, acoustically excited to frequencies as high as 2,100 ~, the fundamental mode of vibration is the only one that is maintained. If any higher modes of motion were present, they were too faint to be discerned. This does not mean that higher modes of motion could not be produced by any kind of excitation within the above ranges of frequency. The effects of very powerful vibrations were not investigated.

Since the natural frequency of this diaphragm, with flat clamping, was observed to be $n_0 = 824$ ~, and since, according to Bessel-Function theory, the natural frequency of the second mode of motion should be $2.09n_0$, we should naturally expect to find this second mode of motion appearing at and above 1,720 ~. Its non-appearance may have been due to the uniformity of acoustic impressed force over the surface, which would tend to favor the first rather than the second mode of forced vibration.

The vibration-amplitude of the diaphragm was found to vary widely with the pitch of the exciting source. At or near the natural

TABLE II*A*.

SHOWING FUNDAMENTAL MODE OF VIBRATION MAINTAINED FOR A RANGE OF OF FREQUENCIES FROM 400 ~ TO 1,800 ~.

Amplitudes of Vibration in Microns (μ) along Radius of Diaphragm No. 1, Flat-Clamped. $n_0 = 704$ ~.

Radial Distance, r Cm.	Frequency of Vibration.						
	400 ~ μ	500 ~ μ	750 ~ μ	1,000 ~ μ	1,250 ~ μ	1,500 ~ μ	1,800 ~ μ
.04	.8	1.6	7.8	1.3	1.3	.9	.3
.29	.8	1.6	7.7	1.3	1.2	.9	.3
.54	.7	1.5	7.2	1.1	1.1	.8	.3
.79	.6	1.4	6.3	.9	.9	.6	.2
1.04	.5	1.3	5.3	.8	.8	.5	.2
1.29	.4	1.0	3.8	.5	.6	.3	.1
1.55	.2	.7	3.0	.3	.4	.2	+
1.79	.1	.4	1.9	.2	.2	.1	+
2.04	+	.3	1.2	.1	.1	+	−
2.30	−	.1	.8	−	+	−	−
2.65	0	0	0	0	0	0	0

fundamental frequency of the diaphragm, the amplitude of the vibratory response was a maximum. Either above or below this

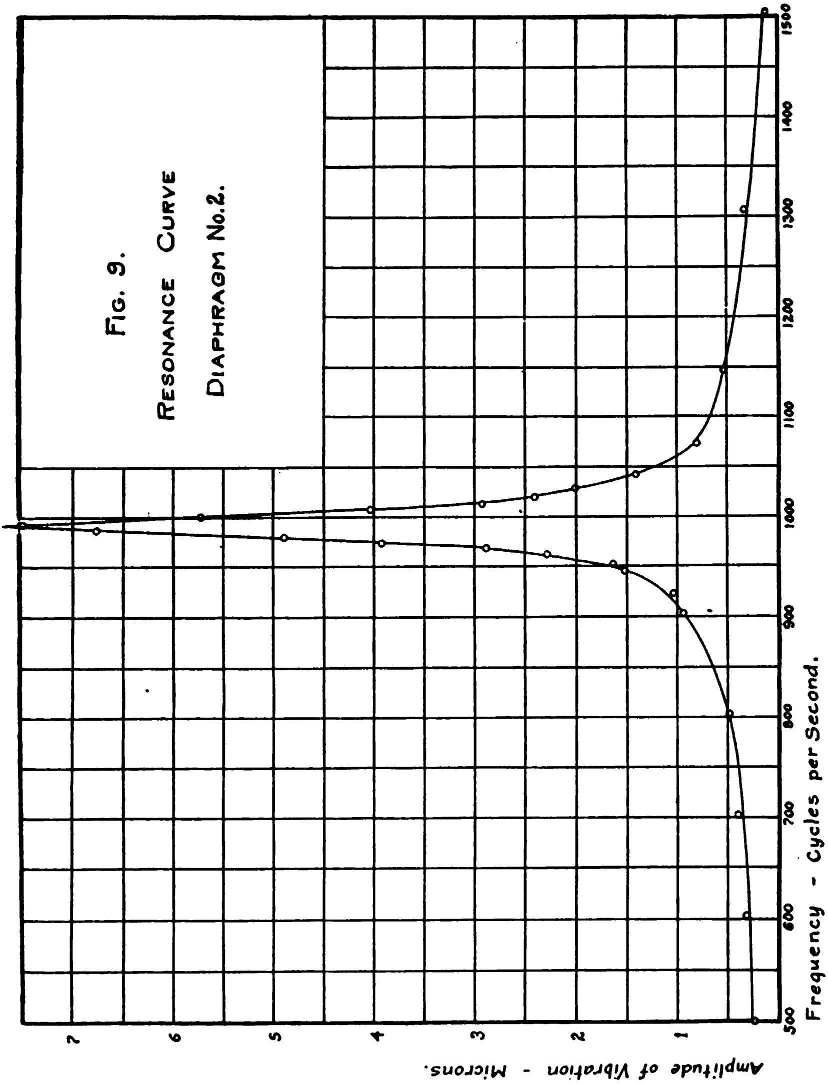

Fig. 9.
Resonance Curve
Diaphragm No. 2.

resonant frequency, the amplitude of vibration, shown by the explorer, fell off very markedly. The curve of relative amplitude at different frequencies is indicated in Fig. 9. It will be seen that when exciting the diaphragm with vibrations remote from the resonant frequency in either direction, the amplitude becomes so small that the degree of precision which may be obtainable near resonance is impossible to secure. The outline theory for this resonance curve, Fig. 9, is given in Appendix II. It is shown that if we multiply the successive ordinates by $\omega = 2\pi n$, the resulting velocity-values correspond to vector chords on a certain velocity circle.

Fig. 1*B* of Appendix I. gives the graph of the explored vibration amplitudes, at successive radial distances from the center of diaphragm No. 1, for the frequency 896 ~. It will be seen that the amplitude falls off smoothly from a maximum at or near the center ($r = 0$), to zero at the flat-clamped edge ($r = 2.62$). The application of Rayleigh's theory of free vibration to these curves is given in Appendix I. In general, the agreement between the acoustically forced amplitudes and theoretically computed free amplitudes was satisfactory.

At or near the resonant frequency, or natural frequency of a diaphragm, especially when its damping coefficient is small, so that the resonance is sharp, a small change either in impressed frequency, or in the constants of the diaphragm due to change of temperature, may have an appreciable influence upon the amplitude of vibration. In other words, although the observed amplitudes are relatively large, and the precision of measurement is seemingly high, yet the system is in a virtually unstable condition. Consequently, although there is no reason to suppose that the conditions at resonance differ from those off resonance, nevertheless, when a reliable and reproducible set of observations of amplitude distribution is desired, it is advisable to select a frequency not too close to resonance, or say of about half the resonant amplitude.

Application of Circular Velocity-Diagram Theory to Results of Explorations.

It is shown in the first-approximation theory of Appendix II., that the behavior at the center of a flat-clamped circular diaphragm, subject to constant vibro-motive force of varying frequency, can be completely predicated, if three constants of the diaphragm are known;[4] namely,

(1) the "equivalent mass" m (gm.),

(2) the elastic constant s (dynes per cm. of displacement at center),

(3) the mechanical resistance r (dynes per unit velocity at center).

All these three constants can be obtained, for an acoustically excited diaphragm, with the aid of the vibration explorer.

Determination of m.

In order to determine the equivalent mass of a diaphragm, it is necessary to know the distribution of amplitude over the entire vibrating surface. As is shown in Appendix III., when the distribution of amplitude conforms regularly with the Rayleigh formula, it would appear that the equivalent mass is 0.183 times the mass of the circular vibrating plate. If, however, the distribution of amplitude is irregular, such as may be produced by bipolar electromagnetic excitation of a telephone-receiver diaphragm, the coefficient 0.183 cannot be depended upon, and the proper coefficient must be determined by some process of quadrature, such as Appendix III. describes.

The Elastic Constant s.

The constant s is the inferred elastic resisting force, which, acting perpendicularly upon the diaphragm's equivalent mass (at its center), would produce the same effect upon the vibratory motion as the distributed elastic forces produce upon the diaphragm's distributed mass, in the presence of the particular impressed force distribution. The simplest way to find s is to measure the natural fundamental frequency n_0 of the diaphragm, by exciting it with an

[4] See Bibliography No. 8.

organ-pipe of adjustable pitch, tuned to produce the maximum vibratory amplitude at the center. As shown in Appendix II., the constant s is then the product of the equivalent mass m and the square of the resonant angular velocity ω_0.

A series of statical measurements were made, by applying small tensions f_s, by means of a calibrated spring, to the center of the diaphragm, and observing, with the aid of the explorer, the central displacements w_s, thereby produced. It was found, as might be expected, that the ratio of f_s to w_s was constant, so long as the latter did not exceed 18μ. Moreover, the value of s obtained from f_s/w_s was approximately the same as that obtained from formula (9), App. II. This static method of finding s, however, is inferior to the resonance method, because precise static measurements are difficult to obtain. The application of electro-magnetic excitation to a steel diaphragm also imposes residual stresses, which make the use of the static method unreliable.

The Mechanical Resistance r.

The constant **r** was measured, with the explorer, by photographing the decay curve of vibration amplitude on a moving photographic film, when the diaphragm was tapped at the center, and allowed to return to the equilibrium position under its own damping forces. It is shown in Appendix II., that the resistance **r** is twice the natural frequency multiplied by the equivalent mass and the logarithmic decrement. Fig. 10 is a tracing from a photograph of

Fig. 10. Tracing from Photograph of Decay Curve. Diaphragm No. 1.

the curve of decay. A small camera, represented in Fig. 11, was set up in front of the explorer, containing a photographic film wrapped around a metal drum. The drum was motor driven at a peripheral speed of approximately 4 meters per second, and the shutter was opened at the time of tapping the diaphragm. The logarithmic decrement of this curve is 0.184, at the frequency of 824 ~; so that with an equivalent mass m of 1.09 gm. the value of

r becomes 328 dynes per cm. per sec. The precision in measuring r by this method is relatively low, owing to the difficulty in measuring the successive amplitudes with accuracy, on a curve of such small dimensions.

Since, as is shown in Appendix II., a circular diaphragm, in its fundamental mode of motion, ordinarily develops a circular graph of velocity, at varying impressed frequency, with constant vibro-

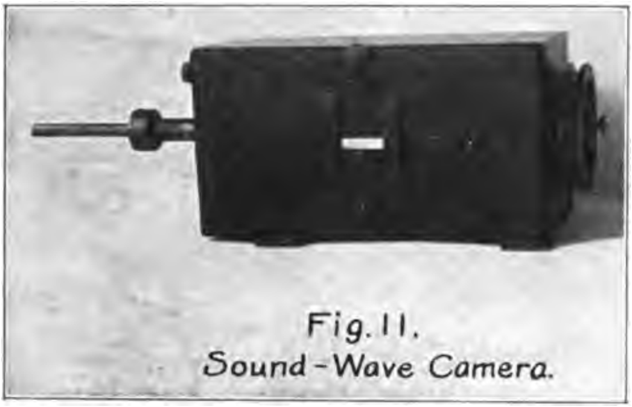

Fig. 11.
Sound-Wave Camera.

motive force, the plan has suggested itself, in the course of this research, to use the circle-velocity diagram of a diaphragm for comparing the vibro-motive forces (vmf.'s) of different organ-pipes. In this connection, the vmf. of a pipe at the exploring diaghragm, may be defined as its harmonically varying pressure $f = F\epsilon^{i\omega t}$ (dynes) produced, at the diaphragm, by the pipe, under the geometrical conditions of the system, including acoustic reflections from walls, or other objects in the room, on both surfaces of the exploring diaphragm. In the simplest, or standard, geometrical condition, the *standard* vmf., which is proportional to the square root of the sound intensity[5] at the diaphragm, would be observed in free space, with the orifice of the pipe facing the diaphragm at a definite distance, and with the diaphragm perpendicular to the line joining them. It is our understanding that there is, as yet, no simple published method of measuring the vmf. of organ pipes, of different

[5] Bibliography 6, Barton, "Text Book of Sound," p. 211, par. 146. Macmillan Co., 1908.

sizes or pitches, at a definite distance from their orifices. If, in a given geometrical environment, pipes of different pitches are set up, in succession, at the same position with respect to the exploring diaphragm, then the observed amplitudes, multiplied by the respective values of ω, should lie on the velocity circle-diagram, if the vmf.'s of the pipes are the same; assuming that the fundamental mode of vibration is produced, that the constants of the diaphragm remain unchanged, and that the overtones of the pipes are negligibly small. The vector departures from the circle diagram would then indicate the inequalities in vmf.'s.

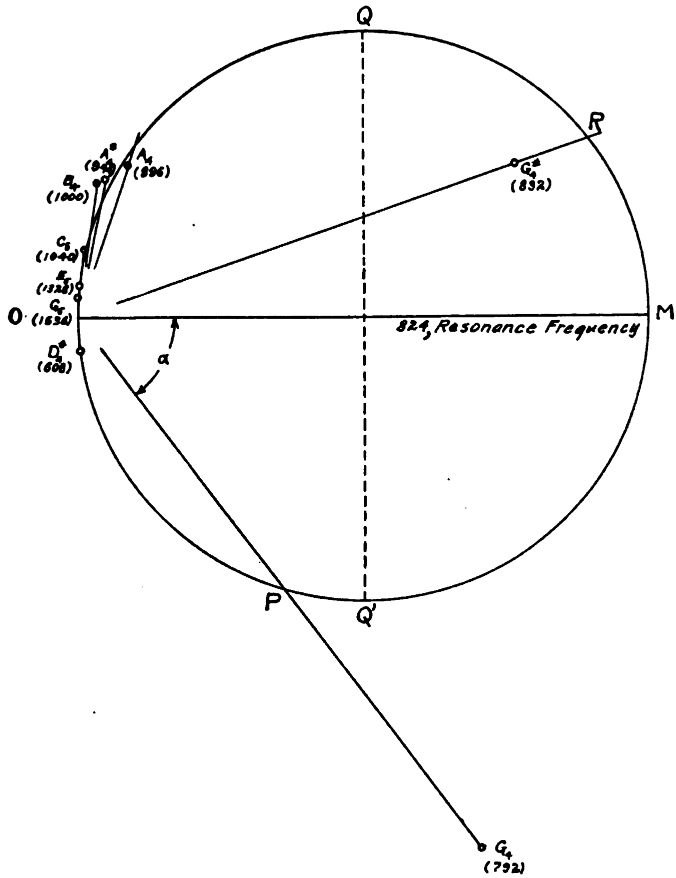

FIG. 12. Diagram Showing Strengths of Organ Pipes Given by the Vibration of a Diaphragm.

Fig. 12 is an inverted velocity-circle diagram for Diaphragm No. 1, based upon its measured values of m, r and s. If we take the diametral velocity OM as 5 cm. per sec., with r = 328 dynes per cm./sec., then the vmf. which, in the particular environment of the experiment, produced this velocity, would be 1,640 dynes, maximum cyclic value. The particular pipe G_4(792 ∼), gave an observed amplitude at the diaphragm center, which, multiplied by $\omega = 2\pi \times 792$, gives the line OG_4 along the chord OP. The phase-angle α must be obtained by considering the mechanical reactance as in (4), App. II. If the vmf. of this pipe were the same as that which produced OM, this point G_4, would lie on the circle. Consequently, the vmf. of the pipe G_4 is to that of the pipe producing resonance, in the ratio OG_4/OP. Similarly, the vmf. of the pipe $G_4^\sharp$ (832 ∼), is less than that producing the resonant velocity, in the ratio $OG_4^\sharp/OR$. It is evident that the range of any one diaphragm, for the precise comparison of vmf.'s from organ-pipes of different pitch, is somewhat limited. In the case presented, it would not exceed one octave, since the chords far from the resonant diameter become so short. By selecting a diaphragm of relatively large damping constant $\Delta = r/2m$, this range can be increased. In fact, the range in ω between the quadrantal points QQ' on the velocity circle, is numerically equal to r/m, or twice the damping constant.

A succession of calibrated diaphragms with overlapping ranges might be employed to cover the musical scale. The writers have not attempted to compare organ-pipes for standard vmf. in this manner. The measurements might have to be made out-of-doors. In the sound-absorbing room in which this research was carried on, the effect of sound reflections from walls and other objects prevented any standard comparisons of vmf. from being made.

Explorations with Electromagnetically Excited Diaphragms.

In order to ascertain the effects of exciting a steel diaphragm (No. 2) electromagnetically, a No. 144 Western Electric Bell telephone receiver was screwed into the explorer, behind the diaphragm, so as to obtain the ordinary air-gap between the diaphragm and its two poles. The cap or screw-cover of the ordinary telephone re-

ceiver was here absent. Alternating current of 2 milliamperes (root-mean-square) was supplied from a Vreeland oscillator, giving a close approximation to a pure sine wave, and in connection with a Rayleigh bridge, for the simultaneous measurement of both the resistance and inductance of the telephone receiver, at 32 frequencies varying between 429 and 2,040 ~. Explorations were

VIBRATION CONTOURS
DIAPHRAGM No. 2.

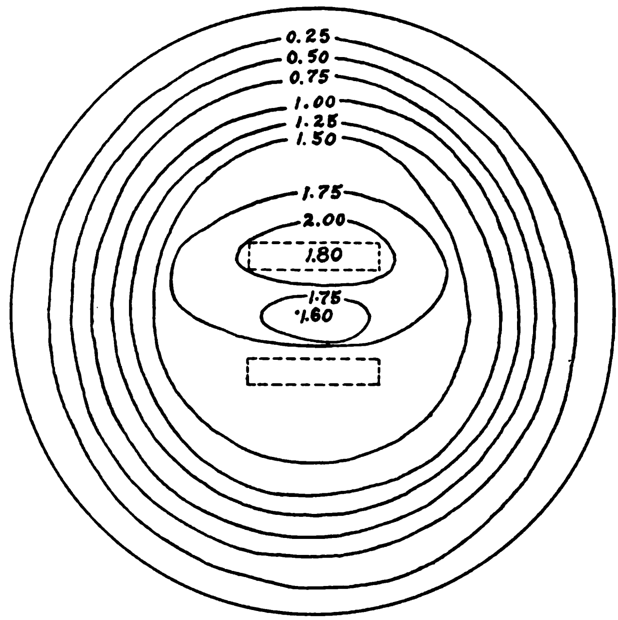

FIG. 13.

made at two frequencies; one, the resonant frequency of 992 ~, and the other slightly below this, or 974 ~. The contour lines for the latter case are presented in Fig. 13, where the outlines of the

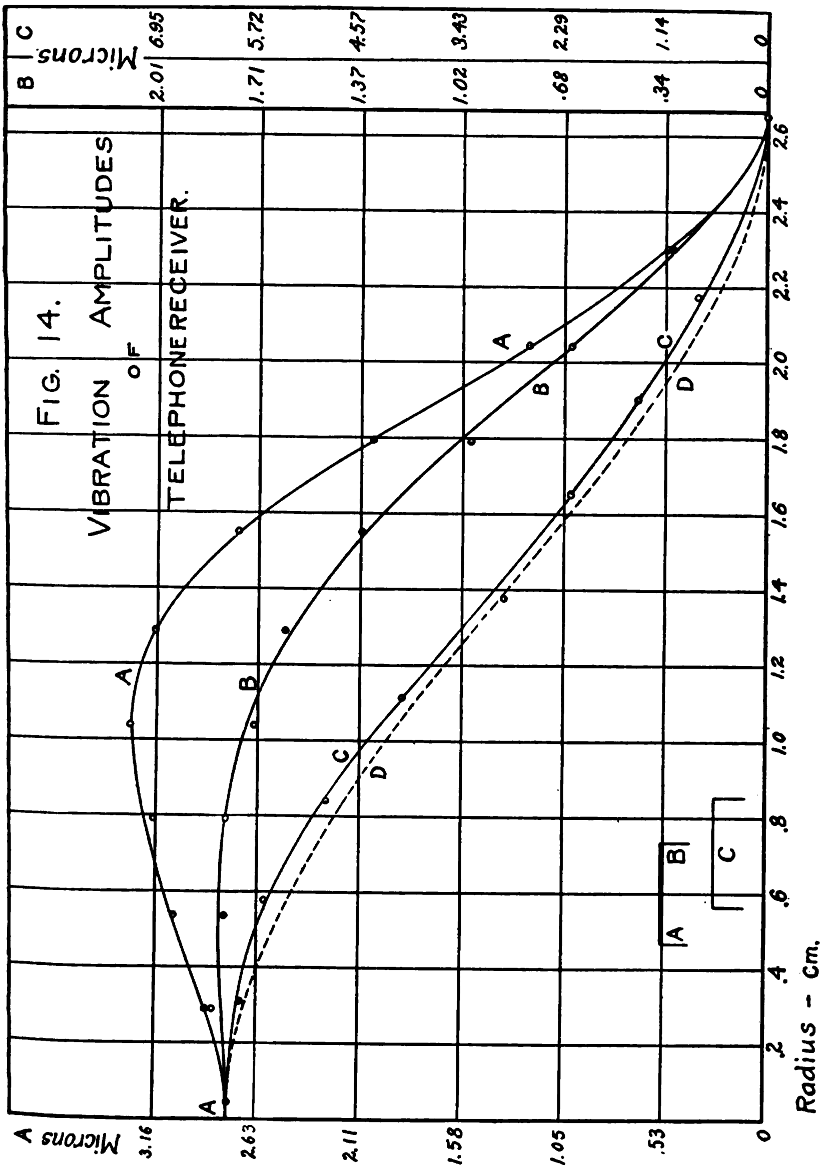

FIG. 14. VIBRATION AMPLITUDES OF TELEPHONE RECEIVER.

two magnetic poles are indicated in dotted lines. It will be seen that while the mode of motion is essentially fundamental, the amplitude is not a maximum at the center, as in the ordinary acoustic case. The maximum amplitude of $2.0\,\mu$ is reached in an elliptical loop embracing the pole at the top. Inside this loop, and immediately over the pole, the amplitude falls off to $1.8\,\mu$. Over the pole underneath, the amplitude is about $1.7\,\mu$, but there appears to be a slight diminution between the poles. If the geometrical and magnetic conditions of the bipolar system were perfectly symmetrical, these dissymmetries would presumably disappear.

The curves of mean amplitude against radial distance are presented in Fig. 14. The curve *AAA* corresponds to that found at resonance, and shows that the amplitude is far from being a maximum at the center of the diaphragm, owing to the attractive forces being established over polar areas on each side of the center. The coefficient of equivalent mass for this curve is over 0.5.

The curve *ABB* gives the corresponding distribution of mean azimuthal amplitude for the frequency of 974 ∼. The swelling of the amplitude over the poles is less marked in this case, and does not materially exceed that at the center. The equivalent mass coefficient for this curve is 0.36, or about double that for the Rayleigh-Bessel curve case, which is indicated by *ADD*. The curve *ACC* gives the distribution of mean amplitude in radial distance, for another steel diaphragm (No. 3) in a bipolar telephone receiver, at the resonant frequency of 1,020 ∼.

For both steel diaphragms Nos. 2 and 3, a series of central amplitude measurements were made, with the explorer, at constant alternating-current excitation, but adjustably varied frequency. Simultaneous measurements were made by Mr. H. A. Affel, of the resistance and inductance of the telephone-receiver coils, with the diaphragm both free and damped. The explorer measurements in both cases satisfactorily checked the electrically deduced velocity-circle diagrams. It is proposed to report upon the electrical measurements in another paper. Moreover, starting with the amplitudes, measured at the center of the diaphragm, in curves *A* and *C* of Fig. 14, the equivalent masses of the diaphragms, computed from the electrical measurements, agreed, within a few per cent., with those found by integrating curves *A* and *C*.

Temperature Effects.

It was found that changes of temperature in the air surrounding a diaphragm had a marked effect, both upon its resonance frequency, and upon its amplitudes at any frequency. The curves representing w against r, were apt to differ appreciably in outline from day to day. The degree of tightness of clamping also had a marked effect in these measurements. In general, such disturbances due to temperature and clamping, are likely to introduce tensions in the substance of the diaphragm, and to cause some of the characteristics of vibrating membranes to be superposed upon those of a vibrating plate. It is, therefore, desirable that the clamping should be effected tightly, and that the measurements should then be made before the temperature has changed. Strictly speaking, the Rayleigh theory shows that there must be a marked difference in both the resonance frequency and in the distribution of amplitudes, if the diaphragm is clamped between circular knife edges, instead of between circular flat rings at the boundary. The experiments have shown that flat-ring clamping is more likely to give consistent results than knife-edge clamping. These clamping difficulties are accentuated in thin glass diaphragms, for the boundary supporting of which, a special technique had to be developed.

Exploration of Thin Glass Diaphragms.

From a number of thin glass diaphragms, one Diaphragm No. 4, was selected, on account of its uniformity in thickness. See Table III. It was found very difficult to obtain uniform results with this in the explorer, owing to the above mentioned troubles with clamping. Finally, the glass diaphragm was cemented, with water glass, to a boundary ring of glass, and this was lightly supported between the clamping rings of the explorer. The diaphragm was then excited acoustically by organ-pipes. The natural pitch of the diaphragm was found to be 492 ~, in the fundamental mode. On raising the frequency, the mode of motion was found to change suddenly, at 968 ~, to that of a single nodal diameter, the two halves of the diaphragm then vibrating harmonically in opposite phases. This mode of motion continued until the frequency reached 1,696 ~,

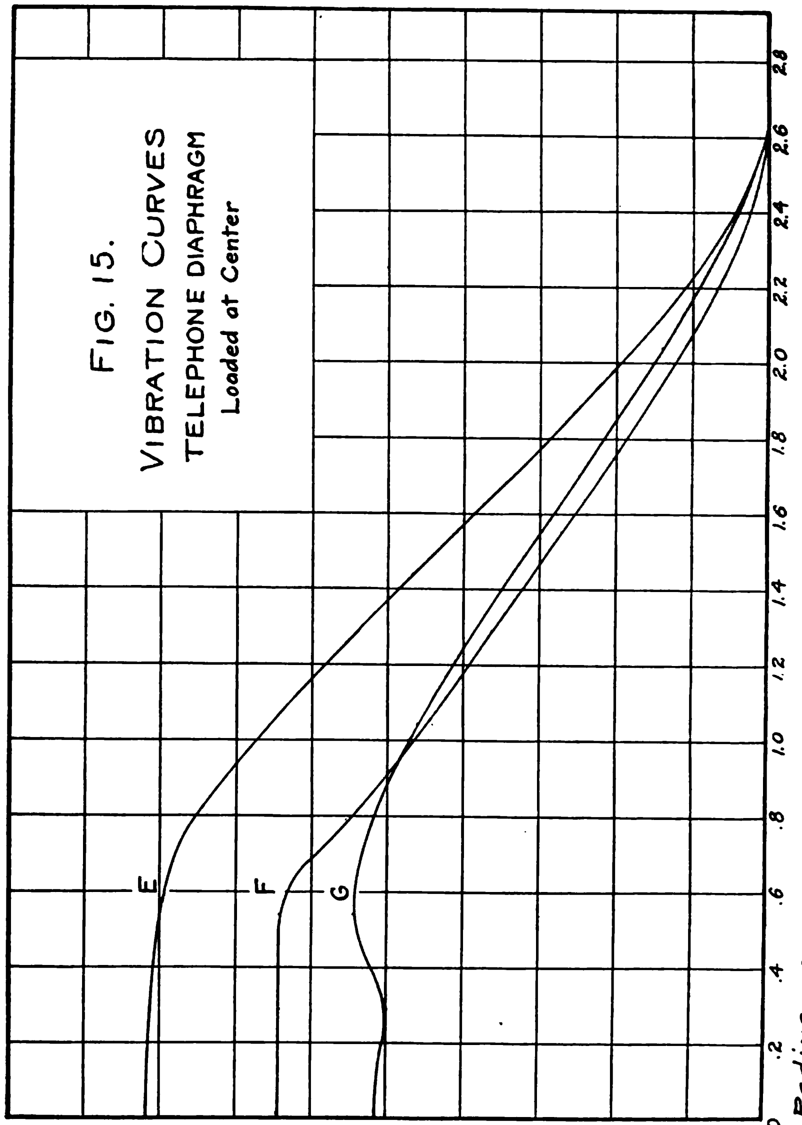
FIG. 15.
VIBRATION CURVES
TELEPHONE DIAPHRAGM
Loaded at Center
E
F
G
0 .2 .4 .6 .8 1.0 1.2 1.4 1.6 1.8 2.0 2.2 2.4 2.6 2.8
Radius - cm.
1 2 3 4 5 6 7 8 9
Amplitude - microns.

when the nodal diameter disappeared and gave place to a single nodal circle. The ratios of the above three frequencies are 1 : 1.97 : 3.44; whereas, according to the Bessel-function theory, they should be 1 : 2.09 : 3.91. The discrepancies may readily be accounted for by imperfections in boundary support, or by temperature effects. Small changes in clamping were found to exercise a marked influence on these ratios.

Loading of Diaphragm.

In the determination of m, r and s, by electrical impedance measurements,[6] only two quantitative relations between these three constants naturally present themselves; whereas, for the evaluation of these three unknowns, three independent quantitative relations must be experimentally obtained. It had been hoped to derive the missing third equation, by applying a small known load-mass at the center of the diaphragm, and by repeating the electrical measurements with this load in place. Electrical experiments showed, however, that while, occasionally, consistent results were obtained in this way, more often the results were discordant. The reason for the discordance has been shown, from explorations of the diaphragm, to be due to a distortion of the amplitude curves; whereby the equivalent mass of the loaded diaphragm is no longer the same as when unloaded.

These conditions are exhibited in the curves of Fig. 15. E shows the w, r curve, for an unloaded telephonic steel diaphragm, excited acoustically at $n = 904 \sim$, its natural frequency being $n_0 = 832 \sim$. The corresponding curve F is for the same diaphragm, after being loaded at the center by a small brass cylinder of 0.536 gm. at $n = 816 \sim$, its new natural frequency being $n_0 = 696 \sim$. After increasing the load to 1.08 gm., the new curve is shown at G ($n = 660 \sim$, $n_0 = 616 \sim$). The shapes of these three curves E, F and G, being so different, it is evident that the equivalent mass of the diaphragm by itself cannot be regarded as constant.

The authors are indebted to Dr. Geo. A. Campbell for a number of valuable suggestions which he made after having read the MSS. of this paper; also to Professor W. C. Sabine for very useful suggestions, during the course of the research.

[6] Bibliography No. 8.

Summary.

1. The distribution of amplitudes over small circular telephonic diaphragms, under simple impressed vibrations, has been measured, it is believed for the first time, by means of a new and specially constructed vibration-explorer.

2. The simple vibrations of the small steel circular diaphragms, used in telephonic receivers, appear to belong to the fundamental mode, within the ordinary telephonic range of intensity and frequency up to 2,000 ~, with the distribution of impressed forces here described.

3. The explorations have confirmed the working theory of the velocity-circle diagram for such vibrations, and have afforded means of determining the three constants *m*, r and *s*, in that theory, for acoustically excited vibrations.

4. In the resonant condition, exploration is somewhat uncertain, owing to slight instability in the vibratory behavior of the diaphragm.

5. The distribution of forced amplitude at varying radial distances, has been found to compare well with the Rayleigh theory of freely vibrating plates, when good flat clamping around the edge can be secured, and with acoustic excitation. The coefficient of equivalent mass appears to be 0.183 for such a case. With electromagnetic excitation, the amplitude distribution may be very different and the coefficient is ordinarily increased.

6. Loading a diaphragm with a small mass at the center, decreases its natural frequency, and tends to reduce the amplitude of vibration at the center, with a relative increase at outlying points; so that the equivalent mass of the diaphragm, considered by itself, is apt to be changed.

7. A means is suggested, based on the velocity-circle diagram, for comparing the acoustic intensities of organ-pipes of different pitches.

8. The distribution of amplitudes over the surface of a steel receiving-telephone diaphragm, with bipolar electromagnetic excitation, was found to be of fundamental mode, but with a tendency to form two maxima, one over each pole.

9. In some small, thin, glass diaphragms, three modes of vibra-

tory motion were observed, in the range of acoustic impressed frequency up to 1,700 ~.

TABLE III.

FLAT CIRCULAR DIAPHRAGMS.

No.	Material.	Diameter, Cm.	Thickness* Over Japan, Cm.	Mass, Gm.	Natural Frequency ~.
1	Steel japanned......	5.4	0.038	5.615	824
2	Steel japanned......	5.52	0.0399	5.979	992
3	Steel japanned.....	5.48	0.031	4.181	1020
4	Glass.............	5.4	0.0108	0.6548	492

APPENDIX I.

Application of Bessel-Function Theory to a Diaphragm Vibrating in its Fundamental Mode.

Referring to Lord Rayleigh's "Theory of Sound," Vol. 1, page 352, the formula for the instantaneous amplitude of free vibration in a flat plate is,

$$w_n = P\{J_n(kr) + \lambda J_n(ikr)\}\cos(n\theta + \alpha_n)\cdot\cos(\omega t + e) \quad \text{cm.,} \quad (1)$$

where subscript n = the number of nodal diameters (numeric),

w_n = instantaneous amplitude at a point on the diaphragm whose polar coördinates are r cm., θ radians (cm.)

P = constant of amplitude-magnitude (cm.),

k = a constant of the material defined by:

$k = \sqrt{\omega}/c \qquad (\text{cm.}^{-1})$,

c = a constant of the material defined by:

$$c = \sqrt[4]{\frac{qb^2}{12\rho(1-\sigma^2)}} \qquad (\text{cm./sec.}^{\frac{1}{2}}),$$

q = Young's modulus for the diaphragm material (dyne/cm.^{2}),

ρ = density of the diaphragm material (gms./cm.^{3}),

σ = Poisson's ratio for the diaphragm material (numeric),

b = thickness of the diaphragm (cm.),

λ = a constant satisfying boundary conditions (numeric),

J_n = a Bessel's Function of the nth order (numeric),

$i = \sqrt{-1}$,

* Thickness of japan 0.0074 cm.

α_n = a phase-angle measured around the diaphragm (radians),
$\omega = 2\pi n$ = angular velocity of vibrating motion (radians/sec.),
n = frequency of diaphragm vibration (cycles/sec.),
t = time elapsed from a given epoch (seconds),
e = a time-phase determined by the epoch (seconds),
a = radius of the diaphragm (cm.).

For the fundamental mode of motion, $n = 0$; or there must be no nodal diameters. Consequently (1) reduces to:

$$w_0 = P\{J_0(kr) + \lambda J_0(ikr)\} \cos(\omega t + e) \qquad \text{cm. (2)}$$

Here the amplitude of vibration at any point w_0, ceases to be a function of θ, and depends only on Bessel functions of r. Since we shall consider only the fundamental mode of vibration in what follows, the subscript will be unnecessary, and we may substitute w for w_0.

Continuing Lord Rayleigh's method of demonstration, if a flat circular diaphragm is clamped at its edge between a pair of flat circular rings, then, referring to (2), we have w vanishing at $r = a$, the clamping radius, and since there is to be no bending or slope of the diaphragm at the clamped boundary, we have also $(dw/dr) = 0$ at $r = a$.

Entering (2) with $w = 0$, we have:

$$\lambda = -\frac{J_0(ka)}{J_0(ika)} \qquad \text{numeric. (3)}$$

Also differentiating (2) with respect to r, for $r = a$, we obtain:

$$\frac{1}{k}\frac{dw}{dr} = J_0'(ka) + i\lambda J_0'(ika) = 0 \qquad \text{numeric, (4)}$$

whence

$$\lambda = -\frac{J_0'(ka)}{iJ_0'(ika)} \qquad \text{numeric. (5)}$$

Combining (3) and (5) we obtain:

$$\frac{J_0(ka)}{J_0(ika)} = \frac{J_0'(ka)}{iJ_0'(ika)} = \frac{J_1(ka)}{iJ_1(ika)} \qquad \text{numeric. (6)}$$

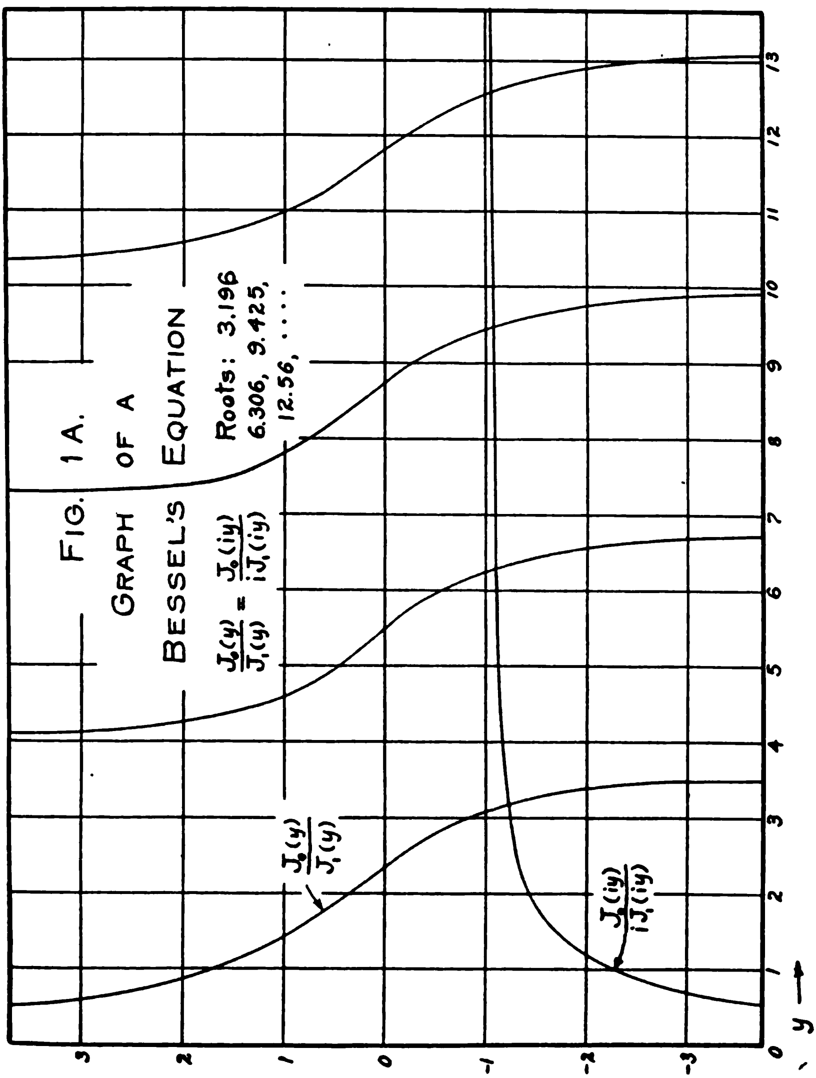
Fig. 1 A.
Graph of a
Bessel's Equation
$\frac{J_0(y)}{J_1(y)} = \frac{J_0(iy)}{iJ_1(iy)}$
Roots: 3.196
6.306, 9.425,
12.56, ...
$\frac{J_0(y)}{J_1(y)}$
$\frac{J_0(iy)}{iJ_1(iy)}$
y →
3
2
1
0
-1
-2
-3
0
1
2
3
4
5
6
7
8
9
10
11
12
13

This is a transcendental equation involving Bessel's Functions of the zeroth and first orders. It is capable of being satisfied, by trial, with an indefinitely great number of roots, each corresponding to a possible mode of vibration with nodal circles. Fig. 1*A* indicates graphically the method of determining the successive roots of (6). The points of intersection of the lower curve with the successive descending branches, indicate the values of $y = kr$ which satisfy (6). In order to have the fundamental mode of vibration, there must be no nodal circles, which means that the first and lowest root for ka must be taken in (6). This root is at $ka = 3.196$. . . . Placing this value for ka in (3) we have:

$$\lambda = \frac{-J_0(3.196)}{J_0(i3.196)} = -\frac{-0.3197}{5.730} = +0.05571 \quad \text{numeric.} \quad (7)$$

Re-entering (2) with this value of λ, we have for the fundamental mode of vibration of the circular diaphragm:

$$w_{max} = P\{J_0(kr) + 0.05571 J_0(ikr)\} \quad \text{cm.} \quad (8)$$

In Fig. 1*B*, the abscissas correspond both to kr, where $k = 1.21$ cm.$^{-1}$, and to r in cm., the relation being as already pointed out that at the boundary $r = a = 2.62$ cm. and $kr = 3.196$. The ordinates are the numerical values of Bessel's functions as taken from Tables. They also represent vibratory amplitudes of the diaphragm, taking the maximum amplitude at the center ($r = 0$) in microns, corresponding to the heavy curve. The upper faint curve shows the graph of the first Bessel function $J_0(kr)$; while the lower faint curve shows the corresponding graph of λ times the second Bessel function, or $0.05571 J_0(ikr)$. Adding these two graphs, as called for by (2), we obtain the heavy curve, which represents the theoretical amplitude of vibration along any radius of this particular diaphragm, assuming such a scale that 1.056 corresponds to the maximum or central amplitude. The small circles near this curve show the amplitudes observed with the aid of the vibration explorer.

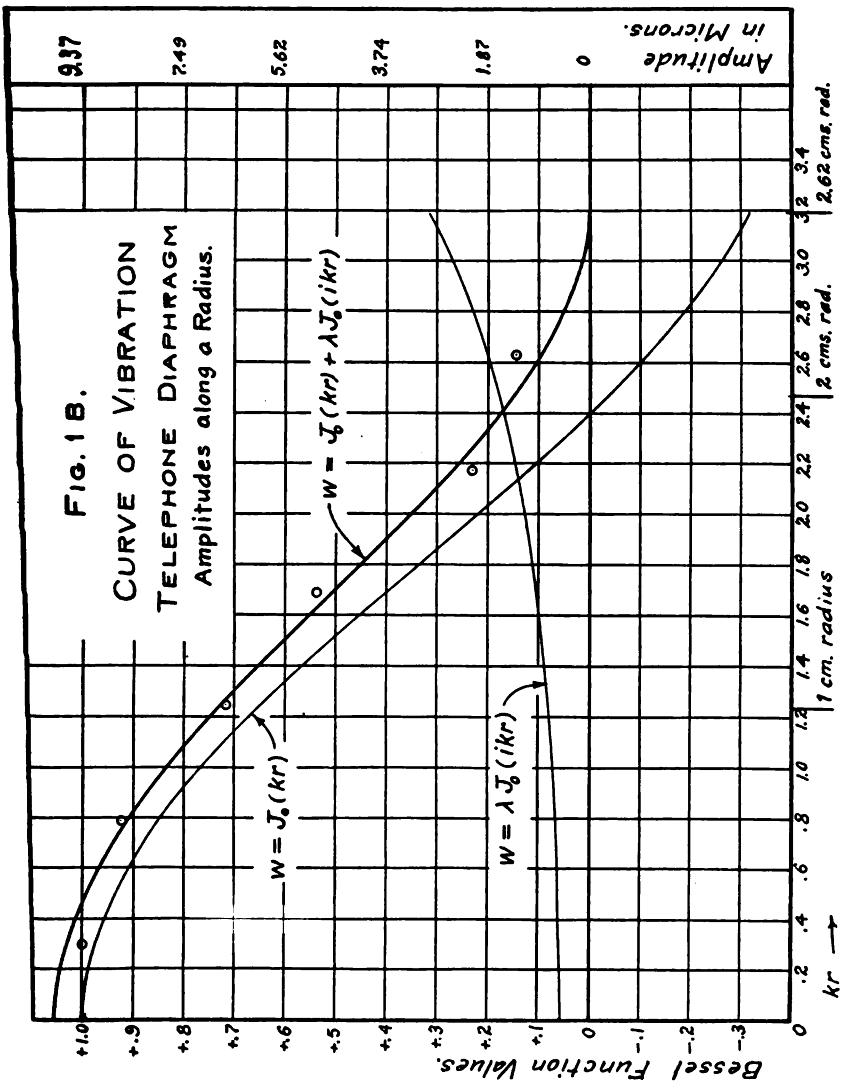

Fig. 1 B.
Curve of Vibration
Telephone Diaphragm
Amplitudes along a Radius.
$W = J_0(kr) + \lambda J_0(ikr)$
$W = J_0(kr)$
$W = \lambda J_0(ikr)$
Amplitude in Microns.
9.37
7.49
5.62
3.74
1.87
0
Bessel Function Values.
+1.0
+.9
+.8
+.7
+.6
+.5
+.4
+.3
+.2
+.1
0
-.1
-.2
-.3
kr →
0
.2
.4
.6
.8
1.0
1.2
1.4
1.6
1.8
2.0
2.2
2.4
2.6
2.8
3.0
3.2
3.4
1 cm. radius
2 cms. rad.
2.62 cms. rad.

Appendix II.

Elementary Theory of the Steady Vibration Amplitude of a Diaphragm Vibrating in its Fundamental Mode, as a Function of the Impressed Frequency.

Let w = the vibration amplitude at the center of the diaphragm[8] (cm. $\angle$),

w_r = the vibration amplitude at the radius r (cm. $\angle$),

$\dot{w}$ = the vibration velocity at the center of the diaphragm (cm./sec. $\angle$),

$\ddot{w}$ = the vibration acceleration at the center of the diaphragm (cm./sec.^{2} $\angle$),

r = frictional resistance to motion of the diaphragm, referred to the equivalent mass, see below (dynes/cm. per sec. $\angle$),

t = elapsed time from a given epoch (seconds),

s = elastic force of the diaphragm per cm. of displacement, referred to the equivalent mass (dynes per cm. $\angle$),

$f = Fe^{i\omega t}$ = impressed simple harmonic moving force on the diaphragm tending to produce displacement w, and measured in the direction of w, referred to the equivalent mass (dynes $\angle$),

$i = \sqrt{-1}$,

$\omega = 2\pi n$ = the angular velocity of a simple harmonic motion of frequency n (radians/sec.),

m = equivalent mass of the diaphragm, defined by the condition that the energy of motion of this mass with the velocity $\dot{w}$ at the center, is equal to the actual energy of the diaphragm with its distributed mass and velocities, according to the equation:

$$\frac{m}{2}(\dot{w})^2 = \frac{2\pi\rho'}{2}\int_0^a r(\dot{w}_r)^2 dr \qquad \text{ergs,} \quad (1)$$

where ρ' = superficial density of the diaphragm (gm./cm.^{2}),

$$m = \frac{2\pi\rho'}{w_{\max}^2}\int_0^a (w_r)^2 r dr \qquad \text{gm.,} \quad (2)$$

[8] The sign $\angle$ after a unit indicates a "complex quantity."

since the velocities $\dot{w}$ and $\dot{w}_r$ being assumed simply harmonic, are respectively proportional to their maximum displacements w_{max} and w_r.

Then on the assumptions that the diaphragm vibrates like its equivalent mass collected at the center, with its observed central velocity, with an elastic opposing force sw on this mass, proportional to the displacement, and with a resisting force $r\dot{w}$ on this mass proportional to the velocity, then the equation of motion of the diaphragm in terms of equivalent mass will be[9]

$$sw + \mathrm{r}\dot{w} + m\ddot{w} = f = F\epsilon^{i\omega t} \qquad \text{dynes } \angle. \quad (3)$$

The solution of this equation, in terms of velocity $\dot{w}$, and the steady state, is known to be

$$\dot{w} = \frac{f}{\mathrm{r} + i\left(m\omega - \frac{s}{\omega}\right)} = \frac{f}{\mathrm{r} + ix} = \frac{f}{z} = \frac{F}{z}\epsilon^{i\omega t} \qquad \frac{\text{cm.}}{\text{sec.}} \angle, \quad (4)$$

where x is the "mechanical reactance," and z is the complex "mechanical impedance," by analogy to alternating electric current theory. Both x and z have the same dimensions as r.

The mechanical impedance relations are indicated in Fig. IIA at the left-hand side. OX and OY being rectangular coördinates, the "mechanical resistance" r in dynes per unit velocity, is measured along OX, and is assumed to remain constant at all frequencies. As the frequency n is increased (and with it the vibratory angular velocity ω) from zero to infinity, the reactance $x = (m\omega - s/\omega)$ varies from $-\infty$ to $+\infty$ along the line yXy'. The mechanical impedance z which is the vector sum of r and ix, will be represented by a complex quantity, or plane vector Op, the extremity of which remains on the line yXy'. At the particular or resonant value of ω, for which $m\omega - s/\omega = 0$, the reactance vanishes, and the impedance z coincides with the resistance r. As shown in the figure, p lies above OX, corresponding to a value of ω somewhat greater than the critical or resonant value.

[9] See Bibliography No. 8.

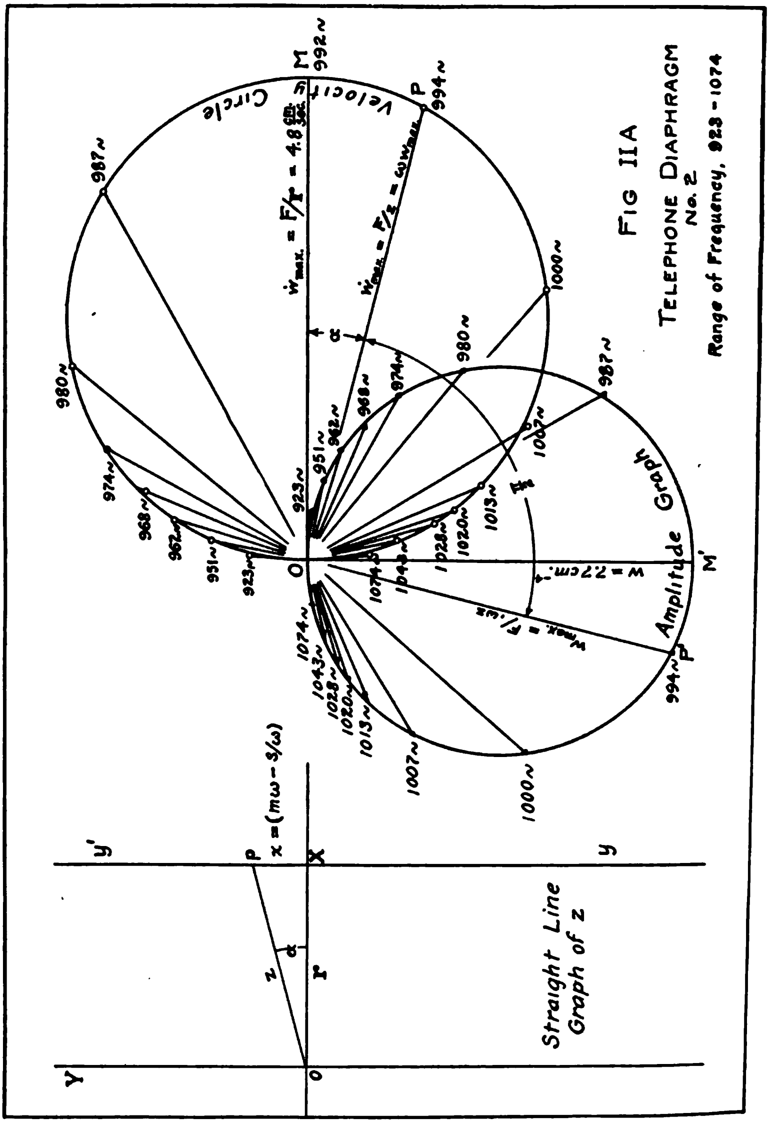

FIG IIA
TELEPHONE DIAPHRAGM
No. 2
Range of Frequency, 923 - 1074

Equation (4) shows that the displacement velocity $\dot{w}$ is equal to the impressed vibro-motive force f, divided by the impedance z. The locus of this velocity, as ω varies from 0 to ∞ with constant F, becomes a circle OMP, the diameter OM of which is equal to F/r cm. per sec., while the angle α of the chord OP, measuring the velocity, is equal and of opposite sign to the angle α of the impedance z. In the case represented by Fig. IIA, the telephone diaphragm No. 2 was actuated electromagnetically at constant alternating-current strength, under varying frequency. At the frequency $n=992\sim$, the vibratory velocity $OM=4.8$ cm. sec., was a maximum, and was in phase with the impressed vibro-motive force F. At $n=994\sim$, the mechanical impedance had increased to op at the angle $\alpha=14°$, and the vibratory velocity had fallen from OM to OP or from 4.8 to 4.65 cm. per sec. lagging in phase behind the impressed vibro-motive force by 14°. The diagram shows that between the frequencies of 923 and 1,074$\sim$, the vector displacement velocity $\dot{w}$ had moved over nearly the entire circumference of the velocity circle OMP, and from a phase nearly 90° ahead of the impressed vibro-motive force to nearly 90° behind it.

If we integrate (4) with respect to time, we obtain, for the steady state of motion,

$$w = \int \dot{w}dt = \int \frac{F\epsilon^{i\omega t}}{z} dt = \frac{F\epsilon^{i\omega t}}{i\omega z} = -\frac{iF\epsilon^{i\omega t}}{\omega z}, \qquad \text{cm. } \angle. \quad (5)$$

This shows that the instantaneous displacement is ω times less than the corresponding instantaneous velocity, and is 90° behind it in phase. If we consider the maximum displacement, we have

$$w_{\max} = -\frac{iF}{\omega z} \qquad \text{cm.} \quad (6)$$

The locus of $w_{\max}$ is therefore a closed curve distorted from a circle by the effect of varying ω in the denominator. Considering it as an approximate circle for this case, the diameter OM' corresponding to $n=992\sim$ represents a displacement amplitude of 7.7μ, lagging approximately 90° behind the maximum velocity OM. At the frequency $994\sim$, the displacement would be $OP'=7.48\mu$,

lagging 90° behind OP. As the frequency varies between 923 and 1,074 ~, the displacement amplitude almost covers the entire graph of the approximate circle $OM'P'$, commencing at about 1 μ, nearly in phase with the vibro-motive force, and ending at about 1 μ in nearly opposite phase. These amplitudes correspond to the ordinates of the resonance curve in Fig. 9.

It follows from (4) that if the vibro-motive force f is kept constant, and the angular velocity adjusted until the central vibration velocity is a maximum, this will occur when the mechanical reactance is zero, or when

$$m\omega_0 - \frac{s}{\omega_0} = 0 \qquad \frac{\text{dynes}}{\text{cm./sec.}}, \qquad (7)$$

that is

$$\omega_0 = \sqrt{\frac{s}{m}} \qquad \frac{\text{radians}}{\text{sec.}}. \qquad (8)$$

So that

$$s = m\omega_0^2 \qquad \frac{\text{dynes}}{\text{cm.}}. \qquad (9)$$

When the vibro-motive force f is made to vanish in (3) with the diaphragm in motion, the solution of the equation is

$$w = W\epsilon^{-\frac{rt}{2m}} \sin(\omega t + e) \qquad \text{cm.}, \qquad (10)$$

where W is the initial displacement (cm), and e a suitable phase (radians). If we obtain two successive values of w, (w_1 and w_2), corresponding to two successive elongations in the same direction, we have

$$\frac{w_1}{w_2} = \epsilon^{r/2mn} = \epsilon^{\Delta/n} \qquad \text{numeric}, \qquad (11)$$

whence

$$r = 2mn \log_\epsilon (w_1/w_2), \qquad \text{dynes/(cm./sec.)}, \qquad (12)$$

where Δ is the damping constant (1/sec.).

The quantity $\log_\epsilon (w_1/w_2)$ is well known as the logarithmic decrement of the decay curve.

Appendix III.

Elementary Theory of Equivalent Mass.

In (2) of Appendix II., the expression for equivalent mass m is

$$m = \frac{2\pi\rho'}{w^2_{\max}} \int_0^a w_r^2 \cdot r \quad dr \qquad \text{gm.} \quad (1)$$

or m is the mass which, vibrating at the center of the diaphragm with the observed maximum ampltiude $w_{\max}$, would have the same kinetic energy as the total distributed kinetic energy of the diaphragm.

In order, therefore, to determine the equivalent mass of a diaphragm, it is necessary to integrate r times the square of the amplitude over its surface. Assuming that the vibration follows Rayleigh's Bessel-function theory as outlined in Appendix I., it should be sufficient to integrate $w_r^2 \cdot r$ over the surface, mathematically. We are indebted to Dr. Geo. A. Campbell for an indication of the solution of this integral.[10]

In (1)

$$w_{\max} = P[J_0(0) + \lambda J_0(i0)] = P(1 + \lambda) \qquad \text{cm.} \quad (2)$$

by reference to (8) Appendix I., putting $r = 0$.
Also

$$w_r = P[J_0(kr) + \lambda J_0(ikr)] \qquad cm. \quad (3)$$

$$\therefore m = \frac{2\pi\rho'}{P^2(1+\lambda)^2} \int_0^a P^2\{J_0^2(kr) + \lambda^2 J_0^2(ikr) + 2\lambda J_0(kr) J_0(ikr)\} r dr \quad (4)$$

$$= \frac{2\pi\rho'}{(1+\lambda)^2} \left[\int_0^a J_0^2(kr) r \cdot dr + \int_0^a \lambda^2 J_0^2(ikr) r \cdot dr + \int_0^a 2\lambda J_0(kr) J_0(ikr) r \cdot dr \right]$$

$$= \frac{2\pi\rho'}{(1+\lambda)^2} \left[\frac{a^2}{2}\{J_0^2(ka) + J_1^2(ka)\} + \frac{\lambda^2 a^2}{2}\{J_0^2(ika) + J_1^2(ika)\} + \frac{2\lambda a}{k^2 - i^2k^2}\{kJ_0(ika)J_1(ka) - ikJ_0(ka)J_1(ika)\} \right], \quad (5)$$

[10] Bibliography (11), (12), (13).

where

$$J_0^2(kr) \text{ stands for } \{J_0(kr)\}^2.$$

But $M = \pi\rho' a^2$ is the total mass of the vibrating diaphragm area.

$$\therefore \frac{m}{M} = \frac{1}{(1+\lambda)^2}\Big[\{J_0^2(ka) + J_1^2(ka)\} + \lambda^2\{J_0^2(ika) + J_1^2(ika)\} \\ + \frac{2\lambda}{ak}\{J_0(ika)J_1(ka) - iJ_0(ka)J_1(ika)\}\Big]. \tag{6}$$

Applying the ratios of (6) Appendix I., this reduces to:

$$\begin{aligned}\frac{m}{M} &= \frac{1}{(1+\lambda)^2}\cdot 2J_0^2(ka)\\ &= \frac{1}{(1.05571)^2}\cdot 2J_0^2(3.196)\\ &= \frac{0.20378}{1.1145}\\ &= 0.18285\end{aligned}$$

or, to three significant digits, 0.183.

The "equivalent mass coefficient," 0.183, for this diaphragm, had also been obtained by quadrature methods applied to the heavy curve in Fig. 1*B*, before the integration was performed as above.

In the case of steel telephone diaphragms excited by bipolar electromagnets, the curves of w_r, r are likely to depart from simple Bessel-function curves, see Fig. 14. In such cases, the coefficient of equivalent mass must be deduced from the exploration curve. In cases examined, this coefficient varied between 0.2 and 0.5.

A quadrature method employed to find the equivalent mass coefficient from curves of any shape is as follows:

Draw the w_r curve as in Fig. 1*B*. Divide the line of abscissas into an integral number n of annular rings of equal area; so that each ring will have a mass of M/n, where M is the total mass of the circular vibrating area of the diaphragm, in grams. We then multiply this annular mass into the square of the observed amplitudes at the middle points of the successive annuli. The sum of these terms will be equal to the product of the equivalent mass m,

TABLE IV.

	kr	w	w (ave.)	w^2 (ave.)
	.0000	1.0557		(1.1145)
1	.4511	1.008	1.032	1.0650
2	.6380	.962	.985	.9702
3	.7814	.918	.940	.8836
4	.9023	.875	.897	.8046
5	1.009	.833	.854	.7293
6	1.105	.792	.812	.6593
7	1.194	.752	.772	.5960
8	1.276	.713	.732	.5358
9	1.353	.677	.695	.4830
10	1.427	.641	.659	.4343
11	1.496	.606	.624	.3894
12	1.563	.572	.589	.3469
13	1.627	.539	.555	.3080
14	1.688	.507	.523	.2735
15	1.747	.477	.492	.2421
16	1.805	.449	.463	.2144
17	1.860	.421	.435	.1892
18	1.914	.394	.408	.1665
19	1.966	.367	.380	.1444
20	2.018	.341	.354	.1253
21	2.067	.318	.330	.1089
22	2.116	.295	.307	.0942
23	2.163	.273	.284	.0807
24	2.210	.251	.262	.0686
25	2.256	.232	.242	.0586
26	2.300	.213	.223	.0497
27	2.344	.195	.204	.0416
28	2.387	.178	.186	.0346
29	2.429	.162	.170	.0289
30	2.471	.146	.154	.0237
31	2.512	.131	.139	.0193
32	2.552	.117	.124	.0154
33	2.592	.104	.110	.0121
34	2.631	.092	.098	.0096
35	2.669	.080	.086	.0074
36	2.707	.070	.075	.0056
37	2.744	.060	.065	.0042
38	2.781	.050	.055	.0030
39	2.817	.041	.045	.0020
40	2.853	.033	.037	.0014
41	2.889	.026	.030	.0009
42	2.924	0.21	.023	.0005
43	2.958	.016	.019	.0004
44	2.992	.012	.014	.0002
45	3.026	.009	.011	.0001
46	3.060	.006	.008	.00006
47	3.093	.004	.005	.00002
48	3.126	.002	.003	.000009
49	3.158	.001	.001	.000001
50	3.196	.000	.000	.000000
				10.2325

$$m = (M/50)(10.2325/1.1145) = .183\,M.$$

and the square of the maximum observed amplitude at the center, or

$$m = \frac{M}{n} \frac{\Sigma w_r^2}{w_{\max}^2} \qquad \text{gm.} \quad (7)$$

The preceding table sets forth this process for the curve of Fig. 1*B*, drawn theoretically, and checked observationally, with $n = 50$, or the diaphragm divided into 50 annuli of equal mass. The result is that the equivalent mass is 18.3 per cent. of the actual mass of the vibrating area. This result checks that obtained from the mathematical integration of the Bessel curve.

Although 50 annuli of equal area and mass were taken in the case above worked out, so as to attain a fairly high degree of precision in the evaluated equivalent-mass coefficient; yet, for many purposes, a sufficient degree of precision might be attained by taking only 10 such equal annular areas.

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TABLE OF SYMBOLS.

a = Radius of the diaphragm clamping-circle (cm.),
α_n = A phase angle measured around the diaphragm (radians),
b = Thickness of the diaphragm (cm.),
c = A constant of the material of the diaphragm (cm./second ½),
d = Sign of differentiation
Δ = Damping constant $= n \log_\epsilon (w_1/w_2) = \mathrm{r}/2m$ (second^{-1}),
e = Time-phase (radians),
ϵ = Naperian logarithmic base (numeric),
$f = F\epsilon^{i\omega t}$ Impressed simple harmonic moving force on the diaphragm (dynes) $\angle$
f_s = Statical tension (dynes),
F = Maximum value of a vibratory force (dynes),
$i = \sqrt{-1}$ (numeric),
J_n = A Bessel's Function of the nth order (numeric),
J' = The first derivative of J with respect to r (numeric),
k = A constant of the material of the diaphragm, defined by $k = (\sqrt{\omega})/c$ (cm.$^{-1}$),
L = Distance from mirror to scale of explorer (cm.),
l = Radius arm of small mirror in explorer (cm.),
λ = A constant satisfying boundary conditions (numeric),
M = Total mass of diaphragm (in Appendix III) (gm.),
M = Magnification factor of explorer (numeric),
m = Equivalent mass of the diaphragm (gm.),
μ = Micron, 10^{-4} cm. (cm^{-4}),
n = Frequency of diaphragm vibration (cycles/second),
n_0 = Resonant frequency of diaphragm vibration (cycles/sec),
n = Number of annular rings in equivalent mass theory of App. III (numeric),
n (Subscript) = Number of nodal diameters (order of Bessel's Function) (numeric),
P = Constant of amplitude-magnitude (cm.),
π = 3.1416 (numeric),
ϕ = Angle in the explorer between the plane of mirror and plane of diaphragm (deg.),
q = Young's modulus for diaphragm material (dynes/cm.^{2}),
r = Frictional resistance to motion of diaphragm $\frac{\text{dynes}}{\text{cm./sec.}}$,

r = Distance along a radius (cm.),
ρ = Density of diaphragm material (gm./cm.^{3}),
ρ' = Superficial density of diaphragm (gm./cm.^{2}),
s = Elastic force of diaphragm per centimeter of displacement, referred to equivalent mass (dynes/cm.),
σ = Poisson's ratio for material of diaphragm (numeric),
Σ = Sign of summation,
t = Time elapsed from a given epoch (seconds),
θ = Azimuth angle measured on surface of diaphragm (radians)
vmf. = Vibro-motive force (dynes) $\angle$,
W = Initial displacement in a vibratory motion (cm.),
w and w_0 = Amplitude of a point on surface of diaphragm for fundamental mode of vibration (cm.) $\angle$,
w_r = Amplitude of vibration of a point at radius r from center of diaphragm (cm.) $\angle$,
w_n = Instantaneous amplitude of vibration (cm.),
w_{max} = Maximum cyclic amplitude at center (cm.),
$\dot{w}$ = Vibratory velocity at center of diaphragm (cm./sec.) $\angle$,
$\ddot{w}$ = Vibratory acceleration at center of diaphragm (cm./sec.^{2}) $\angle$,
w_s = Statical displacement of center of diaphragm (cm.),
$ix = i(m\omega - s/\omega)$ "Mechanical reactance" of vibrating diaphragm (by analogy to alternating-current theory) {dynes/(cm./sec.)} $\angle$,
$z = (\mathrm{r} + ix)$ "Mechanical impedance" of vibrating diaphragm (by analogy to alternating-current theory) {dynes/(cm./sec.)} $\angle$,
$\omega = 2\pi n$ = Angular velocity of vibratory motion (radians/sec.),
$\omega_0 = 2\pi n_0$ = Angular velocity at resonance (radians/sec.),
∞ = Infinity,
$\angle$ = This sign after a unit indicates a "complex quantity,"
$\sim$ = Cycles or vibrations per second (cycles/sec.).

THE RULING AND PERFORMANCE OF A TEN INCH DIFFRACTION GRATING.

By A. A. MICHELSON.

(*Read April 22, 1915.*)

The principal element in the efficiency of any spectroscopic appliance is its resolving power—that is, the power to separate spectral lines. The limit of resolution is the ratio of the smallest difference of wave-length just discernible to the mean wave-length of the pair or group. If a prism can just separate or resolve the double yellow line of sodium its limit of resolution will be $\frac{5896-5890}{5893}$ or approximately one one thousandth, and the resolving power is called one thousand.

Until Fraunhofer (1821) showed that light could be analyzed into its constituent colors by diffraction gratings this analysis was effected by prisms the resolving power of which has been gradually increased to about thirty thousand. This limit was equalled if not surpassed by the excellent gratings of Rutherford, of New York, ruled by a diamond point on speculum metal, with something like 20,000 lines, with spacing of 500 to 1,000 lines to the millimeter. These were superseded by the superb gratings of Rowland with something over one hundred thousand lines, and with a resolving power of 150,000.[1]

The theoretical resolving power of a grating is given as was first shown by Lord Rayleigh by the formula $R = mn$, in which n is the total number of lines, and m the order of the spectrum. An equivalent expression is furnished by

$$R = \frac{l}{\lambda}(\sin i + \sin \theta),$$

[1] The 6½ in. gratings now ruled on the Rowland engine have a much higher resolving power—probably 400,000.

where l is the total length of the ruled surface, λ the wave-length of the light, i the angle of incidence and θ the angle of diffraction, and the maximum resolving power which a grating can have is that corresponding to i and θ each equal to 90° which gives $R = 2l/\lambda$; that is twice the number of light waves in the entire length of the ruled surface.

This shows that neither the closeness of the rulings nor the total number determine this theoretical limit, and emphasizes the importance of a large ruled space.

This theoretical limit can be reached, however, only on the condition of an extraordinary degree of accuracy in the spacing of the lines. Several methods for securing this degree of accuracy have been attempted but none has proved as effective as the screw. This must be of uniform pitch throughout and the periodic errors must be extremely small.

For a short screw, for example one sufficient for a grating two inches in length, the problem is not very difficult, but as the length of the screw increases the difficulty increases in much more rapid proportion. It was solved by Rowland in something over two years.

Since this time many problems have arisen which demand a higher resolving power than even these gratings could furnish. Among these is the resolution of doubles and groups of lines whose complexity was unsuspected until revealed by the interferometer and amply verified by subsequent observations by the echelon and other methods.

Others that may be mentioned in this connection are the study of the distribution of intensities within the spectral "lines"; their broadening and displacement with temperature and pressure; the effect of magnetic and electric fields, and the measurement of motions in the line of sight, as revealed by corresponding displacement of the spectral lines in consequence of the Doppler effect.

All of these have been attacked with considerable success by observations with the echelon, the interferometer and the plane-parallel plate. These methods have a very high resolving power, but labor under the serious disadvantage that adjacent succeeding

spectra overlap, making it difficult to interpret the results with certainty.

Some twelve years ago the construction of a ruling engine was undertaken with the hope of ruling gratings of fourteen inches—for which a screw of something over twenty inches is necessary. This screw was cut in a specially corrected lathe so that the original errors were not very large, and these were reduced by long attrition with very fine material until it was judged that the residual errors were sufficiently small to be automatically corrected during the process of ruling.

The principal claim to novelty of treatment of the problem lies in the application of interference method to the measurement and correction of these residual errors.

For this purpose one of the interferometer mirrors is fixed to the grating carriage, while a standard, consisting of two mirrors at a fixed distance apart, is attached to an auxiliary carriage. When the adjustment is correct for the front surface of the standard, interference fringes appear. The grating carriage is now moved through the length of the standard (one tenth of a millimeter if the periodic error is to be investigated; ten or more millimeters if the error of run is to be determined) when the interference fringes appear on the rear surface. This operation is repeated, the difference from exact coincidence of the central (achromatic) fringe with a fiducial mark being measured at each step in tenths of a fringe (twentieths of a light-wave). As a whole fringe corresponds to one hundred thousandth of an inch, the measurement is correct to within a millionth of an inch.

The corresponding correction for periodic errors is transferred to the worm-wheel which turns the screw; and for errors of run to the nut which moves the carriage. In this way the final errors have been almost completely eliminated and the resulting gratings have very nearly realized their theoretical efficiency.

A number of minor points may be mentioned which have contributed to the success of the undertaking.

(*a*) The ways which guide the grating carriage as well as those which control the motion of the ruling diamond must be very

true; and these were straightened by application of an auto-collimating device which made the deviation from a straight line less than a second of arc.

(*b*) The friction of the grating carriage on the ways was diminished to about one tenth of that due to the weight (which may amount to twenty to forty pounds) by floating on mercury.

(*c*) The longitudinal motion of the screw was prevented by allowing its spherically rounded end to rest against an optically plane surface of diamond which could be adjusted normal to the axis of the screw.

(*d*) The screw was turned by a worm wheel (instead of pawl and ratchet) which permits a simple and effective correction of the periodic errors of the screw throughout its whole length.

(*e*) A correcting device which eliminates periodic errors of higher orders.

(*f*) It may be added that the nut which actuates the carriage had bearing surfaces of soft metal (tin) instead of wood, as in preceding machines. It was not found necessary to unclamp the nut in bringing it back to the starting point.

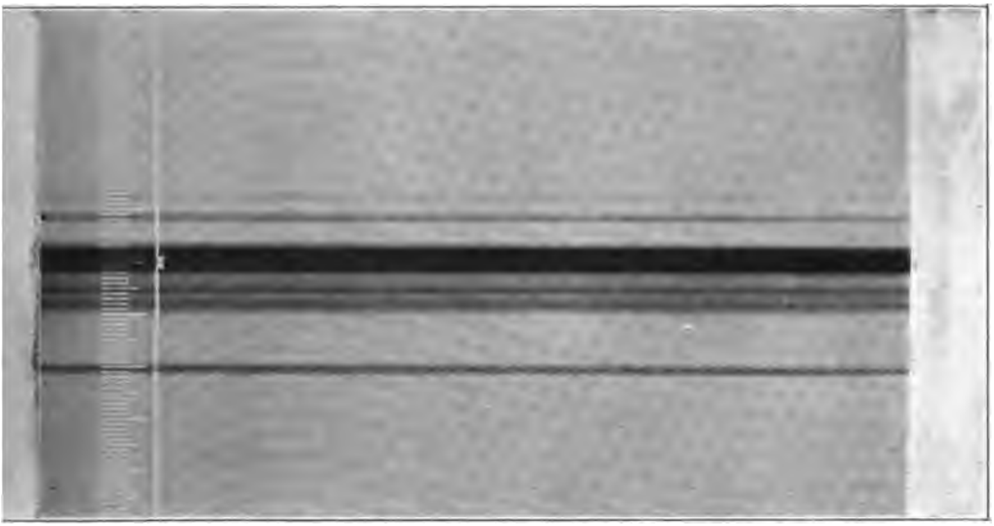
Finally it may be noted that instead of attempting to eliminate the errors of the screw by long continued grinding—which inevitably leads to a rounding of the threads—it has been the main object to make these errors conveniently small; but especially to make them constant—for on this constancy depends the possibility of automatic correction.

The accompanying photograph made with a ten-inch grating, 6th order (actual ruled surface 9.4 inches by 2.8 inches), used in the Littrow form with an excellent 8-inch lens by Brashear, is given in evidence of its performance. The resolving power as shown by the accompanying scale of Angström units is about 450,000. The original negative shows a resolving power of about 600,000. The theoretical value is 660,000.

Doubtless the possibility of ruling a perfect grating by means of the light waves of a homogeneous source has occurred to many—and indeed this was one of the methods first attempted.

It may still prove entirely feasible—and is still held in reserve if

serious difficulty is encountered in an attempt now in progress to produce gratings of twenty inches or more. Such a method may be made partly or perhaps completely automatic, and would be independent of screws or other instrumental appliances.



Enlargement of photograph of the green mercury line λ 5461, taken by H. L. Lemon with 10-inch diffraction grating in sixth order. Scale: 1 division = 0.01 A.U.; ruled surface 9⅜ in. × 2⅞ in., 11,700 lines per inch. Mounted in Littrow form with 8-inch lens by Brashear. Focal length 20 feet.

It may be pointed out that an even simpler and more direct application of light-waves from a homogeneous source is theoretically possible and perhaps experimentally realizable.

If a point source of such radiations send its light-waves to a collimating lens and the resulting plane waves are reflected at normal incidence from a plane surface, stationary waves will be set up as in the Lippman plates; these will impress an inclined photographic plate with parallel lines as in the experiment of Wiener; and the only limit to the resolving power of the resulting grating is that which depends on the degree of homogeneity of the light used. As some of the constituents of the radiations of mercury have been shown to be capable of interfering with difference of path of over

a million waves, such as grating would have a resolving power exceeding a million.

This investigation has had assistance from the Bache Fund of the National Academy of Science, from the Carnegie Institution, and from the University of Chicago.

In addition to the grateful acknowledgment to these institutions I would add my high appreciation of the faithful services rendered by Messrs. Julius Pearson and Fred Pearson.

THE CONSTITUTION OF THE HEREDITARY MATERIAL.

By T. H. MORGAN.

(*Read April 23, 1915.*)

There are two ways in which the relation of the egg to the characters of the individual that develops from the egg has been interpreted.

1. The egg has been thought of as a whole and the characters of the individual as the product of its activity as a unit.

2. The egg has been thought of as made up of representative particles of some sort that stand in a definite relation to the parts of the individual that comes from the egg.

Weismann, whose speculations occupied the forefront of interest at the close of the last century, adopted the latter view; namely, that the germ is made up of particles, which he called determiners. For Weismann embryonic development became merely the sorting out of the particles of the germ to their respective parts of the embryo. Each region of the body owed its peculiarities to the particles that came to it by this sorting-out process. In fact, one may go so far, I think, as to say that Weismann borrowed from Roux this particular form of the preformation in order to give a formal explanation of *embryonic differentiation*. But Weismann's theory soon encountered three serious reverses.

In the first place, the study of the minute structure and behavior of the segmenting egg shows no evidence that any such sorting-out process takes place, as Weismann postulated. It has been shown that the chromosomes divide equally at every division, and that every cell of the body contains the entire complex that was present in the fertilized egg-cell itself.

In the second place, it was shown that the sequence of the cleavage planes of the egg could be artificially altered, yet a normal embryo develop.

In the third place, it was shown that in some eggs each of the first two, or first four cells derived from the egg is capable of forming a whole embryo. This result creates a strong presumption against the adequacy of Weismann's interpretation of development.

Meanwhile one of the greatest biological discoveries of the last century—one that had a very direct bearing on the traditional interpretations of predetermination—was forgotten. I refer to Mendel's work. Mendel showed that when two related organisms, differing from each other in a single character, are crossed, and their offspring are again bred together, that in the second generation individuals appear that are like their grandparents. He showed that the numerical proportions, in which they appear, could be explained on the assumption of one factor difference between the original forms. This result might be interpreted to mean either that the two original germ cells, taken as a whole, represent such a factor difference; or it might be interpreted to mean that the original germ cells had one particulate difference. But Mendel went further, and showed that when two related organisms that differ in two, or three, or more different characters are bred to each other, all possible combinations of the original characters appear later. It might seem then that we must abandon the view that each germ cell is to be thought of as a whole, for we see that the parts of each can be separated to become parts of others. In this sense Mendel's results seem to furnish a brilliant confirmation of Weismann's theory, in so far as it relates to preformation in the germ, and in the last edition of his "Vorträge ueber Descendenz Theorie," Weismann put in his claim to this verification.

In fact, Mendel's discovery does furnish a strong argument in favor of that part of Weismann's view that deals with the constitution of the germ-plasm, but it by no means confirms that part of Weismann's theory which postulates that embryonic development is a sorting-out process of representative particles.

Let us turn our attention, then, to Mendel's law and examine in how far it justifies an assumption that there are specific substances in the germ cells.

Mendel's law postulates that the early germ cells (and it may be added all of the body cells too) contain two of each kind of the

hereditary factors,—one derived from each of its parents. Mendel's law postulates further, that, in the ripening of the germ cells, the members of each pair separate (Fig. 1). Each mature germ cell comes to contain but a single element (or factor) of each kind.

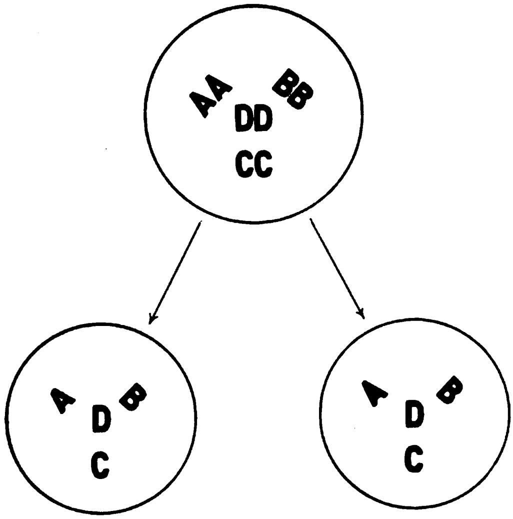


FIG. 1. Diagram to illustrate segregation of factors. The four pairs of factors represented in the upper circle by AA, BB, CC, DD, undergo segregation so that each germ cell comes to contain one member of each pair.

Now students of cytology had quite independently come to this same conclusion in regard to the germ cells. They had found that each cell contains a definite number of chromosomes, and that there are two of each kind of chromosomes in every cell,—one from each parent (Fig. 2, *a*). It had been found that at the ripening of the germ cells the members of each pair of chromosomes conjugate (Fig. 2, *b*), and then separate from each other (Fig. 2, *c*), so that each mature germ cell comes to contain but a single set of chromosomes (Fig. 2, *d*). Furthermore, students of experimental embryology had obtained independent evidence pointing to the chromosomes as the bearers of the hereditary materials.

We find, then, that cytologists had discovered a mechanism in the cell that they had reason to think was the bearer of the hereditary materials, and that the mechanism fulfills the essential

requirements of Mendel's postulates. There were two further steps necessary to bring the two lines of inquiry into complete accord; namely, (1) correspondence between the number of the chromosomes and the groups of inherited characters, and (2) the interchange between the members of the same pair of chromosome.

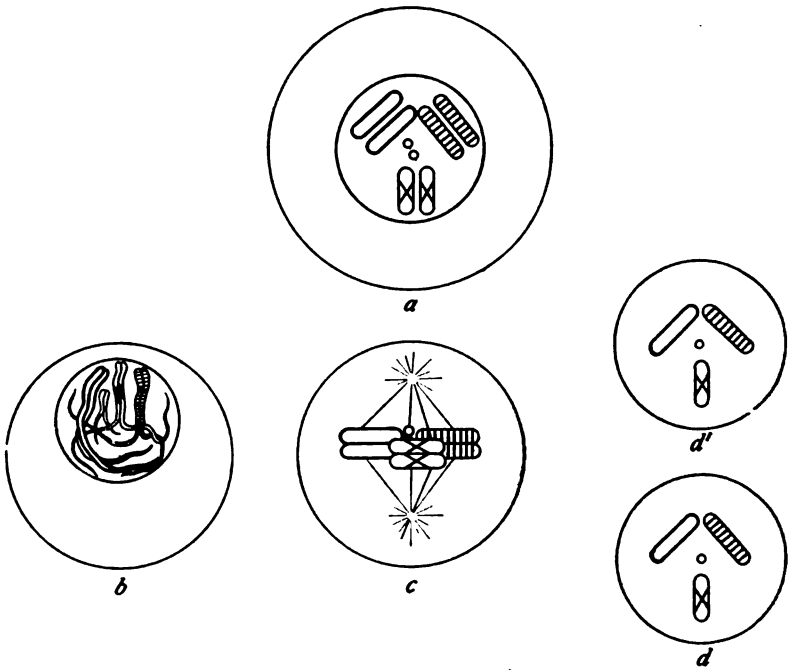


FIG. 2. Diagram to illustrate segregation of chromosomes. The four pairs of chromosomes in the upper circle (*a*), conjugate in (*b*) (synopsis stage), prepare for separation in (*c*) and undergo segregation so that each germ cell (d, d′) comes to contain one member of each pair.

The number of chromosomes is small in comparison with the large number of different characters that an animal or a plant possesses. We should expect therefore if in any animal or plant a sufficient number of character-differences were known that the characters would be found to be inherited in groups, and that the number of such groups should be the number of chromosome pairs that such an animal or plant possesses. In very few cases have enough characters been found to make such a comparison of any value.

But in the fruit fly, *Drosophila,* that has been intensively studied for five years, over a hundred new, and inherited characters have appeared. They fall into four great groups. A partial list of the four groups is as follows:

GROUP I.

Name.	Region Affected.
Abnormal	Abdomen
Bar	Eye
Bifid	Venation
Bow	Wing
Cherry	Eye color
Chrome	Body color
Cleft	Venation
Club	Wing
Depressed	Wing
Dotted	Thorax
Eosin	Eye color
Facet	Ommatidia
Forked	Spines
Furrowed	Eye
Fused	Venation
Green	Body color
Jaunty	Wing
Lemon	Body color
Lethals, 13	Die
Miniature	Wing
Notch	Venation
Reduplicated	Eye color
Ruby	Legs
Rudimentary	Wings
Sable	Body color
Shifted	Venation
Short	Wing
Skee	Wing
Spoon	Wing
Spot	Body color
Tan	Antenna
Truncate	Wing
Vermilion	Eye color
White	Eye color
Yellow	Body color

GROUP II.

Name.	Region Affected.
Antlered	Wing
Apterous	Wing
Arc	Wing
Balloon	Venation
Black	Body color
Blistered	Wing
Comma	Thorax mark
Confluent	Venation
Cream II	Eye color
Curved	Wing
Dachs	Legs
Extra vein.	Venation
Fringed	Wing
Jaunty	Wing
Limited	Abdominal band
Little crossover	II chromosome
Morula	Ommatidia
Olive	Body color
Plexus	Venation
Purple	Eye color
Speck	Thorax mark
Strap	Wing
Streak	Pattern
Trefoil	Pattern
Truncate	Wing
Vestigial	Wing

GROUP III.

Name.	Region Affected.
Band	Pattern
Beaded	Wing
Cream III	Eye color
Deformed	Eye
Dwarf	Size of body
Ebony	Body color
Giant	Size of body
Kidney	Eye
Low crossingover	III chromosome
Maroon	Eye color
Peach	Eye color

GROUP IV.

Name.	Region Affected.
Bent	Wing
Eyeless	Eye

GROUP III.—*Continued.*

Name.	Region Affected.
Pink	Eye color
Rough	Eye
Safranin	Eye color
Sepia	Eye color
Sooty	Body color
Spineless	Spines
Spread	Wing
Trident	Pattern
Truncate intensf.	Wing
Whitehead	Pattern
White ocelli	Simple eye

The four pairs of chromosomes of *Drosophila* are shown in the next diagram, Fig. 3.

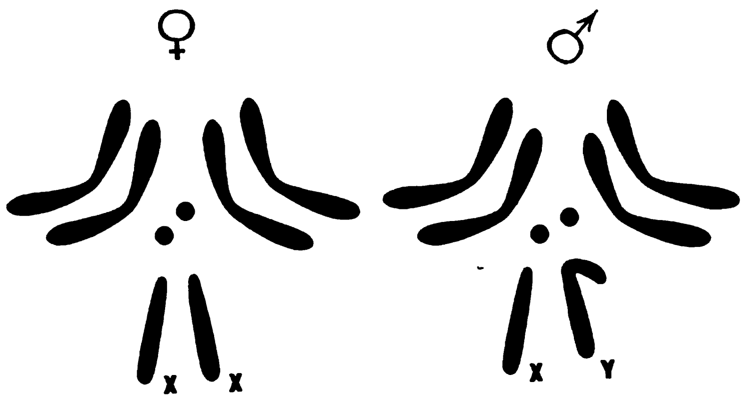


FIG. 3. Diagram of the four pairs of chromosomes of Drosophila ampelophila; to the left the chromosomes of the female; to the right those of the male.

The correspondence between the four character groups and the four pairs of chromosomes is obvious even to the size relations. This relation, or correspondence, does not however tell us anything in respect to the way in which the chromosomes stand for the characters of the group. So far, the result only shows that the characters of a given group are in some way represented in a particular chromosome. Our work has, however, carried us beyond this point. I may illustrate this by an example from the first group, containing sex linked characters. We mean by sex linked characters that they follow the known distribution of the X chromosomes. For in-

stance, the factor that determines the character for white eyes is sex linked, as is also the factor that determines the character for miniature wings. If we cross a female with white eyes and miniature wings to a male with red eyes and long wings, the sons will have white eyes and miniature wings. The explanation of this result is found in the distribution of the chromosomes. The sons get their single X chromosomes from their mother. Hence they show the characters that this chromosome carried in the mother, who had white eyes and miniature wings. The daughters, however, get one of their X chromosomes from their father through his female producing sperm. This chromosome carried a factor for red eyes and another for long wings, which factors dominate those carried by the other X chromosome that the daughters get from their mother, namely, the factors for white eyes and for miniature wings. These relations are shown in Fig. 4.

If these daughters and sons are bred to each other they produce four kinds of individuals, viz., red long, white miniature, red miniature, and white long. These are the four classes that Mendel's law calls for, but they do not occur in the Mendelian proportion (9:3::3:1) when two pairs of factors, as here, are involved. The reason for this is two-fold. In the first place the female alone carries two X chromosomes. The male carries but one. Hence there is an unequal distribution of the X chromosomes in the spermatozoa, for, only half of them can get an X chromosome. These are the female-producing spermatozoa. The result is, as has been shown, that in the first generation the sons inherit their single X chromosome from their mother and none of the dominant characters of the father. Since in this case the sons carry no dominant factor either in their X bearing (female producing), or in their Y bearing (male producing sperm), the second generation here reveals completely the composition of the egg cells that the F_1 female carries.

On Mendel's law of random assortment of two pairs of factors we should expect the four classes that here appear in the second generation to be equal in number. On the contrary we find that two of them are twice as numerous as the other two. On inspection we see that the two larger classes are white miniature and red

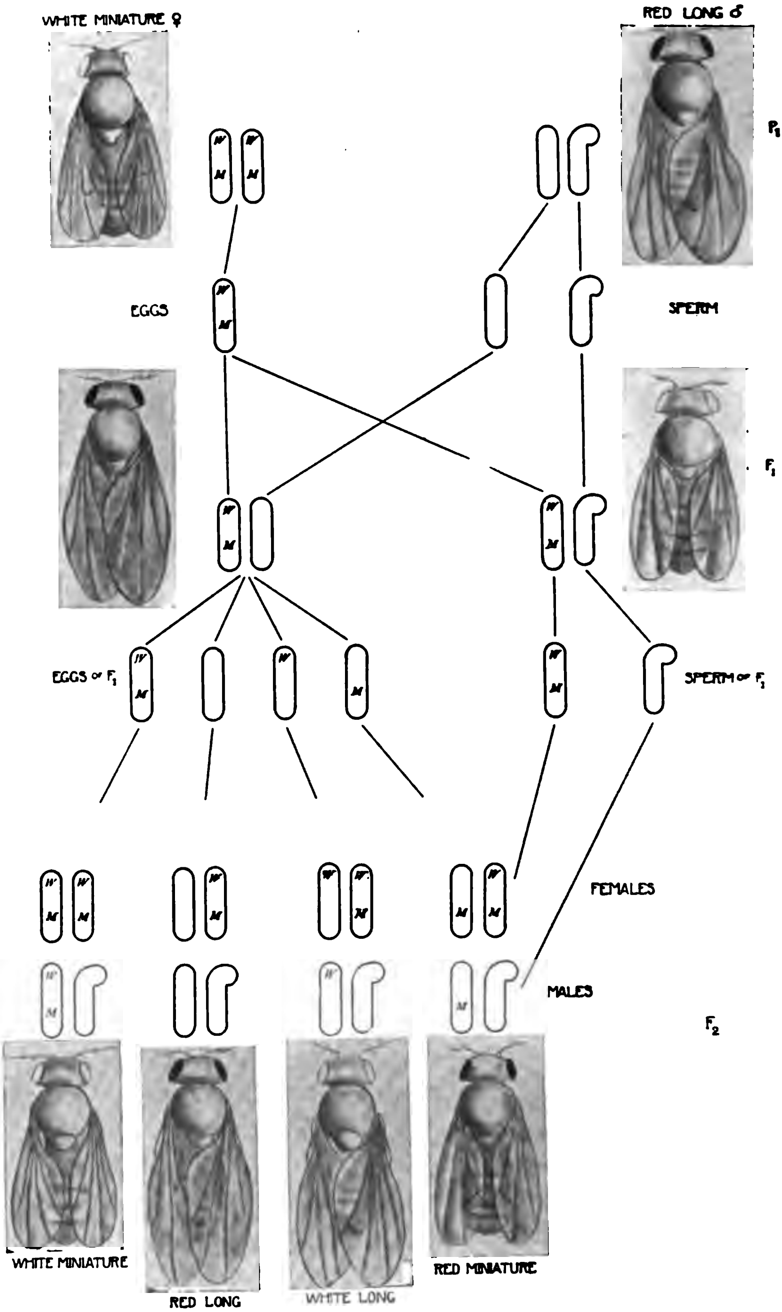


FIG. 4. Diagram to show the inheritance of two pairs of recessive sex linked characters, viz. white eyes (W) and miniature wings (M). The normal, dominant allelomorphs of these factors are omitted.

long. These correspond to the two grandparents. The two smaller classes are white long and red miniature.

We can account for this result if we assume first that the two factors that went in together in the same chromosome tend to hold together. This would account for the two larger classes. Second that the two smaller classes are due to interchanges between the two X chromosomes. Such interchange would here take place only once in three times.

We can test this conclusion by planning the experiment in such a way that white and miniature now go in from opposite sides,—white from one parent, and miniature from the other. When we do this we find that the large classes in the second (back cross) generation will be red miniature and white long and that the small classes will now be red long and white miniature. The ratio of the large to the small classes will be exactly the same as in the first case. In other words the interchange between the X chromosomes is the same regardless of what factors each contains.

If one admits that the chromosomes are the bearers of the hereditary factors he is forced to admit that experiments like these prove that somehow interchange of factors in homologous chromosomes must occur.

If one thinks of the factors as lying in a linear series in the chromosome (and there is certain evidence that I can not consider here that makes this view imperative) then the chance of a crossing over taking place somewhere in the region between two pairs of factors would be greater the farther apart the factors lie. The percentage of times that crossing over takes place becomes then a measure of the distance apart of the factors in question. If we make this assumption we find that we can give a consistent explanation of everything that we have found in the inheritance of linked factors in *Drosophila.* Not only this, but a far more important fact comes to light. If we determine, on the aforesaid basis, the relation to each other of all the known factors in each of the four groups, then, when a new factor appears, we need only determine its group and its relation to two factors in that group. With this information we can predict its relation to all other members of that group. In other words we can predict what the numerical relation

will be in the second generation. There is no other way as yet discovered by means of which this relation can be predicted.

If we compare our conception of the structure of the germ plasm with that of Weismann we find in all of his writings except the last one, that he supposed the chromosomes to be alike and that each consisted of a series of ids that contained the totality of the determiners that influence development.

It is true that in his last writing he partially abandons his earlier idea of *whole ids* for a conception nearer to ours of *partial ids*,—at least for some of the determiners. In this respect his view more nearly approaches the one here maintained. But even then his view not being based on numerical data would leave us entirely helpless in explaining the phenomena of inheritance in any particular case. Without wishing in the least to detract from the value of Weismann's brilliant speculation, nevertheless the difference in the way in which the conclusions were reached in the two cases is one of fundamental significance in all scientific work. Our view is based on accurate numerical data that enables us to predict what any given result in this field will be. It is this power to predict that gives significance to a scientific theory. In this regard we believe that our interpretation is a long step in advance of the purely imaginative conception of the germ plasm that Weismann advanced.

If now we bring our conception of the germ plasm to bear on the problem of development we have a very different view point of that process from the one Weismann pictured.

We think of every cell in the body containing one set of chromosomes received from the mother plus one set from the father. The materials carried by these chromosomes influence development in their entirety. Although we are able to localize certain materials in the chromosomes that when present cause the eyes to be white, and others that cause the eyes to be red, we do not mean that these materials in the chromosomes go directly only to the parts that show their influence more markedly. We mean that given one kind of material and the rest of the cell there is elaborated a white eye; given a different material in the same locus it produces, in conjunction with the rest of the cell, a red eye.

To say that the germinal material that makes a white eye is different from the germinal material that makes a red eye is a platitude. But to be able to locate a particular material in the one case *in relation to other materials* is a very different matter, because by means of this information we are able to explain the results on a mechanistic basis, and are able to predict the results of untried combinations. Without this information the prediction would be impossible.

We are led then to a third conception of predetermination. It is this! That while the hereditary material is made up of different discrete and separable particles (chemical substances) that have a definite position in the chromosomes, the effects of each of these particles must be supposed to be produced in combination with many, or even with all other parts of the cells in which they are contained.

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SPONTANEOUS GENERATION OF HEAT IN RECENTLY HARDENED STEEL.

By CHARLES F. BRUSH.

(*Read April 22, 1915.*)

Two or three years ago, when studying the behavior, under certain conditions, of several specimens of hardened tool steel, I observed that they all spontaneously generated a small quantity of heat, the amount of which diminished from day to day, but which was observable for several weeks. In each case the steel had been hardened only a few days prior to its use. It seemed highly probable that the generation of heat was associated with some sort of "seasoning" or incipient annealing process, perhaps accompanied by slight change of volume, and that it would be most rapid immediately after hardening. I resolved to investigate this curious phenomenon more fully, but failed to spare the time until a few months ago. This investigation forms the subject of the present paper.

Fig. 1 is a diagram of the apparatus employed. *A, B* represent two large silvered Dewar vacuum jars selected to have very nearly equal thermal insulating efficiency. They are supported in a wooden rack inside a thick copper cylinder *C* packed in granulated cork in a wooden box *E*. *D* is a paper extension of *C*, packed with layers of felt by removal of which and the loose copper cover of *C* easy access is had to the Dewar jars. The copper cylinder weighs 52 pounds and its functions are, by reason of its large thermal capacity and high conductivity, to protect the Dewar jars from any rapid change of temperature, and from temperature stratification.

The box *E* is surrounded by a much larger wooden box *F* lagged with a half-inch layer of felt. A long resistance wire is strung back and forth in the air space between the boxes at the bottom and four sides of *E*. Electric current controlled by a thermostat warms the wire, whereby the temperature of the air space may be maintained very nearly constant as many days or weeks as desired. A

thermometer T, easily read to hundredths of a degree, indicates the temperature of the air space.

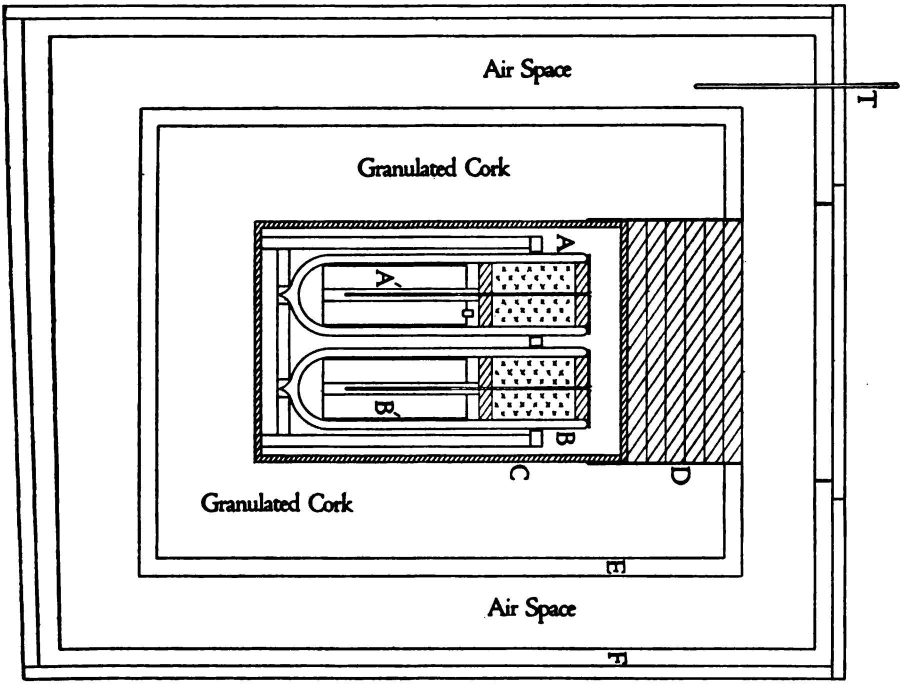


FIG. 1.

Returning now the core of the apparatus: A' is an air-tight cylinder of thin copper, six inches high and two and a half inches in diameter, provided with an open half-inch axial tube also of copper. A small round opening at the top of A' permits the introduction of a weighed quantity of water, after which the opening is tightly corked to prevent any change of temperature by evaporation of the water. B' is another copper cylinder just like A' except that it has a removable top to permit the introduction of the substance whose thermal behavior is to be investigated. The high thermal conductivity of these copper cylinders prevents temperature stratification within them. The Dewar jars are filled above the copper cylinders with layers of felt, and granulated cork, and covered with waxed cardboard carefully sealed on to prevent temperature dif-

ference inside the jars which would follow unequal loss or gain of moisture by the felt and granulated cork. A small thin glass tube, flanged at top and closed at bottom, is located in the axis of each Dewar jar and extends from the waxed cover nearly to the bottom of the inclosed copper cylinder. The glass tubes contain the ends of thermo-electric couples of fine constantan, copper and iron wires, one iron-constantan and one copper-constantan junction at the bottom of each tube. The leading-out wires are copper, and connect the thermo-couples with a reflecting galvanometer having the customary reading telescope and scale. Careful callibration has shown that 55 scale divisions of the galvanometer indicate one degree C. temperature-difference between A' and B', and that temperature-difference and galvanometer deflection are very closely proportional throughout the range used.

In the following experiment A' and B' were removed from the Dewar jars and allowed to attain equal room temperature. Twelve half-inch round bars of tool steel, five inches long and with machined surfaces, were hardened by heating to high "cherry-red" in a reducing atmosphere of a gas furnace and quenching in *cold* water. The bars then had a thin and strongly adhering coating of black oxide. They were next stirred in a large quantity of water at room temperature, to acquire that temperature, wiped dry, and oiled with heavy, neutral mineral oil to prevent generation of heat by further surface oxidation, wiped free of excess of oil and placed in the copper cylinder B'. A weighed quantity of water, also at room temperature, just sufficient to equal the steel bars in thermal capacity had already been placed in A'. The whole apparatus was then assembled as quickly as possible, and galvanometer readings commenced within forty-five minutes of the time of hardening the steel.

The upper curve in Fig. 2 shows the progress of heat generation in the steel bars during the first 150 hours after hardening. A very slow generation of heat was still easily observable at the end of a month.

It is seen that the temperature of the steel bars was rising rapidly when the galvanometer readings commenced, and reached a point (nearly 3° C. at the summit of the curve) where gain and loss of heat balanced each other in about 8 hours.

The "Normal Cooling" curve was obtained five or six weeks after the other, and when the generation of heat had very nearly

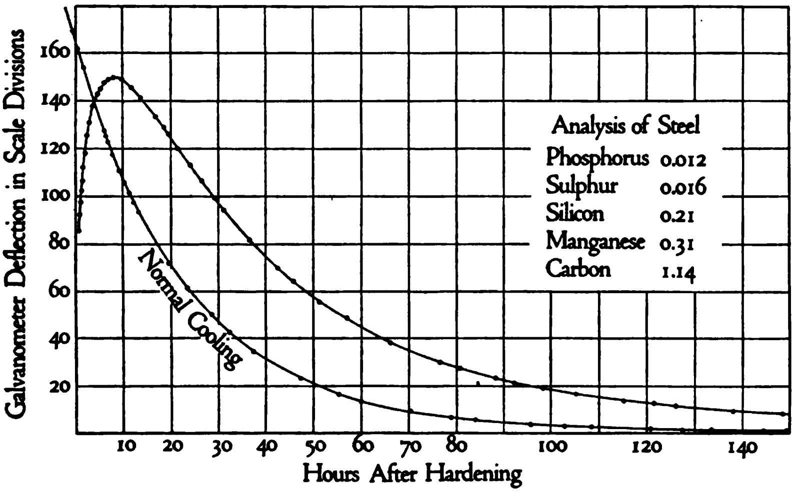


FIG. 2.

ceased. For this purpose the steel bars were removed, warmed a few degrees, and replaced; then galvanometer readings were made from time to time as before. This curve is plotted in a location convenient for visual comparison with the heating curve, but otherwise might just as well be plotted further to the right.

From the two observed curves I have computed a third curve (not shown) which represents the progressive rise in temperature which would have occurred if the thermal insulation of the steel had been perfect, so as to prevent any loss of heat. The curve is strikingly similar in character to the shrinkage curve shown in Fig. 5, and indicates a close association of heat generation and shrinking, to which I shall refer again. The total rise in temperature indicated (about five degrees C.) is of little quantitative importance because it is highly probable that it would have been different if the steel had been hardened at a different temperature, or more uniformly hardened throughout each bar, or had a different carbon content. Yet it is interesting to note that the observed quantity of heat spontaneously generated in the steel, measured by its rise in temperature multiplied by its thermal capacity, indicates internal

work of some sort sufficient to lift the steel bodily about 800 feet high against the force of gravity.

I next prepared a batch of "high-speed" tungsten steel consisting of the same number of bars of the same dimensions as in the first experiment. The bars were water-hardened at white heat, not far below the fusing point, brought to room temperature, oiled and introduced just as in the former case, and galvanometer readings were commenced an hour after hardening.

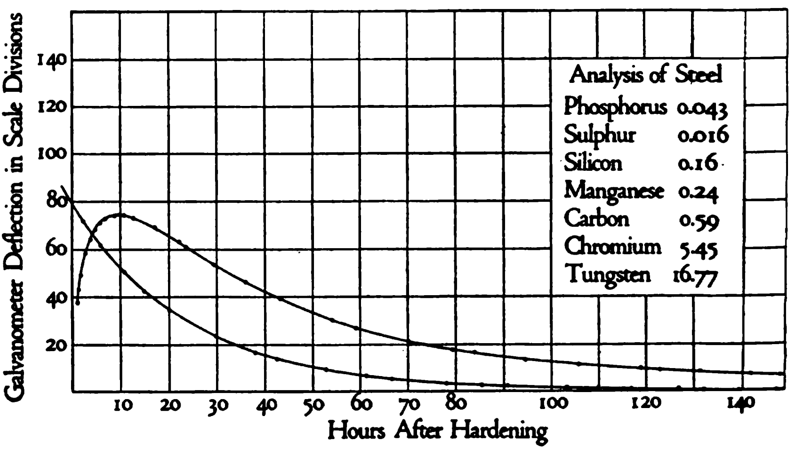


FIG. 3.

Fig. 3 shows the curve of heat generation in the "high-speed" steel, and the curve of normal cooling located with respect thereto as in Fig. 2. The cooling curve here shown is the lower part of that used in Fig. 2. It is permissible to use the same cooling curve for both kinds of steel because the thermal capacity of the two lots was very nearly the same.

It is seen that heat generation in the tungsten steel was the same in character as in the carbon steel of Fig. 2, though much less in amount and somewhat more persistent.

Many workers in steel are aware that the metal expands a little when hardened, and shrinks when annealed; but I have not met with any quantitative data on the subject. With the hope of throwing some light on the spontaneous generation of heat already described, I investigated this phenomenon of swelling and shrinking as follows:

Having no more of the carbon steel used in the first experiment, I procured another half-inch round bar of the same brand, though slightly different in composition as the analyses show. With a piece of this bar two and a half inches long I made a careful determination of its specific gravity under the conditions, and with the results, shown in the following table.

TABLE I.

Specific Gravity		Analysis of Steel	
Commercial Condition	7.8507	Phosphorus	0.015
After Hardening	7.8127	Sulphur	0.021
After Tempering to Light Blue	7.8350	Silicon	0.16
After Annealing	7.8529	Manganese	0.33
		Carbon	1.07

The difference in density and volume between the hardened and annealed conditions is fully a half per cent., which is much more than I expected to find; and nearly half of the total shrinkage was brought about by the very moderate heating necessary for "tempering to light blue." The annealing was very thorough, and, as the figures show, was more complete than in the annealed "commercial condition."

The shrinkage incident to tempering was large enought to encourage the hope that if any spontaneous shrinking, at room temperature, occurs during the generation of heat which follows hardening, it might be detected and measured. For this purpose the apparatus shown in Fig. 4 was designed and constructed.

In Fig. 4 *G* and *H* are two vertical steel rods three feet long and one millimeter in diameter. They are hung from a common rigid metal support *I*, and at their lower ends carry parallel brass bars *G′*, *H′* which move with perfect freedom, yet in close contact, between guides *K*, *K*. The brass bars are accurately machined, and their front edges are polished. The rod *G*, whose function is purely comparative, is kept under moderate and constant tension by the long spiral spring *L*; while the rod *H* carries a four pound weight *M*. An enlarged sectional diagram at the right shows the method employed in mounting each steel rod. Each end of the rod passes through, and is soldered into, a brass head having a hemi-

spherical end which accurately seats itself in a hollow metal cone. The rods are quickly removable through vertical slots in the cones.

After some preliminary experiments, to get acquainted with the apparatus, a fresh rod *H* was hardened by placing it horizontally in a wooden rack just above a trough of water at room temperature, quickly heating it to bright redness by passing suitable electric

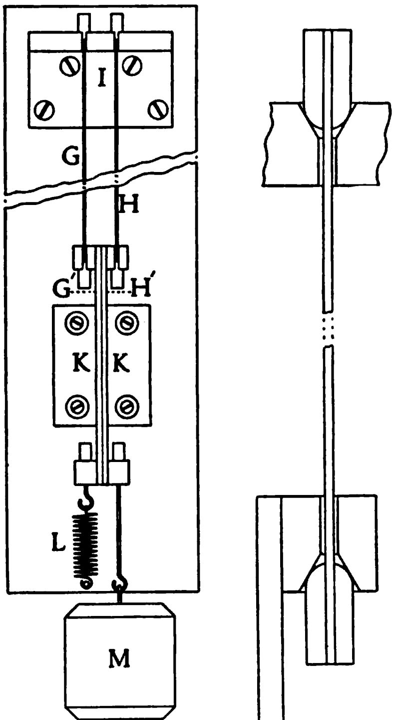


FIG. 4.

current through it and plunging it in the water beneath, the act of lowering the rod serving to break the electric circuit. The rod was kept straight while hot by means of a weak spiral spring which took up the expansion. Preliminary experiments had shown that a rod could be hardened in this way without warping.

The hardened rod, already at room temperature, was quickly wiped dry and put in place beside *G*. Then, without delay, a fine

horizontal scratch was drawn across the polished fronts of the bars G', H' by means of a straight-edge and sharp needle point lightly applied. A microscope, magnifying about 200 diameters and very solidly mounted, was brought into position and focused on the horizontal scratch, which of course consisted of an independent scratch on each bar, the two halves being initially in perfect register. The microscope was provided with a filar micrometer eyepiece carefully calibrated and adapted to measure accurately any departure from register of the two half lines or scratches.

Shrinkage of the hardened rod H was detected within two minutes after scratching the brass bars, and was easily observable at the end of two weeks.

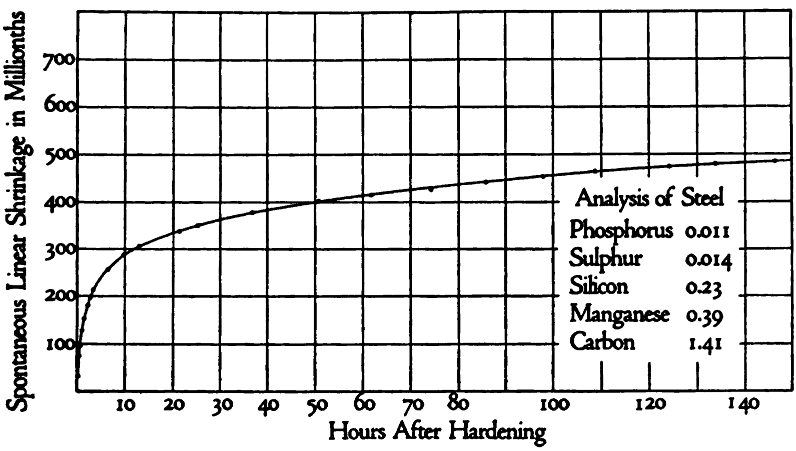


FIG. 5.

Fig. 5 shows the progress of shrinking during the first 150 hours. The curve reached the 500 line a day or two later. The hardened length of the rod was assumed to be 35 inches, so that its actual shrinkage at the 500 line of the curve was 0.0175 inch.

The rod was next scoured clean and tempered to light straw color by electric warming, then to light blue color, and its total shrinkage measured after each operation. Finally, it was thoroughly annealed by bedding in mineral wool, heating to very low redness half an hour, and then gradually reducing the heating current to nothing in the course of two or three hours, after which

the shrinkage was again measured. The rod shrank very considerably in each operation, as indicated quantitatively in Table 2, in which the annealed length is taken as unity or 100 per cent.

TABLE 2.

Length of rod after hardening	100.383
After spontaneous shrinking	100.332
After tempering to light straw	100.182
After tempering to light blue	100.131
After annealing	100.000

Of course the shrinkage in volume must have been very nearly three times the linear shrinkage, or considerably more than one per cent. from the hardened to the annealed condition, which is more than double that observed in the bar steel used in the first experiment. Doubtless this was due to the higher carbon content of the small rod, and more uniform hardening owing to its small size. It is highly probable also that more heat was generated per unit of mass.

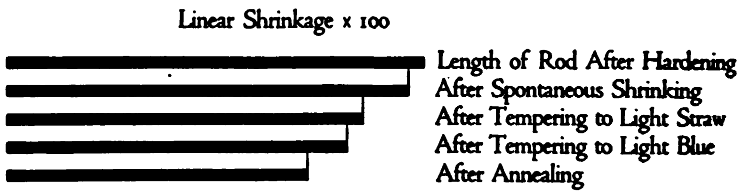


FIG. 6.

In Fig. 6 I have visualized the stages of shrinking of the small rod by magnifying a hundred-fold the observed quantities in Table 2.

I have already pointed out the close similarity in character of the spontaneous-shrinkage curve (Fig. 5) and the computed curve of total heat generation; and there seems little room for doubt that the two phenomena are quantitatively related. But it is equally clear that spontaneous shrinking is only incident to, and is not the prime cause of the generation of heat, because the internal work represented by the heat generated is hundreds of times more than necessary to bring about the accompanying change in volume. This

is found as follows: The small steel rod spontaneously shrank 0.0175 inch. To spring it back to its original length required a weight of 15 pounds hung below *M*, Fig. 4 (= 12.400 pounds strain per square inch of cross-section). Hence, in longitudinally shrinking 0.0175 inch, the rod had done work equal to lifting 15 pounds half this distance or 0.00875 inch. The rod weighed about 1230 times less than the weight, so that the work done was sufficient to lift the rod itself $1230 \times .00875 = 10.76$ inches. But this represents one-dimensional shrinking only, and we must take three times this amount of lift, or, say 2⅔ feet, to represent the work done in the three-dimensional shrinking which certainly occurred. We have already seen that the internal work spontaneously done in the steel bars of the first experiment, in generating the observed amount of heat, was sufficient to lift the bars about 800 feet, which is 300 times greater than the work done in spontaneously shrinking the small rod. If spontaneous shrinkage was less in the large bars than in the small rod, which is highly probable, then this ratio was accordingly greater than three hundred to one. The disparity in weight between the twelve large bars and the one small rod does not count, because the work done in each case is computed for the weight of steel which did it.

It has been suggested that loss of the generated heat may perhaps be regarded as a cooling process without change of temperature (which implies reduction in specific heat), and that this may be sufficient to account for the spontaneous shrinkage. But this hypothesis accounts for only a modest fraction of the shrinkage; while the implied change in specific heat is much too large to be admissible.

An attempt was made to measure Young's modulus of elasticity in the small rod both in the hardened condition (after spontaneous shrinking) and after annealing, by hanging various weights below *M*, Fig. 4, and measuring with the microscope the distortions produced,—always far within the elastic limit. But I was unable to obtain reliable results because of an interesting fact which was brought to light, as follows: In the annealed condition the steel exhibited a small amount of viscosity or internal friction which somewhat delayed full distortion and subsequent restitution; but in the

hardened condition the viscosity was *many times greater*. This is a further illustration of the instability of the hardened steel.

In conclusion, I am led to regard the hardened steel as being in a condition of very great molecular strain somewhat unstable, especially at first. Spontaneous relief of a small portion of the strain causes generation of heat until stability at room temperature is reached. Any considerable rise of temperature, as in tempering, permits further spontaneous relief of strain, or molecular rearrangement, doubtless accompanied by more generation of heat, and so on until annealing temperature is reached. It is obvious that the process of tempering or annealing steel is an exothermic one, and conversely that hardening is an endothermic process.

CLEVELAND,
April, 1915.

RELATIONSHIPS OF THE WHITE OAKS OF EASTERN NORTH AMERICA,

WITH AN INTRODUCTORY SKETCH OF THEIR PHYLOGENETIC HISTORY.[1]

BY MARGARET V. COBB.

(PLATES IV–VI.)

(Read April 23, 1915.)

I. HISTORY OF THE FAGACEÆ: A RECONSTRUCTION.

Prantl's Classification of the Fagaceæ.

Castaneæ	Quercus.
	Pasania.
	Castanea.
Fageæ	Fagus.
	Nothofagus.

The five or six genera of the family Fagaceæ to which the oaks belong were well differentiated at least as far back as the Cretaceous age. The beeches are sharply separated from the remainder of the family (the pasanias, chestnuts and oaks), and are undoubtedly the more primitive of the two groups. *Nothofagus,* the genus of primitive beeches, is a characteristically sub-Antarctic genus, still surviving in Tasmania, New Zealand, and the southern part of South America (a South Pacific distribution). *Fagus* itself, once more widely spread, is now found only in Japan, North America and Europe.

The pasanias, chestnuts and oaks are at present in possession of the temperate and tropical regions of Asia, North America, Europe and Mediterranean Africa. Species are most numerous in southeast Asia and in Mexico (regions separated by the Pacific). *Pasania* is limited to southeast Asia, except for one species in California

[1] This paper owes a great deal to the extensive knowledge and the never-failing interest and aid of Dr. William Trelease, under whom the work was done at the University of Illinois in the year 1913–14.

and one in New Zealand (ranges separated by the Pacific). *Castanopsis* (the less specialized chestnuts) is limited to southeast Asia, except for two Californian species (ranges separated by the Pacific). *Castanea* is present in southeast Asia, North America and Europe. *Quercus* has most numerous species in southeast Asia and (especially) Mexico and Central America (regions separated, again, by the Pacific), while the subgenus *Cyclobalanopsis* is limited to southeast Asia (monsoon province). In consideration of the facts that the most primitive genus still lingers on the two sides of the southern Pacific, and that so many other groups are found only in regions bordering on the northern Pacific, it is more than plausible that the family Fagaceæ originated in the Antarctic-Pacific region, and moved northward towards its present northern-hemisphere distribution in the region of the Pacific Ocean. This of course involves the hypothesis of an ancient Cretaceous or pre-Cretaceous Pacific continent—for which there is much other distributional evidence and which Scharff,[2] among others, holds to be highly probable. The broad similarity of the ranges of *Pasania, Castanopsis* and *Cyclobalanopsis* was undoubtedly determined at this early time. The problem of the extension of certain species of *Fagus* and *Castanea* to Europe seems entirely separate, and probably belongs to a more recent period. *Quercus* is involved with both the older and the more modern distribution; they have been mapped out here for convenient reference in the coming discussion of *Quercus.*

II. History of Quercus, Hypothetically Reconstructed.

Oaks, living or fossil, have been reported from every continent. Living species, however, are unknown in the southern hemisphere, except that they are found south of the equator in the East Indies, and among the mountains of Ecuador (localities separated by the Pacific). Species, as was said, are most numerous in Mexico and Central America and in southeast Asia; the subgroup *Cyclobalanopsis* is limited to southeast Asia. Remembering that *Pasania* and *Castanopsis* are almost limited to the same region, and that the pasania-chestnut-oak group of the Fagaceæ shows here a concentration, and a profusion of species, seen nowhere else in the world,

[2] Scharff, "Distribution and Origin of Life in North America."

it is natural to suppose that this part of Asia (or more probably, to allow for the outlying species in California, and the oaks in Mexico, a region *east* from southeast Asia) has been the center of distribution, and hence the point of origin of the pasania-chestnut-oak group. And *Quercus* itself, with its black oaks limited to America, its *Cyclobalanopsis* limited to southeast Asia, and its numerous white oak species in both places, undoubtedly differentiated from the pasanias (or their ancestors) in one or other of these regions, or more probably between the two. At any rate, the primitive, little-differentiated *Quercus* must have had a distribution that included both regions, as well as the space between them. We are thus brought again to an hypothetical Pacific continent; for since neither black oak nor *Cyclobalanopsis* exists or gives evidence of having existed in western Asia or Europe, any cretaceous or earlier connection of the two regions in that direction is well-nigh inconceivable. (It is unnecessary to suppose that this Pacific land extended much farther north than the equator).

According to our hypothesis, the disappearance of this Pacific land isolated the two extremes of the range of *Quercus*. The genus had already become differentiated; the Asiatic part of the range received the stock of *Cyclobalanopsis* (found nowhere else) as well as the more typical *Quercus* stock. Certain species of *Quercus,* even today, form a part of the oldest Asiatic flora, which holds its own in isolated regions,—in parts of the Himalayas, for instance. Some of these ancient endemic species are the white oaks *Q. lanata, semecarpifolia,* and *dilatata,* of which the last is said by Schottky to stand nearest of all oaks to the *Cyclobalanopsis* group. (American black oaks, however, show certain features in common with *Cyclobalanopsis*—apical ovules, type of style).

The American end of the range received a group of oaks of which (according to evidence from distribution and palaeontology) *Quercus chrysolepis* is probably our nearest representative; these may have been the basis of both the black and the white oaks of America. It is suggestive to find that *Q. semecarpifolia* (representative of the ancient oaks of Asia) bears some resemblance to this early American oak. Some of the European oaks are also of this ancient type; but since one, *Q. Ilex,* occurs in both Asia and Europe,

the inference is that they all reached Europe westward from Asia. Though the older fossil evidences in this continent have all been referred to *Q. chrysolepis* (these date back to the Cretaceous), it seems not improbable that types such as *Q. emoryi* and *Q. hypoleuca* were soon present, and that differentiation early took the lines towards our American *black oaks* and *white oaks*. Since in *Cyclobalanopsis,* and in the pasanias, the abortive ovules are carried upward in growth till in the mature acorn they are typically apical, this may be considered the primitive condition in *Quercus*. *Chrysolepis,* which has them only lateral, is on the way towards having them in the basal, white-oak, position. The black oaks, on the contrary, have preserved the primitive character in this as in other particulars.

(Since the black oaks resemble *Cyclobalanopsis* in some ways, it may be that they differentiated from *Cyclobalanopsis,* in the Pacific region, before reaching America. Or all three may have diverged together from the primitive *Quercus*. Distribution may have been such that *Cyclobalanopsis* went to Asia, *Erythrobalanus* to America, *Lepidobalanus* to both.)

Having thus some conception of a possible Cretaceous history for American oaks, black and white, and of their relationship to the ancient types of Old World oaks, we may now limit ourselves to the white oak group in North America (*Leucobalanus*). For the black oaks, being limited to the western hemisphere and becoming only more sharply differentiated, can give us no further light on white oak relationships. To begin with, we may mark off *Leucobalanus* as follows:

QUERCUS.

Cyclobalanopsis: Abortive ovules apical, styles short, subcapitate, often recurved, cup scales grown into a solid ring, fruit ripening in one year, leaves evergreen, tertiary nerves very fine.

Erythrobalanus: Abortive ovules apical, styles elongated, subcapitate, often recurved, acorn tomentose within, cup scales thin, appressed, fruit ripening in two years, leaves deciduous or evergreen, lobes when present with bristle points.

Styles slender or very short and flattened, not cephalated at apex. *Lepidobalanus.*

Cerris: Abortive ovules basal, styles long, tapering, cup scales often long, bractlike, fruit ripening in two years, leaves more or less dentate.

Leucobalanus: Abortive ovules basal, styles very short, spatulate, acorn not tomentose within, cup scales often thickened at base, fruit ripening in one year, leaves deciduous or evergreen, lobes when present rounded.

The most stable characters in this classification seem to be the position of the abortive ovules, the lining of the acorn shell and the form of the style. Appression of scales, time for ripening fruit, and time of keeping leaves are all more or less variable among the white oaks.

The earliest home of *Leucobalanus* on this continent, using the term to include the white oaks as they separated themselves from the black oaks in America, seems to have been northern Mexico and the southwestern states. The older type (A. below) still predominates in this region, which has probably long been stable, with a climate similar to the present. It is a region which seems to have been for many species a center of distribution to other parts of the continent. Since the Cretaceous, much differentiation has taken place, the main lines of which may be represented by the following division of North American white oaks:

A. Leaves persistent, usually evergreen, entire, sinuate or dentate, or, if deeper lobed, with pungent tips.
  1. Many species, southwestern U. S. and Mexico.
  2. *Virginiana* and varieties—an early offshoot.

B. Leaves deciduous, lobed or divided, or serrate; lobes rounded, obtuse or acute but not pungent.

The evergreen series, represented, say, by *Q. undulata,* is the more direct continuation of the Cretaceous type, the deciduous the more modern form.

It is barely possible that not all of this differentiation took place on this continent. *Leucobalanus* reached Europe at some time; and the possibility that this took place early (by means of Scharff's Mediterranean land bridge), and that the deciduous oaks originated there, rather than on this continent, must be taken into account. Species of this type occur also in Asia, but there seems to be little doubt that they are sharply separated from the ancient Asiatic species like *semecarpifolia,* and reached Asia in the Tertiary from the eastward. The fact that the range of these species, in the Ter-

tiary, was, at the boreal end, continuous from Asia across America to Europe, gives the possibility of the center of distribution being either in Europe or in America. My data on European oaks are insufficient to decide this point; it seems, however, highly probable that the white oaks with thin, deciduous, lobed leaves originated in or near northern Mexico.

The early members of the group *Leucobalanus*, then, marked by entire, evergreen leaves, gave rise, probably in North America, to a form with thin, deciduous, lobed leaves. This type is now dominant over the greater part of the United States, while the older form holds its own in the southwest and in Mexico, where the climate has probably known no great fluctuations since the Cretaceous, and where it still finds suitable dry and arid habitats. This evergreen type occupies the Mexican highlands, Arizona and New Mexico, extending east into Arkansas, and west into California. *Quercus virginiana* seems also to have been a very early offshoot; with its varieties it forms a well-marked coastal group, ranging from North Carolina south along the shores of the Gulf into Mexico (where it stretches inland up the mountain sides), and appearing also on the California coast.

III. Deciduous White Oaks of North America.

The oaks with which we are familiar in this part of the country are of the lobed-leaf type. Geographically, at least, there are three parts to this group,—the eastern, the Rocky Mountain, and the Californian lobed-leaf oaks. It is not clear, however, whether or not these geographical groups can be separated taxonomically. They may be parallel groups, cut off from one another comparatively recently; or, possibly, the Californian group may be more closely related to the deciduous oaks of Europe (type *Q. robur*) than it is to the oaks of the Rocky Mountains and the east. The habit, leaf form and texture, and bud form of the Californian oaks have suggestive resemblances to those of the English oak; and it is perhaps not venturing too much to speculate as to whether these oaks, like certain other forms on our Pacific slope, may not have their closest relatives, not in America at all, but in Europe. There is besides at least one oak in California, *Q. sadleriana*, which appears to find its

nearest relatives in the modern Asiatic oaks, which were mentioned as having probably reached Asia in Tertiary times from the eastward. The *gambelii* group in the Rockies and the Atlantic group are apparently the separated branches of the latest developed white oaks (and the Californian oaks are perhaps a third corresponding group), which before glaciation may have succeeded in covering the greater part of the continent. Glaciation left survivors of this forest, it would seem, in two parts of the land—mountainous regions which projected above the ice—the southern Rockies, and the southern Alleghanies. From the one *Q. gambelii* has spread northward, keeping rather closely to the mountains and differentiating numerous but similar species; while from the other the early species (possibly *lyratiformis* and *minor*) have recovered an enormous stretch of territory, and have produced a correspondingly large number of varied species.

IV. White Oaks of Eastern North America.

The white oaks found east of the Rocky Mountains comprise the following species (see key):

1. *breviloba* *durandii*	2. *lyrata* *bicolor* *macrocarpa*
3. *chapmani* *minor* *margaretta* *alba*	4. *michauxii* *prinus* *muhlenbergii* group.

These species are all of the deciduous, thin-leaved type of *Leucobalanus*, except that *durandii* and *breviloba*, in ranging from Alabama west and south into northern Mexico, show a series of transitions towards the smaller, more entire, evergreen type of leaf. It might be that a careful study of these forms would show them to be transitional in other features also. Their range seems to indicate an ancient center of distribution in the southwest; this again is in sharp contrast to all the other species, which may be referred to a more recent center in the southeast. In short, there seem to be

several reasons for marking off rather sharply *durandii* and *breviloba* from the remainder of the species present in this area, and for suggesting the possibility that they may be a relic from the time of the differentiation of this deciduous section of *Leucobalanus*.

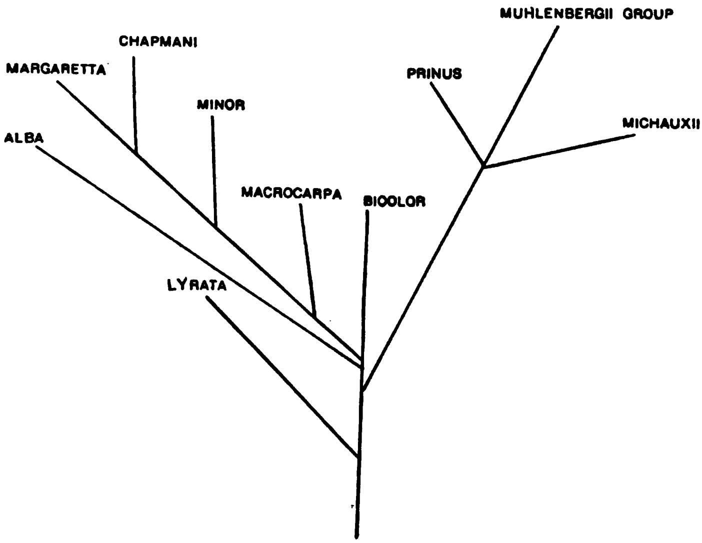
The remainder of the group has a very wide range. It touches the Rockies in Canada, and reaches Texas, Florida, and Maine. Nevertheless, it is almost true to say that every one of the species includes in its range the region of the southern Alleghanies. This region certainly seems to have been a center of distribution after the retreat of the ice fields, for this as well as for certain other groups of plants and animals (*Cambarus*, and the Unionidæ, for instance). The present distribution must have been largely achieved by the Pleistocene, for late Pleistocene fossils indicate a range broadly similar to that of the present.

The species, aside from (1) *durandii* and *breviloba*, fall into three main groups—(2) *macrocarpa* group, (3) *minor* group, (4) *prinus* group. Their relation to one another is not entirely clear. The *macrocarpa* group in some ways holds a central position, which suggests that it may be the oldest. So do the *persistent stipules* of all members of the group; this is without any doubt a primitive character. Its species moreover have the widest range, *macrocarpa* extending in the north to Saskatchewan and Maine, and in a great southward curve with its lowest point well down the Mississippi Valley; south of this it is replaced by *lyrata*. Again, Tertiary leaf-prints which have been referred to deciduous *Quercus* are limited thus far to types resembling *lyrata* and *minor*. (Cockerell's species *lyratiformis* from the Florissant beds is now reported from the John Day Basin, Oregon, where Knowlton also recognizes leaves of the type of *minor*.) There are so many suggestions of this sort that at present we must assume the *macrocarpa* group to be nearest to the ancestral type; and, though the fruit is aberrant, *lyrata* may well stand near the base of the group.

The *minor* group, or at least *minor* itself, has some affinities with *bicolor* and *macrocarpa*. Its wide range and the Tertiary occurrence of this or a similar species show that it has valid claims to antiquity. Whether *alba* belongs in this group is uncertain; it is difficult to see reasons for connecting it closely with any other species. *Mar-*

garetta, regarded by some as a good species, but which has often been regarded as an *alba-minor* hybrid, suggests such a relationship, but this is more or less doubtful.

The clearest and most highly differentiated group is that of the chestnut oaks. It may be connected with the more typical forms through forms such as *bicolor* (shape of leaf) and *lyrata* (bud-scales). That the serrate leaf is secondarily derived, through a lobed form, and not a persistence of the serration found in older portions of the genus is perhaps not proven; the tendency to lobation rather than serration on young shoots, as well as the general relation of the chestnut oaks to the other oaks of this region make it, however, highly probable.



The above diagram may make more concrete these suggestions concerning relationships.

Key to Deciduous White Oaks of Eastern North America.

Leaves deciduous, lobed or dentate, not spinulose.

I. Leaves lobed.
- *A*. Stipules persistent; buds more or less acute.
  - 1. Twigs slender, smooth. *Lyrata.*
  - 2. Twigs stout, pubescent.
    - *a*. Fruit sessile, larger; cup usually deeper and fringed. *Macrocarpa.*
    - *b*. Fruit pedunculate, smaller; cup more shallow, seldom fringed. *Bicolor.*
- *B*. Stipules deciduous; buds rounded.
  - 1. Twigs smooth. *Alba.*
  - 2. Twigs pubescent.
    - *a*. Leaves deeply five-lobed, pubescent below. *Minor.*
    - *b*. Leaves undulate, glabrous below *Chapmani.*

II. Leaves dentate.
- *A*. Buds less elongate, leaves narrower, widest near middle. *Muhlenbergii.*
- *B*. Buds more elongate, leaves broader, widest above middle.
  - 1. Cup scales free at tips only; upper scales very small. *Prinus.*
  - 2. Cup scales free; upper scales often forming a fringe to cup. *Michauxii.*

Description of Plates.

Plate IV. Buds of the rounded type, without stipules. × 3.

Fig. 1. Q. alba (Urbana, Illinois).
Fig. 2. *Q. minor* (collected by H. H. Bartlett, Maryland).

Plate V. Buds of the more acute type, stipules persistent. × 3.

Fig. 1. *Q. macrocarpa* (Urbana, Illinois).
Fig. 2. *Q. bicolor* (Urbana, Illinois).

Plate VI. Buds of the elongated, chestnut oak type. × 3.

Fig. 1. *Q. prinus* (collected by H. H. Bartlett, Maryland).

FIG. 1.

FIG. 2.

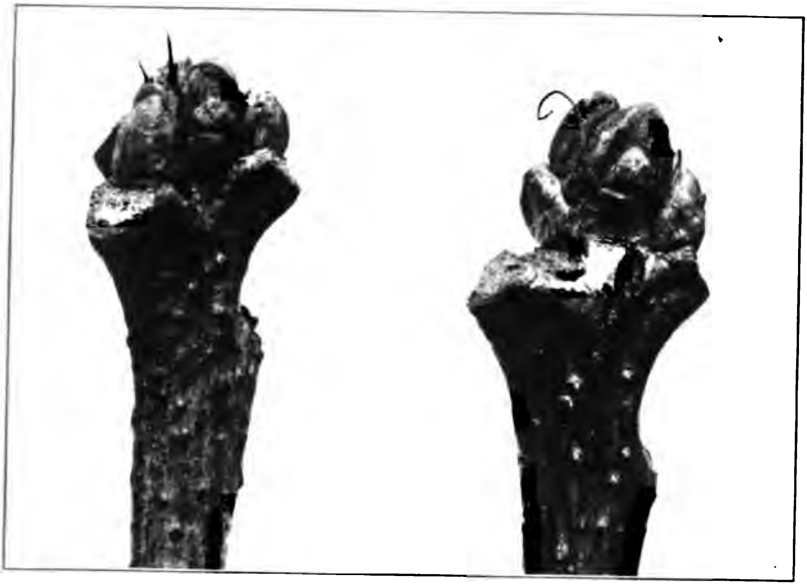


FIG. 1.

FIG. 2.

FIG. 1.

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A NEW FORM OF NEPHELOMETER.

By J. T. W. MARSHALL and H. W. BANKS, 3D.

(Read April 23, 1915.)

The nephelometer (Gr. νεφέλη, a cloud), an instrument for the quantitative determination of small amounts of material in suspension, has attracted considerable attention of late, although the principles involved are by no means new. Since the time of Gay-Lussac attempts have been made to estimate small quantities of material by the turbidity or opalescence of their suspensions. This was generally done by comparing the suspension with a graded series of known suspensions prepared in the same way, and the comparison was made by looking through a column of the liquid and noting the turbidity, or by observing the opalescence, that is, the light reflected from the minute particles when the liquid is illuminated by a powerful beam of light. It is evident that matter in smaller quantities or in a finer state of subdivision may be recognized more easily by the opalescence than by the turbidity of its suspension. That even excessively minute particles possess the ability to diffract light has been shown by the ultramicroscope, while by the Faraday-Tyndall convergent beam of light, the optical in-homogeneity of solutions of crystalloids has been detected.

T. W. Richards in the course of atomic weight determinations in 1894[1] devised a simple instrument to enable the opalescence of very dilute suspensions of silver bromide to be more readily observed, and in a measure, quantitatively determined. Ten years later, Richards and Wells[2] improved the instrument optically and suggested its applicability to suspensions of substances other than the silver halides. Their actual determinations, however, seem to have been arrived at by a process of approximation; that is, the unknown was compared in the instrument to a suspension of known concentration, and from these readings a first approximation of its strength was calculated. A new standard of more nearly the same concentration as the unknown was then prepared

[1] *Proc. Am. Acad.*, XXX., 369, 1894.

[2] Richards and Wells, *Am. Chem. Jour.*, XXXI., 235, 1904.

and comparison again made. This process was repeated until a standard was obtained which when precipitated under the same conditions and compared in the instrument with the unknown gave the same amount of opalescence. The postulate involved, that the same quantities of material precipitated under identical conditions give equal opalescences, is undoubtedly correct, but the method is somewhat tedious in application, although good accuracy was obtained in about three approximations.

Wells in 1906[3] published the results of numerous experiments in which silver chloride was precipitated under different conditions, showing the influence of electrolytes both on the maximum opalescence developed and on the time required for this maximum to be reached. He came to the natural conclusion that the amount of light reflected varies not only with the quantity of material in suspension but also with its state of subdivision. In this investigation he used the Richards instrument of 1904 except that for the usual standard suspension he substituted fixed standards of ground glass as reflecting surfaces.

P. A. Kober[4] in 1913 took up the problem of determining quantitatively by the use of the nephelometric method, proteins and other substances occurring in biochemical investigations for which the ordinary gravimetric methods are either very tedious or inadequate. He used an instrument on the principle of the Richards nephelometer but adapted to the framework and optical parts of the Duboscq colorimeter. In comparing the opalescences of suspensions differing considerably in concentration, he observed that the readings were not quite inversely proportional to the concentration of matter in suspension, and from a large number of experiments with suspensions of different substances he developed an empirical formula expressing the relation between scale readings and concentration. This formula holds very well for ratios up to 1:3. He has successfully applied his instrument and method to the determination of a number of organic substances such as casein in milk, uric acid, and other purines. The nephelometer in various modifications has been used by W. R. Bloor to determine the fat

[3] Wells, *Am. Chem. Jour.*, XXXV., 99, 1906.

[4] P. A. Kober, *Jour. Biol. Chem.*, XIII., 485, 1913.

in blood, by McKim Marriot for acetone, and by S. S. Graves in ammonia determinations.

A number of instruments and methods have been devised for determining the amount of substance in suspension by the turbidity of its solution and these find considerable use in industrial chemistry. While the theory underlying this method is undoubtedly simpler than the nephelometric theory, it may easily be seen from the following considerations that the turbidimeter cannot equal the nephelometer in delicacy or sensitivity. Let us suppose that a standard as used in the turbidimeter absorbs about 10 per cent. of the light, then an unknown of twice the concentration will absorb about twice that quantity. However, it is not the amount of light absorbed, but the amount transmitted that is observed in this instrument; consequently the quantities measured would be in the ratio of about 9:8. The reflected lights measured in the nephelometer on the other hand would be nearly in the ratio of 1:2. Clouds which may be measured with considerable accuracy in the nephelometer show very slight absorption when observed by transmitted light in the turbidimeter.

Our reason for devising a new nephelometer may be made more apparent by a brief review of some of the considerations involved in the use of such instruments. The following are the chief factors involved in the amount of light reflected by an opalescent solution. First, the amount of substance in suspension. Second, its physical state, *i. e.*, the number and size of the particles, and their albedo which depends upon their own refractive index and that of the medium in which they are suspended. The amount of light observed is again modified by the fact that the light from any particle is reduced by an amount dependent upon the absorbing power of that part of the liquid above the particle. Thus we receive less light from the bottom layers of the suspension than from those nearer the top. This complex relation between reflection and absorption demands less consideration when the lengths of the illuminated columns are kept equal than when they are varied. As far as we are aware, in the nephelometers hitherto described the light from the two tubes has been equalized by changing the lengths of the illuminated columns of suspension. Although in purely

empirical work the elimination of this factor is not of very great importance, the theoretical consideration of the problem is greatly simplified thereby.

As Wells states, the opalescence of a liquid containing a definite amount of substance in suspension will, owing to the greater total reflecting surface, increase with the continued subdivision of the particles until these reach a limiting size. Rayleigh has pointed out this fact in a mathematical dissertation on the blue color of the sky, stating that as the particles approach the size of a wave length of light their reflecting power decreases. He shows that for very minute particles the amount of light reflected should vary as the sixth power of their radius. The maximum opalescence of the solutions as used in a nephelometer seems, however, to be developed when the particles are much smaller than a wave length of light—in fact of ultramicroscopic size.

The amount of reflected light lost through absorption is also a function of the number and size of the particles.

It is evident that as the refractive index of the medium approaches that of the particles, the amount of light reflected will decrease until, when the two refractive indices become equal, there will be no reflection. This phenomenon may be observed if powdered glass be suspended in a mixture of carbon disulphide and benzol.

With a view to determining some of the underlying laws of opalescent solutions, we undertook to design a nephelometer better adapted both to theoretical and to practical work than those in use at present. By using equal columns of suspension and actually measuring the reflected lights with a suitable photometer, not only is one of the variables eliminated, but also we are enabled to determine the absolute ratio of the lights reflected by various suspensions. The photometric part of the apparatus consists of a wedge of neutral tinted glass by which the light from one of the suspensions may be controlled; and a suitable optical arrangement for observing the two beams of light. A Lummer-Brodhun prism would serve this purpose admirably, but by a simple arrangement of mirrors, a field far more sensitive than that of the Duboscq colorimeter may be obtained.

Briefly the design of the instrument is as follows: The suspensions to be compared are contained in the two cells *A* and *B* shown in the accompanying diagram (Figs. 1 and 2). These consist of cylindrical glass tubes about 4 cm. high and 1 cm. in diameter. A glass plate is sealed into one end, while the other end is covered by a circular plate of glass slightly countersunk and held in place by caps of black fiber. These prevent stray light reflected from the edges

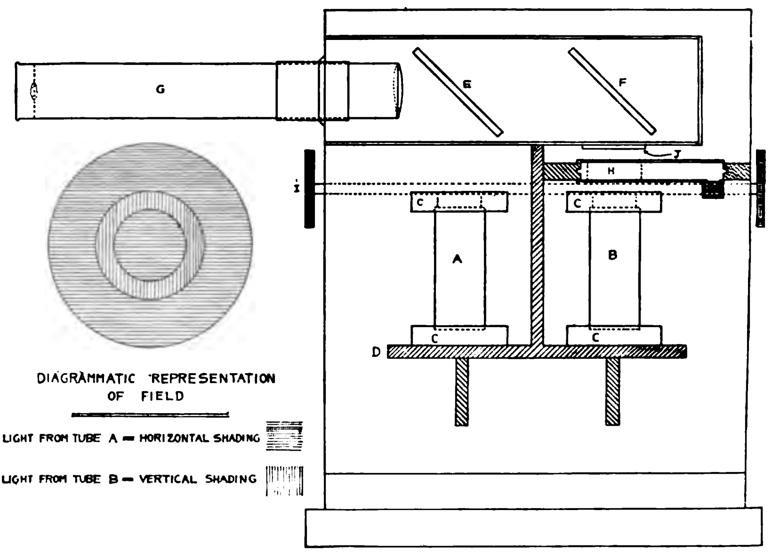

FIG. 1.

of the glass from entering the instrument. Difficulties arising from the agitation of the liquid by plungers are also thus avoided by having the cells completely enclosed. The cells rest on a shelf and are illuminated normal to their axes by a parallel beam of light from a 100 Watt lamp. The rays reflected from the suspended particles pass upward to the two mirrors *E* and *F* whence they are reflected into the magnifying eyepiece *G*. This is focused on mirror *E*. A circle cut through the silvering of mirror *E* permits the juxtaposition of the light from tubes *A* and *B* thus giving the eyepiece a field which is represented diagrammatically in the accompanying illustration. Photometric balance is effected by changing the intensity of the light from tube *B* by means of the sliding wedge of

neutral tinted glass *H*. This adjustment is made by the thumb-screw *I* and the position of the wedge is read on a scale mounted alongside (not shown in the diagram). A compensating wedge wedge may be placed at *J*, but unless the sliding wedge *H* is of fairly steep pitch, this is unnecessary, as the illumination of the field is sufficiently uniform without it. All parts of the instrument from which extraneous light may be reflected are painted a dead black.

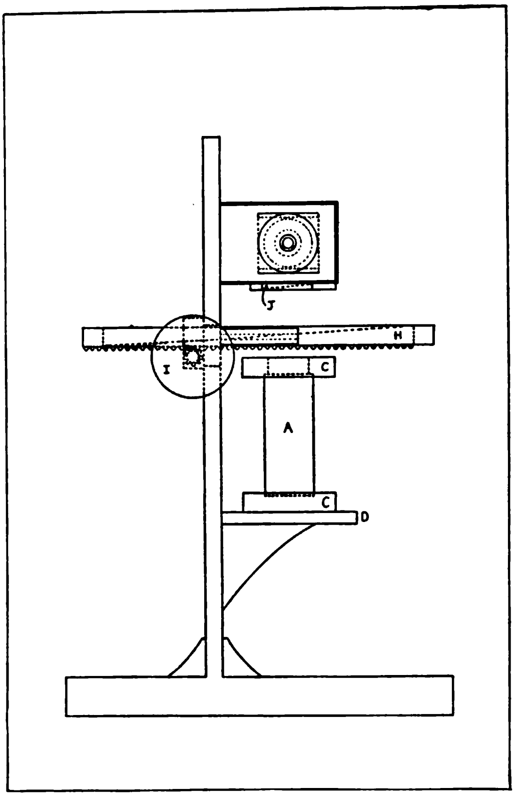

FIG. 2.

The construction of this instrument was delayed owing to difficulties encountered in securing neutral tinted glass. While awaiting its completion we decided to improvise a nephelometer in which several minor changes have been made. Among these may be mentioned the substitution for the glass wedge of a metal plate in which was cut a tapered slot. With this instrument we undertook some

work of rather an empirical nature along biochemical lines. Kober in one of his papers suggested the possibility of a nephelometric determination of albumin in urine, and a turbidimetric method for the same has been developed by Folin and Denis.[5] We therefore decided to apply our instrument to this problem. The standard was prepared from fresh normal human serum as recommended by Folin and Denis, and was standardized by nitrogen determinations and also by gravimetric determination of the heat coagulable proteins.

Difficulty was encountered at the start in comparing in the nephelometer albumin precipitated in the urine with that precipitated in the solution of standardized blood serum, on account of the difference in color due to the urinary pigments. In order to eliminate this interference of color, and also to obtain identical conditions of precipitation for both urine and standard, two equal portions of the urine of from 0.3 c.c. to 10 c.c. depending upon the quantity of albumen present, were taken. To one of these a known amount of standard was added (about 0.5 c.c. of 0.4 per cent. solution of serum protein). Both were then diluted to 75 c.c. with water and finally made up to 100 c.c. by the addition of a 7.5 per cent. solution of sulpho-salicylic acid. This gave a final concentration of 1.87 per cent. sulpho-salicylic acid, while the amount of protein varied from 2 to 5 mg. in 100 c.c. The resulting opalescent solutions were then compared in the nephelometer, the tube containing the urine plus standard being placed under the tapered slot. The light from this tube was then progressively diminished by adjustment of the slotted plate until photometric balance was obtained. From a scale with suitable vernier the position of the plate was read. As the theory has not advanced far enough as yet to permit of a purely formulative interpretation of the readings, the ratio of the concentrations of the two suspensions was determined from a curve. This curve had been obtained by plotting against the concentrations the scale readings obtained when known ratios of serum, made up with albumin free urine and precipitated with sulpho-salicylic acid under identical conditions, were compared. From the ratio R determined by means of the curve, the amount X of albumin originally present in the urine was found by the formula $R = \frac{X}{X+n}$ where n is the amount of

[5] Folin and Denis, *Jour. Biol. Chem.*, XVIII., 273, 1914.

serum albumin added. Quantities of urine and of standard were so taken that *R* would be in the neighborhood of one half. Urines containing large amounts of albumin (1 per cent. or over) were, after suitable dilution, compared directly with standard serum solution. In the case of such urines the high dilution necessary to obtain suitable nephelometric clouds eliminated the differences of color mentioned above. The results were compared with gravimetric determinations made according to Scherer's method. The clear filtrates from the coagulated protein were tested with sulpho-salicylic acid to make sure that none of the protein remained in solution. Duplicate gravimetric determinations gave good agreement. It was immediately evident that the nephelometric determinations were considerably higher than the gravimetric. Moreover, in the case of determinations on daily specimens of urine from one patient, the nephelometric results were consistently about 25 per cent. higher than the gravimetric, while in a similar series from another individual the ratio between nephelometric and gravimetric determinations was very variable, ranging from 1 to about 1.5. This at once suggested that the different proteins of the serum, while closely related chemically and equally precipitable by sulpho-salicylic acid, might give, in the nephelometer, clouds of different intensities. It is a significant fact that in the case of patient *R* where the ratio of nephelometric to gravimetric was variable, half saturation of the urine with ammonium sulphate gave a considerable precipitate of globulin.

In order to determine what differences might exist between the opalescences produced by equal amounts of the various serum proteins on precipitation with sulpho-salicylic acid under identical conditions, albumin, euglobulin, and pseudoglobulin were prepared from horse serum. Solutions of these when compared in the nephelometer gave surprisingly different results. The albumin gave about two and one half times as great an opalesence as the euglobulin and about three times as great as the pseudoglobulin. Compared with casein[6] suspensions, the following ratios, expressing the light reflect-

[6] As standard solutions of casein are easily prepared and also give very satisfactory clouds on precipitation with sulpho-salicylic acid, this substance forms a very convenient standard of reference in nephelometric work with various proteins.

ing power of equal amounts of these proteins, were found: Casein = 0.67 albumin; euglobulin = 0.63 casein; pseudoglobulin = 0.51 casein.

From the results experimentally obtained with various urines and from the differences in the clouds produced by equal amounts of the serum proteins, it may be seen that the nephelometric comparison of urine, in which these proteins may occur in varying amounts, with any definite standard such as serum cannot give a determination of the total protein. We hope by the use of specific precipitants to apply the nephelometric method to the separate determination of albumin and globulin in urine. This may be of value in diagnosis.

As the object of this paper has been to consider mainly the design of the instrument and the reasons for this design, the discussion of its application to the determination of albumin in urine has of necessity been hardly more than a suggestion of the work along that line. The results of the investigation of this particular problem with the experimental details, will be published shortly.

Summary.

1. The previous work in nephelometry has been briefly reviewed and the underlying principles of the nephelometric and turbidometric methods have been compared.

2. A new form of nephelometer has been described in which columns of suspension of equal lengths are used. The lights reflected are equalized and compared by means of a movable wedge of neutral tinted glass. Juxtaposition of the two emergent beams is secured by mirrors.

3. The variations found in preliminary experiments on the nephelometric determination of albumin in urine indicated that equal amounts of the various serum proteins might give different opalescences. Investigation showed that upon precipitation with 1.87 per cent. of sulphosalicylic acid, the same concentrations of serum albumin and serum globulins gave widely different clouds.

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THE RÔLE OF THE GLACIAL ANTICYCLONE IN THE AIR CIRCULATION OF THE GLOBE.

BY WILLIAM HERBERT HOBBS.

(*Read April 24. 1915.*)

CONTENTS.

The Fixed Anticyclones Above Existing Continental Glaciers.

The Anticyclones as Agents of Glacier Alimentation.—In two monographs published in 1910[1] and later in my "Characteristics of Existing Glaciers,"[2] a theory of fixed glacial anticyclones centered over the snow-ice masses of Greenland and Antarctica was put forward upon the basis of a comprehensive review of the results of polar exploration. This theory furnished an explanation for the nourishment of these inland-ice masses through adiabatic melting and vaporization of the ice particles of the cirri, as they are drawn down within the vortex of the anticyclone, and the precipitation of this moisture, generally as fine ice needles, when it comes into contact with the glacier surface and the cooled air layer immediately above it. The obvious application of this theory of alimentation to the even greater continental glaciers of the Pleistocene and earlier glacial cycles, was made in a separate contribution.[3] For these fixed anticyclones themselves, which are deserving of a special name, so much evidence has now accumulated that their existence can hardly be disputed, though differences of opinion will no doubt arise concerning their dominance over or dependence upon the usual migrating cyclonic and anticyclonic movements in the atmosphere.

The Northern and Southern Glacial Anticyclones Compared.—That a great fixed anticyclone exists within the south polar region

[1] "The Ice Masses on and About the Antarctic Continent," *Zeitsch. f. Gletscherk.*, Vol. 5, 1910, pp. 107–120; "Characteristics of the Inland-ice of the Arctic Regions," *Proc. Am. Philos. Soc.*, Vol. 49, 1910, pp. 96–109.

[2] Macmillan & Co., New York and London, 1911, Chaps. IX. and XVI. and afterword.

[3] W. H. Hobbs, "The Pleistocene Glaciation of North America Viewed in the Light of Our Knowledge of Existing Continental Glaciers," *Bull. Am. Geogr. Soc.*, Vol. 43, 1911, pp. 641–659. When this theory of alimentation was announced, I supposed it to be new to science. Professor Hans Crammer has since called my attention to a little-known paper by Fricker published as early as 1893, in which a similar idea was made as a suggestion and at a time when there was little known which could have been cited in its support. (Dr. Karl Fricker, "Die Entstehung und Verbreitung des antarktischen Treibeises," Ein Beitrag zur Geographie der Südpolargebiete. Leipzig, 1893, p. 96; also "Antarktis," Scholl und Grund, Berlin, 1898, pp. 187–188.)

seems to have been early recognized by a number of scientific men, due especially to the writings of the late Sir John Murray, Bernacchi and Buchan. By them it was, however, assumed that this condition was determined in some manner by the earth's southern geographic pole, and was not connected with the inland-ice. A like natural tendency to regard movements within the lower atmosphere as determined primarily by their positions relative to parallels of latitude, is more or less general. As an illustration, it is generally assumed upon the basis of few and scattered observations within all save the central European areas, that the ceiling of the troposphore in its descent from the equatorial regions reaches its minimum altitude above the geographic poles, though it is far more probable that in the northern hemisphere at least its minimum of altitude is to be found to the southward above the continental glacier of Greenland. In the southern hemisphere the Antarctic continental glacier is probably centered near the pole, and in consequence conclusions drawn from geographic positions are there relatively indecisive. During the winter season the great deserts of moderate latitudes become likewise the loci of anticyclones. Their influence upon the general circulation within the earth's atmosphere should be, however, relative to that of the inland-ice small by comparison. It is because the inland-ice masses have a domed surface that they permit the air which is cooled by contact to flow outward centrifugally and so develop at an ever accelerating rate a vortex of exceptional strength. As already pointed out in my earlier papers, this is one of the essential conditions for the formation of strong glacial anticyclones.

Their Strophic Action Believed to be Dependent upon an Automatically Recurring Disturbance of Balance Between Opposing Forces.

The Refrigerating Air Engine.—The strophic action of glacial anticyclones is one of their most marked characteristics, and would appear to be dependent upon the shield-like form of the glacier surface. Opposed to each other are here the abstraction of heat from the air above the glacier surface tending to make it slide off radially, and the increase of temperature due to resulting conden-

sation. Unlike the latter, which is determined by the measure of the vertical component of its fall, the contact cooling is in direct ratio to the time the layer of air rests upon the snow-ice surface. Conditions of calm therefore favor cooling and descent of air currents, as high wind velocity, does the warming and consequent retardation or even reversal of the descending current. It is not surprising, therefore, that the strophic glacial storms are initiated in calm conditions, "work themselves up" or become accelerated to accord with the acceleration of velocity of bodies sliding upon inclined surfaces (here further accelerated by increasing slope toward the margins), and bring about their own extinction when the air passes over the surface too rapidly for surface cooling to exceed or equal adiabatic warming. The sudden check in the outward flow of air, which is one of the most striking features of these strophic movements, in turn promotes new surface cooling and causes the precipitation of fresh snow within the zone of near contact to ice, thus often taking place with the sun but little obscured. In the automatic recurrence of similar movements the glacial anticyclone thus bears considerable resemblance to the hydraulic ram.

The Lines of Evidence for Fixed Glacial Anticyclones.

The Earlier Evidence.—The observational evidence which in earlier papers was adduced in support of the existence of the glacial anticyclone above continental glaciers, was drawn chiefly from the then available reports upon exploration of the inland-ice masses of Greenland, Antarctica, and Northeast Land (Spitzbergen). This evidence may be profitably summarized under the following heads:

1°. Centrifugal flow of surface air currents above inland-ice masses.

2°. Outward (centrifugal) sweeping of surface snow largely derived from the central areas, and its deposition and accumulation as a marginal fringe about the inland-ice.

3°. Snow in large part wind-driven above the sloping portions of the ice mass.

4°. Sudden warming of the air at the end of the blizzard—foehn effect in descending currents.

5°. Behavior of upper air currents and movements of the cirri.

6°. The evolution of the Antarctic blizzard and its termination.

7°. Areas of relative calm corresponding to the flat central bosses of the ice domes.

8°. Air highly charged with moisture within the flat central area of calms, and precipitation of snow or ice near the glacier surface.

Confirmation in Later Exploration.—In the three years which have elapsed since the appearance of my "Characteristics of Existing Glaciers," important new explorations have been carried out; the inland-ice of Antarctica has been twice penetrated to the southern geographic pole and new areas have been explored; several crossings of Greenland have been made along new routes; and full reports upon some earlier explorations have become available. It is proposed, therefore, to review the evidence and show how this has been enlarged by the recent observations; as well as to add evidence along hitherto undeveloped directions. Such a discussion of the evidence seems to be called for at the present time, since in a paper recently read before the Royal Meteorological Society, Brooks has presented this theory as his own, merely citing my book for references to glacial conditions.[4]

Evidence for More than One Anticyclonic Center Above Each of the Greater Areas of Inland-Ice.

Greenland.—The three transections of the Greenland continent which have now been made within the central and southern portions, have revealed the fact that there are at least two higher plains upon the snow-ice surface which are separated by a depression. This depression clearly lies to the northward of de Quervain's route, since his summit level is considerably lower than that of either Nansen or Koch and Wegener, though like Nansen's, his highest point is found near the east coast. The southern of the two nourishing centers of the Greenland ice-sheet is thus located toward the east coast and south of the Arctic circle, whereas the other center lies toward the west coast from the medial line of the continent,

[4] Charles B. Brooks, "The Meteorological Conditions of an Ice Sheet and their Bearing on the Desiccation of the Globe," *Quart. Jour. Roy. Meteorol. Soc.*, Vol. 40, 1914, pp. 53–70.

and in an as yet undetermined latitude, though certainly well to the northward of Disco Island (Fig. 1).

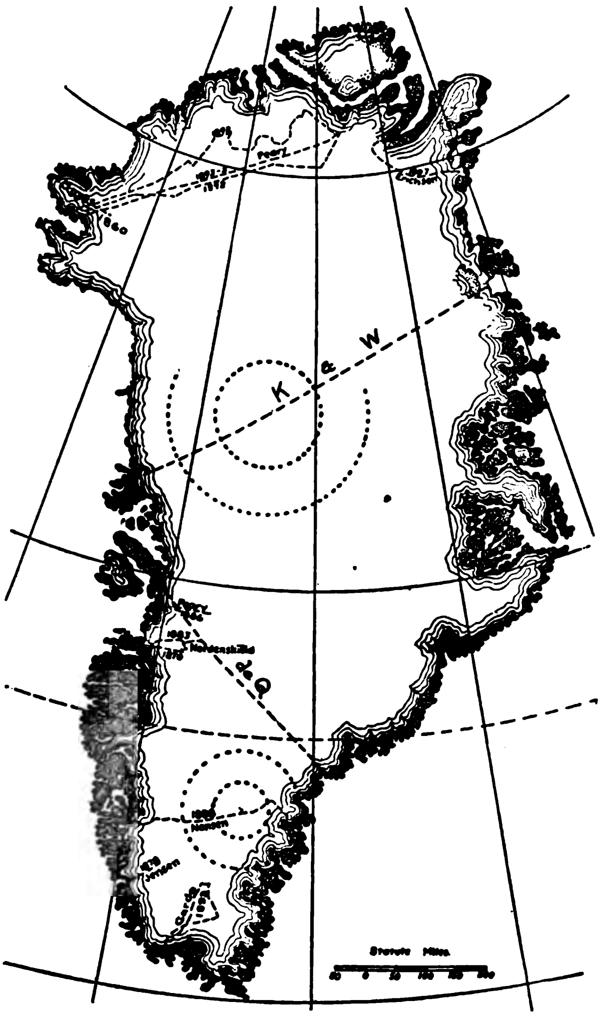

FIG. 1. Sketch map of Greenland to show roughly the position of the ice domes within the central and southern portions.

Antarctica.—This discovery that Greenland is provided with more than one nourishing center for its inland-ice, is wholly in accord with what has now been learned concerning the Pleistocene continental

glaciers of North America, which had the Keewatin, Labradorean and Patrician nourishing centers that repeatedly waxed and waned so as to reach their several maxima at different times (Fig. 2).

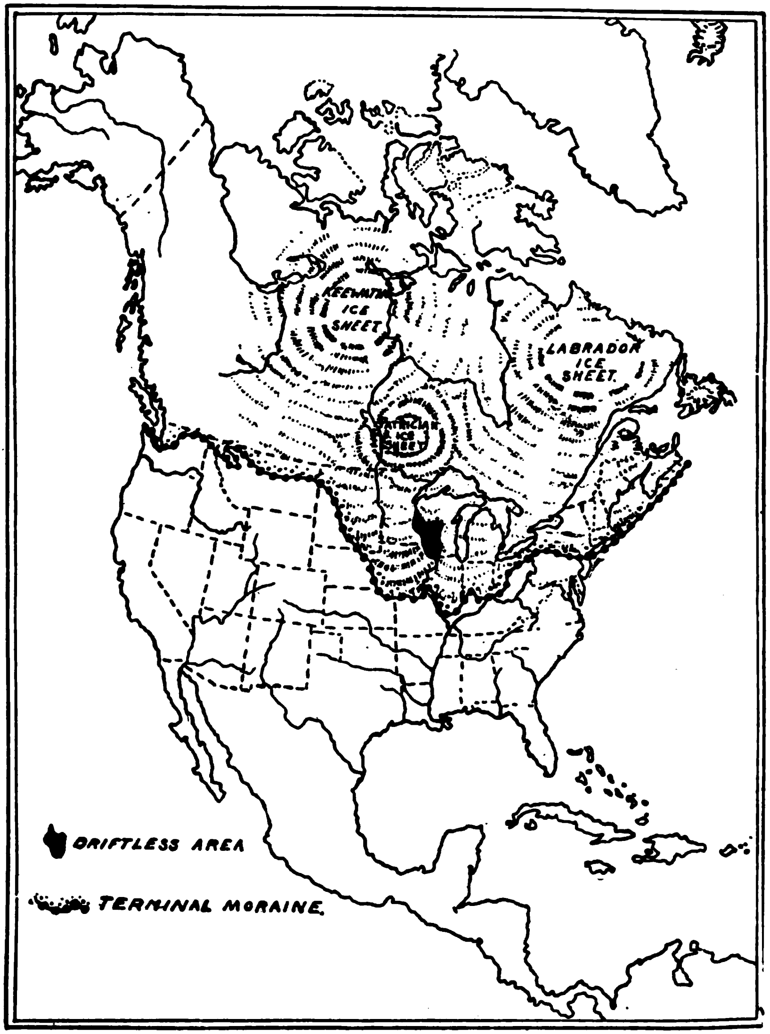

FIG. 2. Map showing the known anticyclonic centers of the Pleistocene continental glacier of North America.

From the Antarctic region the experiences of Mawson strongly indicate a near-by anticyclonic area probably located near the mag-

netic pole.[5] Within a vortex of this nature the wind velocity is determined by angular velocity multiplied into the radius, and hence one of relatively small dimensions should exceed in vigor one that is spread over a vast field and in which the steeper marginal area bears a smaller ratio to the whole. Mawson has expressed the belief that his base was near the center of a permanent anticyclone.[6]

The Centrifugal Flow of Surface Air Currents Above the Inland-Ice Masses.

Early Evidence from Greenland.—In 1911 when my work on glaciers was published, evidence was available upon this from both the eastern and western coasts of southern Greenland in latitude 64° (Nansen), from west Greenland in latitude 69° (Peary and later de Quervain and Stolberg[7]), from northwest Greenland in latitude 78–83° (Peary), and from northeast Greenland in latitude 77° to 82° (Trolle). With the exception of the first and last mentioned, these data applied exclusively to the western coast where the prevailing surface winds come from the easterly quadrants.

Later Confirmation.—The later evidence for the centrifugal flow of surface air is ample and throughout confirmatory. De Quervain, who crossed the inland-ice in 1912 between the latitudes of 66° and 68°, found head winds while ascending the west slope, but winds from behind during his descent to the east coast.[8] Referring to the low temperatures and the wide diurnal temperature range within the central area, de Quervain says:

> "It is the cold air of this middle part which even in summer streams like water from off the high surface toward all margins, deviated to the right in consequence of earth rotation" (p. 137).

Measurements of snow temperature made at different depths show

[5] Sir Douglas Mawson, "Australasian Antarctic Expedition 1911–1914," *Geogr. Jour.*, Vol. 44, 1914, pp. 257–286.

[6] L. c., p. 69.

[7] The first Swiss expedition, which penetrated some seventy miles from the coast (A. de Quervain und A. Stolberg, "Durch Grönlands Eiswüste," Strassburg, 1909).

[8] A. de Quervain, "Quer durchs Grönlandeis, Schweizerische Grönland-Expedition 1912–13." Reinhardt, München, 1914, 196 pp., 15 pls., 37 figs. and map. Also personal communications.

how exactly the air temperature follows that of the snow (p. 94). The diary of the journey (pp. 85–104) shows that for the first three weeks on the inland-ice the wind blew almost uninterruptedly down slope from in front, became more variable and shifting on the plain with slope a few seconds of arc, and reversed direction and blew from the northwest soon after passing the divide, where slopes became 8′ of arc to the eastward.

Koch and Wegener in their transection of the Greenland continent at its widest section (between latitudes 72° and 73°) encountered essentially the same conditions, the outward blowing currents constituting a veritable succession of storms whose vigor increased toward both margins of the section.[9]

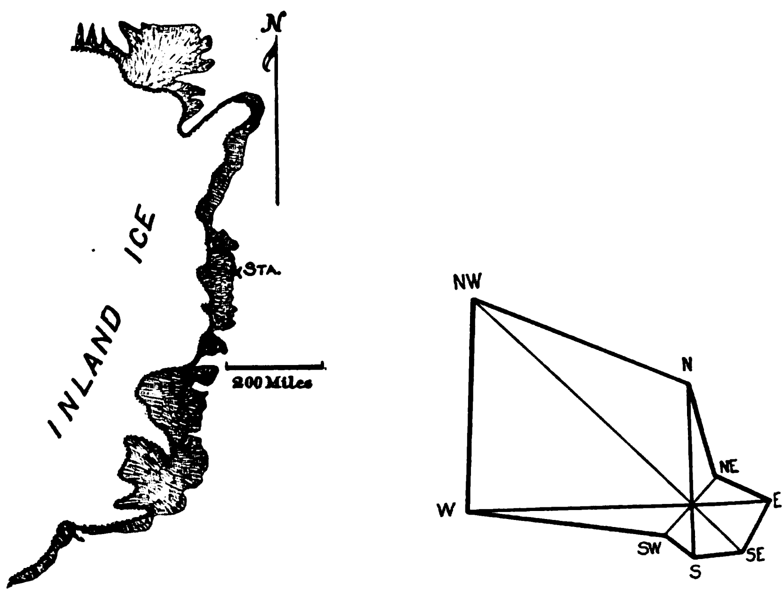

Fig. 3. Frequency wind-rose at Danmarks-Haven in northeast Greenland and (at the left) a sketch map showing location of the station with reference to inland-ice (after Wegener).

From northeast Greenland there was available at the time of my earlier discussions of the glacial anticyclones, only a preliminary

[9] J. P. Koch, "Unsere Durchquerung Grönlands 1912–1913," *Zeitsch. d. Gesellsch. f. Erdk. z. Berlin*, 1914; Alfred Wegener, "Vorläufiger Bericht über die wissenschaftlichen Ergebnisse der Expedition," ibid.

statement concerning the prevailing direction of surface winds at the Danish base near Cape Bismarck. More recently (1911) the full meteorological report by Wegener has been issued; and, confirming the earlier statement, shows that all strong winds come from the westerly (inland-ice) quadrants. The frequency wind-rose to cover the entire period of two years over which the observations extended, is reproduced in Fig. 3.[10] If the wind force had been taken account of, the easterly sections of the rose would have almost disappeared, since easterly winds are always light sea breezes, which at an elevation of only 1,000 meters have been completely overwhelmed by the northwest winds.[11] In this rose the dextrorotatory deviation of the down-slope winds is apparent.

Early Evidence from Antarctica.—Over the Antarctic inland-ice the law of surface air circulation had been clearly indicated by the results of exploration at the time of my early discussion of the subject. The more important data had been derived from the sledge journeys of Captain Scott, Sir Ernest Shackleton, Professor David and Dr. von Drygalski. As early as 1902 Captain Scott had ascended the Ferrar glacier outlet to the inland-ice above the mountain rampart and pushed west southwestward over it for a distance of two hundred miles, ascending on ever decreasing grades to the farthest point attained, and encountering winds of nearly constant direction coming from the south-southwest. The prevalence of such winds was demonstrated by a single set of sastrugi which pointed in the same direction (see Fig. 4).[12] Shackleton on his polar journey ascended the Beardmore outlet and for a like distance of two hundred miles over the inland-ice found strong winds blowing from the southerly quarter and sastrugi pointed in the same direction. David pushed northwestward from Ross Sea over the inland-ice to the south magnetic pole, crossing over a crest in the ice and descending on low grades during the last stage before reaching the pole. Here the same rule of distribution of currents applies,

[10] A. Wegener, "Med. om Grönland," Vol. 42, 1911, pp. 324–326.

[11] Wegener, "Med. om Grönland," Vol. 42, 1909, pp. 73–75.

[12] For this and other references to work published before 1910, see "Characteristics of Existing Glaciers," Chapters XIV.–XVI.

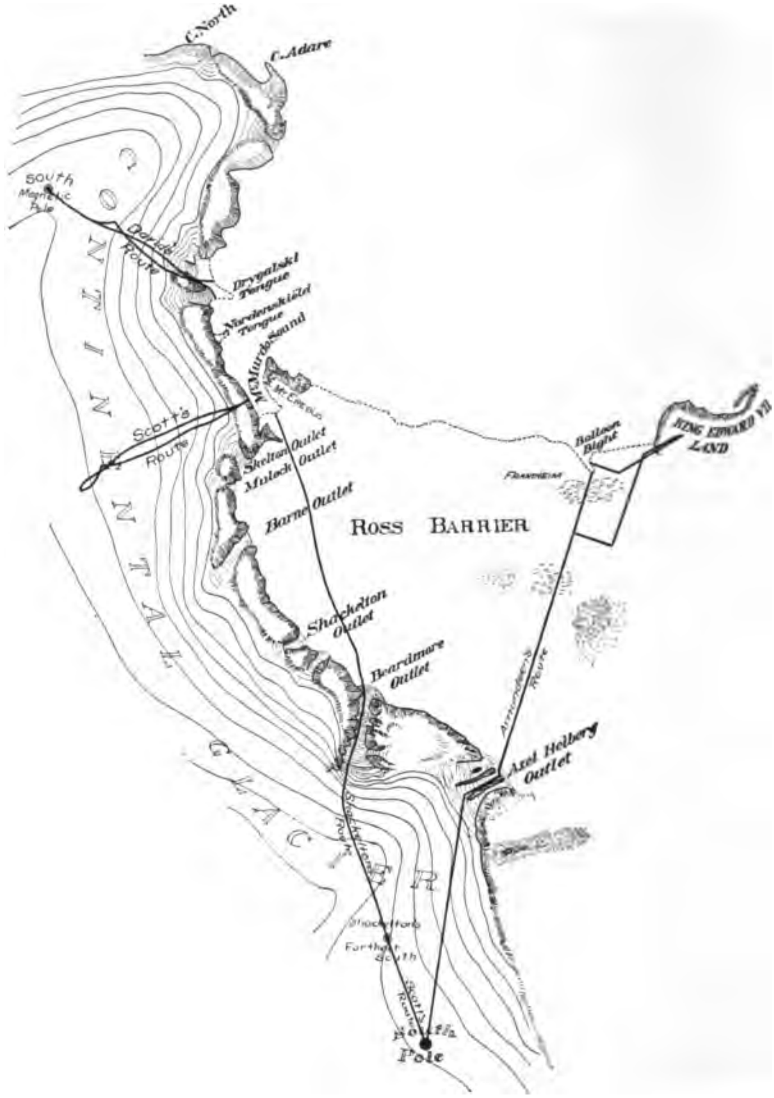

FIG. 4. Map of South Victoria land showing the sledge routes of Scott, Shackleton and David over the inland-ice.

for during the ascent he encountered northwest winds with sastrugi pointing toward the same quarter, but after passing the divide and on the down grade winds blew from behind—southeast. These observations were fully confirmed by the return journey

In Kaiser Wilhelm land also the report of von Drygalski shows that the prevailing winds blow downward off the inland-ice onto the sea and the shelf-ice in front, being deviated to the left—the prevailing strong winds are from the easterly quarter.

Later Confirmation.—Later data which bear upon the problem are derived from the Amundsen and the second Scott south polar expeditions, from the second German expedition to the Antarctic commanded by Filchner, and from the Australasian Antarctic expedition of 1911–14 under command of Dr., now Sir Douglas, Mawson. The route of Captain Amundsen passes through the mountain rampart which hems in the inland-ice, keeping a direction diagonal to it and for some distance after leaving the outlet behind taking a course near a high mountain range. The few data upon wind directions which he has jotted down in his narrative, appear to indicate local currents controlled by these mountains until he had reached the 88th parallel, where he entered an area of calms and light variable winds.[13] The second Scott expedition inasmuch as it followed the route of the earlier Shackleton expedition, has for the greater part of the distance, or until it entered the area of calms, served only to confirm the prevalence of outwardly flowing wind currents described by Shackleton.[14]

The recent Australasian expedition supplies evidence from a new quarter—the long coastal area near the Antarctic circle and to the westward of the Ross Sea, on which coast the inland-ice is not held in restraint by any barrier of mountains, as is the case in South Victoria Land. Along this coast, summer and winter alike, almost incessant storms blow off the ice onto the sea. These outwardly directed storm winds tend to keep the near sea area clear of pack-ice but offer great difficulties in the way of effecting a landing at all save those rare occasions when the force of the wind falls away.[15]

In Prince Regent Luitpold Land, where the later German expedition effected a landing upon the inland-ice—here likewise unconfined by a mountain wall and with partially detached shelf-ice in

[13] Roald Amundsen, "The South Pole," Vol. 2, 1913.

[14] "Scott's Last Expedition," Vol. 1, Chapters XVII–XIX.

[15] Sir Douglas Mawson, "Australasian Antarctic Expedition 1911–14," *Geogr. Jour.*, Vol. 44, 1914, pp. 257–286, maps and plates.

front—much the same conditions obtain, the wind blowing out to sea with velocities sometimes as high as 40 m.p.s.[16]

Outward Sweeping of the Surface Snow which Falls Over the Central Areas of the Ice Domes, and its Accumulation About Their Margins.

The Centrifugal Snow Broom.—What may be characterized as the centrifugal snow broom which sweeps out snow deposits from the central areas and collects them upon and about the margins of continental glaciers, is a necessary consequence of strong anticyclonic conditions; and its work is in evidence within all areas where inland-ice has been extensively explored.

From observations by Wegener, a wind velocity of 6–7 m.p.s. raises the snow lying upon the ground and sets it in motion along the surface at heights up to several decimeters (a foot or thereabouts). With wind velocities of 10–15 m.p.s (22.4–33.6 miles per hour) the migrating drift snow rises in a layer several meters in height and interferes seriously with seeing conditions. With velocities of 20 m.p.s. (44.7 miles per hour), the snow is carried to a height of 20 meters, or over sixty feet, and much higher in the lee of obstructions in its path.[17]

The Sweepings Below Outlets.—It is obvious that the results of snow drifting by centrifugal surface currents above inland-ice will be different according as the ice mass has been built up within a rampart of mountains (South Victoria Land and the greater part of Greenland), or as it has been allowed to shape itself independent of such retaining walls. In the former case the drift snow pours out along the courses of the outlet glaciers to form characteristic aprons at their bases,[18] or perhaps to produce definite fringing gla-

[16] "Deutsche Antarktische Expedition, Bericht über die Tätigkeit nach Verlassen von Südgeorgien," *Zeitsch. d. Gesellsch. f. Erdkunde z. Berlin*, 1913, p. 15; see also, *Kön. preusz. Meteorol. Institute*, Abh., Bd. 4, Heft II., p. 9.

[17] *Med. om Grönland*, Vol. 42, p. 345.

[18] In the light of observations by Scott, Shackleton and David in South Victoria Land, it seems probable that these apron-like snow deposits in the form of dry deltas are due largely if not wholly to this cause. Not only have explorers observed the rapid collection of the drift snow at the base of the Beardmore outlet, but this origin is probable for the reason that accord-

ciers such as have been described by Chamberlin[19] and Salisbury[20] from northwest Greenland, and by the Danes in northeast Greenland.[21]

Shackleton, who advanced over the inland-ice in his southern journey on a layer of granular surface snow, returned over a marble-like floor from which the snow had all been swept by the fierce blizzard encountered near his farthest south. On arriving at the Beardmore outlet, he found the lower forty miles of the stream buried deep under great drift accumulations. Scott on his last expedition was much less fortunate while on the plateau, and the burden of his diary is a prayer for strong wind to clear the surface. As is well known, he encountered heavy sweepings of powdery drift snow at the base of the Beardmore, both during his advance and on the return, and his floundering progress through this soft snow was a main contributing cause of the final disaster which overtook the expedition.

From what is known of the characters of freshly precipitated snow at different air temperatures, it is possible to rather definitely ascribe the enormous snow drifts which piled up for four consecutive days upon the Beardmore glacier apron as the *chasse neige* in process of melting as a result of adiabatic rise in temperature in descending currents. This snow, Captain Scott tells us, was the fine powdery type, though the temperature was phenomenally high ($+27° — 31°$ F.), stuck to hair and beard, and produced pools of water everywhere.[22] On the return the snow here was soft, loose and sandy, and sledge work was like "pulling over desert sand."[23]

Marginal Accretions of Snow.—Valuable new observations which bear strongly upon this point, have been supplied in the preliminary report upon the crossing of Greenland by Koch and

ance of *surface* level is generally observed to characterize the junctions of tributary with main glacier streams wherever snow drifting plays only a secondary rôle.

[19] *Jour. Geol.*, Vol. 3, 1895, p. 579.

[20] L. c., p. 886.

[21] Koch und Wegener, "Die glaciologischen Beobachtungen der Danmark-expedition," *Med. om Grönland*, Vol. 46, 1912, Chaps. VI.-VII., pls. and figs.

[22] "Scott's Last Expedition," Vol. 1, pp. 335-339.

[23] L. c., p. 396.

Wegener. They report almost continual storms in all save the highest section of their journey, the wind descending the slopes and filling the air with drift snow. Within the marginal portions of their section, it was established that the finely granular surface layer of snow is joined abruptly to a more coarsely crystalline subjacent layer and corresponds to the annual deposit. This layer was by a series of measurements shown to vary in thickness from 20 cm., or about eight inches, in the central portion, to one half meter (or about two and a half times that thickness) near the east coast, and a meter (or five times this thickness) near the west coast. Schematically represented with grossly exaggerated scales, this distribution is expressed in Fig. 5. It was further determined that the snow

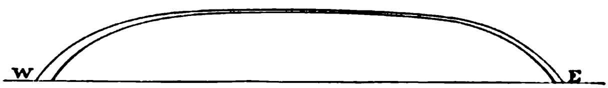

FIG. 5. Diagram to illustrate the marginal thickening of annual snow deposit upon the Greenland continental glacier due to drifting on radial lines.

deposit at Borg, the winter station upon the inland-ice though relatively near its margin, was less than on the coast to the eastward.[24]

Still more recently has appeared the preliminary report of Mawson upon the Australasian Antarctic expedition, in which he tells us that at the winter station on the margin of the inland-ice, the winds which blew down slope and off shore raised "a sea of drifting snow which poured fluid-thick over the landscape."

> "For months the drifting snow never ceased, and intervals of many days together passed when it was impossible to see one's hand held at arm's length. The drift snow became charged with electricity and in the darkness of the winter night all pointed objects and often one's clothes, nose, and finger tips glowed with the pale blue light of St. Elmo's fire. . . . Such weather lasted almost nine months of the year. Even in the height of summer, blizzard followed blizzard in rapid succession."[25]

Where tongues of ice extended out to sea from the shore, snow collected upon them though the marginal slopes were swept free of it by the force of the blizzard.[26]

[24] A. Wegener, "Vorläufiger Bericht über die wissenschaftlichen Ergebnisse der Expedition," *Zeitsch. d. Gesell. f. Erdkunde z. Berlin,* 1914.

[25] Sir Douglas Mawson, "Australasian Antarctic Expedition, 1911–14," *Geogr. Jour.*, Vol. 44, 1914, pp. 269.

[26] Mawson, "The Home of the Blizzard," 1915, Vol. 2, p. 33.

SUDDEN WARMING OF THE AIR AT THE END OF THE GLACIAL BLIZZARD—FOEHN EFFECT IN DESCENDING CURRENTS.

Intensive Foehn Effective in Outlets.—This familiar foehn effect is so general a phenomenon about the margins of both the great continental glaciers that it has long been recognized.[27] The general rule holds that the temperature of the air rises as the blizzard is evolved.[28] Wherever a mountain rampart exists, the elevation of temperature becomes accentuated within the glacier outlets, and melting in Antarctica is almost unknown except under these conditions. An interesting example of this which has not before been emphasized, is supplied by Armitage, who on the first ascent of the Farrar outlet found a stream of water seven feet in width and nine inches deep flowing beside the ice.[29] The effect of similar currents of water was noted by David on his ascent to the plateau from McMurdo Sound. A remarkable instance, also, with long continuance of high temperature, is that above cited from Captain Scott's journal, while camped on the apron below the Beardmore outlet.

The Greenland Foehn.—The characteristic Greenland foehn has been subjected to a special study by Stade, the meteorologist of the Berlin Geographical Society's expedition to Greenland.[30] He finds that the temperature changes are much more pronounced during the winter season, the rise on March 5, 1893, having been 12° C. and probably much more within the space of a few minutes. Stade's conclusion is that these foehn winds are connected with low areas moving northward in the Davis Straits, the maximum of air temperature and the minimum of relative humidity corresponding either exactly or approximately with the minimum of pressure at the station. De Quervain's later studies would indicate that Stade's moving depressions may better be regarded as pulsations within a stationary low pressure area lying over Davis Straits and Baffin's

[27] See "Characteristics of Existing Glaciers," pp. 149–150, 268–271.

[28] Cf. Mawson, "The Home of the Blizzard."

[29] A. A. Armitage, "Two Years in the Antarctic," London, 1905, p.

[30] Dr. H. Stade, "Über Foehnerscheinungen an der Westküste Nordgrönlands und die Veränderung der Lufttemperatur und Feuchtigkeit mit der Höhe, Nach den Beobachtungen auf der Station Karajak, Grönland Expedition 1891–93," Vol. 2, 1897, pp. 501–533.

Bay. It would then seem more in harmony with the facts to reverse this conception and assume that the low pressure area is stimulated to greater vigor by the arrival of the strong winds of the glacial blizzard over the inland-ice.

Foehn Level and Foehn Clouds on Greenland Coast.—In northeast Greenland the monumental investigations by Wegener furnish us with clearly defined results. In addition to full station weather observations collected for a period of two years at two neighboring stations—Pustervig, relatively near the inland-ice margin but within a canyon, and Danmarks-Haven, fifty miles further outward and upon the coast;[81] we have systematic observations with kites and captive balloons in ascents to heights generally of 1,500 meters and occasionally of 3,000 meters.[82] The results indicate that the larger weather disturbances are in the main controlled by the great high pressure area lying over the continent, that two strongly marked lower inversions in the atmosphere occur almost uniformly; the first within the lower 200 meters and explainable by surface radiation and latent heat of freezing and thawing, while the second lies between a thousand and fifteen hundred meters of altitude, at which level the great outward streaming from the inland-ice pours over the rock plateau to the westward of the station (average height of the plateau 800 meters). The most prevalent cloud form at the stations consists of a series of flat mushroom shapes in a succession of steps or stages located near the upper inversion level—on an average, 1,200 meters. These being clearly due to foehn conditions, they have by Wegener been given the name, "foehn clouds."

The twenty-three ascents of kites and balloons which were carried out at the time of more pronounced foehn, indicate that owing to the partial disappearance at such times of the lower cold moist layer, the temperature inversion of this lower layer is less pronounced and the temperature fall in the layers above it more pronounced, than at other times—in the most marked instances this fall

[81] A. Wegener, "Meteorologische Terminbeobachtungen am Danmarks-Haven, *Med. om Grönland,* Vol. 42, 1911, pp. 124–355. W. Brand und A. Wegener, "Meteorologische Beobachtungen der Station Pustervig," ibid., 1912, pp. 446–562.

[82] A. Wegener, "Drachen- und Fesselballonaufstiege aus gefuhrt auf der Danmark-Expedition 1906–08," ibid., 1909, pp. 1–75.

is super-adiabatic. The typical foehn cloud layer at 1,200 meters is also at such times much more marked, and up to this level the wind velocity falls off with altitude. Of the greatest significance were the results of ascents made at the time of easterly winds—always light; since these show that the easterly winds fade away below the altitude of 1,000 meters, at which level they become replaced by the westerly winds which are controlled by the anticyclones.[33]

Areas of Relative Calm and of Air Highly Charged with Moisture Corresponding to the Central Plains Upon the Ice Domes.

Few Early Data.—At the time "Existing Glaciers" was published, no observational evidence bearing upon this point was available from either of the large continental glaciers, since neither had been penetrated to the central area. Nansen's crossing of Greenland within its narrowed southern portion, had revealed an area of calm near the divide on his section, but it could not then be predicated that this represented more than the margin of the central ice plain. The most valuable evidence then available was derived from Northeast Land (Spitzbergen), which is covered by a dome of inland-ice about a hundred and eightly miles in diameter and between two thousand three thousand feet in altitude in the central area. This area of inland-ice had in 1873 been penetrated by A. E. Nordenskiöld and Palander, who several times observed the simultaneous fall of irregular ice-grains enveloped in water and of small snow-flakes either rounded or star-like, the ice-grains freezing immediately on falling and becoming attached to the hair or clothes, since the air temperature was —4° to —5°.[34]

Recently Acquired Evidence from Antarctica.—During his penetration of the inland-ice area of Antarctica, Captain Amundsen entered near the 88th parallel, what he believed to be a region of permanent calm or of light winds and of generally clear weather. As evidence of this, the snow surface was smooth and with no in-

[33] A. Wegener, "Drachen- und Fesselballonaufstiege," *Med. om Grönl.*, Vol. 42, 1909, pp. 60–75.

[34] Cf. "Existing Glaciers," p. 277.

dication of drifting. To a depth of 2 meters no hard snow layers were encountered, so that the cutting of blocks (for guide cairns) was all but impossible. During the fortnight spent within this region the sky was clear with light winds, except on two days when there were snow flurries at intervals. The brightening after the snow was accompanied by such a high sun heat that even with most clothing removed the perspiration poured from the bodies of the men.[35]

Captain Scott, who entered the same general region about a month later, found conditions of atmosphere and snow which during the three weeks of his stay within it, agreed strikingly with those described by Amundsen. After passing the latitude 87½°, hardly a day passed that he did not jot down in his diary the fact of variable light winds and the noteworthy softness of the snow surface, several times expressing his opinion that the area is one of light winds. He was evidently puzzled by the appearance of the clouds, "which don't seem to come from anywhere, form and disperse without reason." Again he describes them as "coming and going overhead all day, drifting from the S. E., but constantly altering shape. Snow crystals falling all the time" (Vol. 1, p. 370). On January 19 on the return from the pole, he notes, "Snow clouds, looking very dense and spoiling the light, pass overhead from S., dropping very minute crystals; between showers the sun shows and the wind goes to the S. W."

Again and again he calls attention to the dampness and the chill in the air, so that when the temperature is observed, all are surprised that it is not lower. The sun was often shining through the snow mist, and bright sunlight and overcast sky interchanged with kaleidoscopic suddenness. Near the margins of this area snow blizzards were experienced, but in comparison with the Barrier blizzards Scott notes that the wind was surprisingly light. Temperatures rise after the blows. Within this central area the sastrugi are found in isolated areas, show cross directions and general lack of constancy. The snow got softer the farther they went to the southward, and it was soft below the surface also "as deep as you like to dig down." Yet with all the wind variations, there was evidently a preponderance of southerly and southeasterly winds. Like

[35] Roald Amundsen, "The South Pole," Vol. 2, Chapters XI.–XIII.

Amundsen, Scott noticed a slight descent toward the pole from latitude 89½°, which, taken in connection with Shackleton's observations, would indicate that a crest of the inland-ice lies to the westward of the routes.[36]

Recent Data from Greenland.—The account of de Quervain's transection of Greenland in 1912 in latitudes 66° to 70° N., affords strikingly similar pictures. Whereas for the first three weeks of the journey upon the inland-ice, or until the ascent had been made to the interior plain, the outward blowing winds had been so constant as to be depended upon in laying the course; shifting winds of light force were encountered upon the plateau, and when the grade had been reduced to 3″ of arc even west or northwest winds blew for short intervals. The air appeared to be strongly saturated with moisture, and at times only the heads of the party would be visible at moderate distances because of the bank of mist, and beards, chins, caps, etc., became frozen into solid masses of ice. Once over the divide, where the slope took on a descent of 8′ of arc, the wind blew strongly from the northwest.[37]

The expedition of Koch and Wegener which crossed Greenland in its widest section (in latitudes 71° to 79°), perhaps furnishes us with the most satisfactory evidence that has yet become available upon meteorological conditions above the central boss of a continental glacier; for the reason that no other expedition has penetrated so close to the heart of the area. From the preliminary report we learn that above the flat dome of the ice shield, an area of atmospheric calm was encountered and much mist, which in the morning was generally so dense as to hide the sun. The air was so supersaturated with moisture that the clothing was constantly wet and could be dried only occasionally and with much difficulty. Everywhere above the altitude of 2,000 meters the snow surface was granular and underlain by coarser grained material, though without hard separating crusts.[38]

Despite the supersaturation of the air and the frequent deposition of minute ice crystals from the clouds, it is pretty clear that if

[36] "Scott's Last Expedition," Vol. 1, pp. 363–383.

[37] A. de Quervain, "Quer durchs Grönlandeis," 1914, pp. 85–137.

[38] Alfred Wegener, "Vorläufiger Bericht über die wissenschaftlichen Ergebnisse der Expedition," *Zeitsch. d. Gesell. f. Erdk. z. Berlin,* 1914.

referred to the plateau surface, the peculiar shifting clouds so often observed by Scott and Amundsen are at a low level. The diurnal temperature chart published by de Quervain for his transection of Greenland, shows that radiation from the surface is apparently but little interfered with by clouds after the central plain has been reached. The abrupt change from this condition to one of small daily range of temperature, is found on both margins of the summit plain.

The Cirri Above and About the Existing Continental Glaciers.

The Earlier Data.—The relative abundance of cirrus and cirro-stratus clouds, not only above but about the margins of the continental glaciers, will be patent to any one who will read the lists of cloud observations which are published in the reports of the exploring expeditions.[39] In 1911 it was possible to cite the observation of Nansen, that during his crossing of the inland-ice though the sky was in the main clear, those clouds which were present were generally the cirri or some combination of these with cumuli or strati. From the Shackleton expedition in the Antarctic it was learned that the upper air currents near the winter station generally appeared to move in from the northwest quadrant and veer southerly as they advanced toward the pole. The "polar bands" or "Noah's Arc" clouds (cirro-strati) in general moved southerly, but to the west of the Ross Sea, the "polar bands" moved in from the north northeast or northeast veering round from the north. Thus, as a general rule, it would appear that in this region the upper currents carrying the cirri move roughly parallel to but in opposite direction from the stronger surface currents. In the same region additional evidence was derived from the behavior of the

[39] See, for example: "Wilkes Exploring Expedition (when off the Antarctic Continent)," Vol. XI., *Meteorology,* pp. 276–291; Mohn und Nansen, "Durchquerung von Grönland," *Pet. Mit.,* Ergänzungsh. 105, pp. 22–29; Duc d'Orleans, "Croisières océanographiques dans la mer du Gronland en 1905, Résultats Scientifiques," Bruxelles, 1907, pp. 52–67; Stade, "Grönland Expedition der Gesellschaft für Erdkunde," Vol. 2, pp. 417–441; Wegener, "Meteorologische Terminbeobachtungen," etc., *Med. om Grönland,* Vol. 42, 1911, pp. 202–311.

vapor cloud above Mt. Erebus, which starts from an elevation of nearly 14,000 feet.

Later Investigations.—In endeavoring to investigate further the movement of the cirri upon the borders of the inland-ice, the data supplied by the Greenland Expedition of the Berlin Geographical Society have been taken into consideration. Stade in his tabulated meteorological data at Station Karajak on the west coast, in some thirty-nine instances has supplied the direction of movement of the cirri observed. These I have plotted to form a wind-rose (Fig. 6),[40] which shows clearly the dominance of movements from the

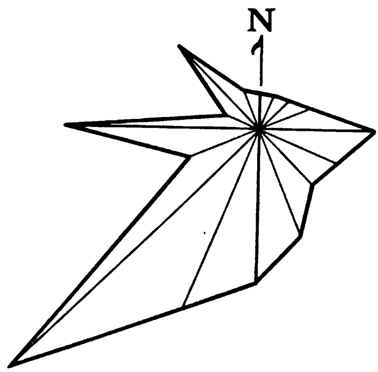

FIG. 6. Wind-rose for the cirri whose direction of motion was observed at station Karajac, West Greenland (several identified doubtfully as cirri are included).

southwest towards the northeast, or in other words in the general direction toward the interior region of the Greenland glacier.[41]

THE EVOLUTION OF THE GLACIAL BLIZZARD AND ITS ABRUPT TERMINATION IN FOEHN.

The Sequence of Events.—While there is apparently much in common between the Greenland and the Antarctic glacial blizzards,

[40] H. Stade, l. c., pp. 417–441.

[41] In central Europe Hesselberg has discovered a general correspondence between the drift of the cirri and that of the low pressure areas, but in view of the observations of de Quervain upon the stationary character of the depression over Baffin's Bay, it is unlikely that this conclusion can be applied to the borders of the inland-ice (Th. Hesselberg, "Ueber die Luftbewegung im Zirrusniveau und die Fortpflanzung der barometrischen Minima," *Beitr. z. Physik. d. fr. Atmosphäre,* Vol. 5, 1913, pp. 198–205.

we are indebted especially to Professor David, the meteorologist of the Shackleton expedition, for a careful study of the Antarctic type of blizzard as observed by him at the winter station of the expedition. I shall here cite my earlier summary of the sequence of events with some personal interpretations.[42]

"The sequence of events during a blizzard begins with gentle northerly winds which continue for a day or two during which temperatures are low. David has suggested that during this time air is flowing south to take the place of air whose volume has been reduced as a result of the heat abstracted from it on the ice surface. Then there follow two or three days of absolute calm, during which the temperature continues to fall. Still further cooled upon the ice surface, the air, a week or more after the calm begins, starts to move outward in all directions and so develops (on the edge of the barrier) a southeasterly blizzard. Simultaneously with this movement the steam cap over the volcano of Erebus, which normally indicates an upper current from the northwest, swings round to the north and takes on an accelerated movement, as though it were being drawn from that direction to supply air to the void resulting from the violent surface current toward that direction. Corresponding to the increased velocity, the normal foehn effect near the pole must be much increased as it is also on the descent of the surface current from the plateau. As soon as the warming of the polar air from this cause has become general, the high air pressure of the central area is automatically reduced, and thus the blizzard gradually brings about its own extinction. To the warming effect of the descending air current there is rather suddenly added the latent heat of condensation of the moisture when it is precipitated in the form of fine ice crystals within the air layers just above the snow-ice surface. The rather sudden termination of the blizzard may be thus in part explained. David has suggested that a 'hydraulic ram effect' may be induced in the air of the upper currents, since the steam clouds over Erebus, normally the antitrades, are temporarily reversed in direction at the termination of a blizzard, and for a short interval blow northward."

Source of the Precipitated Snow.—The actual initiation of the strong wind may begin very suddenly, as has been especially emphasized by Simpson[43] and even more strikingly brought out by Mawson.[44] Referring to the source of the moisture of the blizzard as the cirri, I stated in 1911:

"There is, however, the probability that in general this snow or ice is adiabatically melted and vaporized during its descent to the plateau, and subsequently congealed as it mixes with the cold air above the plateau

[42] "Characteristics of Existing Glaciers," pp. 269–270.

[43] "Scott's Last Expedition," Vol. 2, p. 325.

[44] Mawson, "The Home of the Blizzard," Vol. 1, Chap. VII.

surface. This would explain the clear skies which are so general over both Greenland and Antarctica during snows in the higher levels. It is of course true that the latent heat of fusion and vaporization of ice, abstracted as it is from the air during its descent within the eye of the anticyclone, will counteract to some extent the warming adiabatic effect; and it is not improbable that the long duration of Antarctic blizzards and their somewhat sudden terminations accompanied by snowfall are explained in part by the transformations of latent and sensible heat.

"Additional evidence for the continental and glacial rather than the polar nature of the Antarctic anticyclone is derived from the strong blizzards observed at the British winter quarters on McMurdo Sound. *Whereas the lighter gales came from the southeast and indicated a control by local conditions, a blizzard of the first magnitude was not thus influenced, and always swept down from the southwest—that is, from the high plateau, and not from the pole, since otherwise the earth's rotation would have given it an easterly direction.* When its powers begin to wane, it is once more controlled by local conditions and the wind again comes from the southeasterly quarter."

Amundsen's Meteorological Records at Framheim.—Hardly less significant were the directions of prevailing winds observed at Framheim, the winter quarters of the Norwegian Antarctic expedition of 1910–12, when the position of the station is considered in reference to areas of inland-ice and shelf-ice. The great dome of inland-ice of King Edward Land lies to the eastward and southeastward distant only about 115 miles, whereas that of South Victoria Land and its extension to the southeastward, lies a number of times that distance away to the southwestward and westward. Now it was found that easterly winds predominated (31.9 per cent. of the time), with southwesterly and southerly winds next in order (14.3 per cent. and 12.3 per cent. respectively). Southeasterly winds were especially rare, and as calms reigned for a fifth of the time (21.3 per cent.), the winds for four fifths of the period are those accounted for. Earth rotation should deviate original southwesterly winds into a southerly direction, and southeasterly to easterly.[44a]

Alternations of Calm and Gale.—The strophic characteristic of the glacial blizzard thus involves frequent alternation of calms with strong gales, and all systematic observations about the inland-ice reveal this characteristic. As already pointed out, the strophic quality is to be expected from the recurring disturbance of balance and later recovery in opposing forces (ante, p. 188). Below in tabu-

[44a] R. Amundsen, "The South Pole," Vol. 2, pp. 381–382.

lar form are set forth the percentage of calm days to all others as determined at several stations near the margin of inland-ice:

PERCENTAGE OF CALM DAYS TO ALL OTHERS.

	Per Cent.
Danmarks-Haven, Northeast Greenland[45]	26
Cape Adair, South Victoria Land[46]	45
Scott's First Base, South Victoria Land[47]	23
Cape Evans, South Victoria Land[48] (up to 4 miles per hr. 29.8 per cent.)	
Framheim, Whale's Bay[49] (up to 4 miles per hr. 42.2 per cent.)[48]	21.3

THE THEORY OF CIRCUM-POLAR WHIRLS VS. THE GLACIAL ANTICYCLONES.

Views of Ferrel and Hann.—From a theoretical view-point, the theory of circumpolar whirls first enunciated by the American meteorologist Ferrel, has been a most serious obstacle in the way of securing a clear conception concerning the air circulation above continental glaciers. Ferrel's theory assumed that strong westerly winds sweep about the geographic poles with increasing acceleration of velocity and corresponding centrifugal effect, producing polar areas of calm and of low barometer. Of the southern polar region, Hann stated as late as 1897:[50]

"The whole Antarctic circum-polar area presents us, as already stated, with a vast cyclone, of which the center is at the pole, while the westerly winds circulate round it."

This view was of course largely speculative, and when Bernacchi of the "Southern Cross" expedition had brought out on the basis of observations at Cape Adare the evidence for anticyclonic conditions over the south polar regions, Hann cautiously qualified his earlier statements in the following manner:

[45] Wegener, "Med. om Grönl.," Vol. 42, pp. 325–326.

[46] Bernacchi, in Borchgrevinck, "First on the Antarctic Continent," p. 306.

[47] Shaw, "National Antarctic Expedition, 1901–1904, Meteorol.," Pt. I., 1908.

[48] Simpson, "Scott's Last Expedition," Vol. 2, p. 320.

[49] Amundsen, "The South Pole," Vol. 2, pp. 381–382.

[50] "Handbuch der Klimatologie," 2te aufl., Vol. 3, 1897, p. 543.

"As regards the Antarctic Anticyclone, I have certainly not expressed myself quite clearly in my 'Klimatologie,' as you very fairly point out.

"It is certain that an area of pressure, which is higher than that of the surrounding area, lying over a chilled continent, or over any considerable land area, can coexist with a great polar cyclone, for instance, round the South Pole. The very low temperature can produce in the lower strata of the atmosphere a pressure higher than its environments. The anticyclone, however, must be very shallow, and at a moderate elevation the ordinary circulation of the atmosphere must reëstablish itself. . . . It is just possible that further inland a slight increase of pressure might be observable. There is certainly no chance of the existence of a real continental anticyclone, inasmuch as at Cape Adare the barometer falls from summer to winter."[51]

The above and later qualified statements by Hann[52] fail to take proper recognition of the facts as known at the time, and in treatises on meteorology published within the last five years, the circum-polar whirls are still treated with slight qualifications of statement, and as though in harmony with observed facts.[53]

View of Meinardus.—Probably the fullest discussion of this subject is that of Meinardus in 1909, who is so firmly convinced that the anticyclonic conditions that were encoutered in Kaiser Wilhelm Land at the margin of the inland-ice, cannot have an upward extension beyond 2,000–3,000 meters, that he even prophesied for the interior portions of Antarctica a bare land area destitute of snow.[54] He says:

"The elevated parts of Antarctica above 2,000–3,000 meters extend into the great cyclone of the polar whirl and encounter westerly air currents during the entire year. With this verification, which also further can be supported by certain observations from the marginal region, there follows the conclusion *that the Antarctic anticyclone can in general be present as active element in the air circulation only in the lower parts of the South Polar region.* . . . At the sea level and on the borders of the inland-ice, that

[51] Letter written to Captain R. F. Scott in 1900, *The Antarctic Manual,* 1901, p. 34.

[52] "Lehrbuch der Meteorologie," 2te aufl., 1906, p. 345; Klimatologie, Vol. 1, 1908, p. 334.

[53] Moore, "Descriptive Meteorology," 1910, p. 141. Milham, "Meteorology," 1912, p. 162.

[54] W. Meinardus, "Meteorologische Ergebnisse der Winterstation der 'Gauss,' 1902–03, Deutsche Südpolar Expedition 1901–03," Vol. 3 (*Meteorol.,* I., Vol. 1), p. 332. (The italics are in the original, W. H. H.)

is, within the known coast areas, the anticyclonic conditions do not yet prevail."[55]

Referring to the observations by Captain Scott and by others upon the plateau back of the Admiralty Range in South Victoria Land, Meinardus is quick to seize upon the westerly winds which there prevail as evidence that the anticyclone has at these levels given place to the supposed overlying cyclones; failing utterly to note that the winds are here blowing directly down slope from the ice plateau—that is, radially.[56] Other statements in the report are likewise strikingly at variance with facts either known at the time or revealed by later exploration.

Objective Studies by Barkow in Antarctica.—The first opportunity to measure the upward extension of anticyclonic conditions over Antarctica, has been taken advantage of by Barkow, the meteorologist of the Second German Antarctic Expedition; who at the margin of the inland-ice of Prince Regent Luitpold Land (lat. 77° 45′ S., long. 34° 40′ W.) sent up pilot balloons, one on February 2, 1912, to the extreme elevation of 17,200 meters, or over 8 km. above the base of the stratosphere.[57] These observations disclose the fact that easterly and northeasterly winds prevailed at the time of observation in all levels *up to the ceiling of the troposphere*,[58] whereas with the beginning of the stratosphere, where at an elevation of 9,000 meters the wind turns suddenly through an angle of 180° and blows steadily from the southwest. If, as is probable, the margin of the continent corresponds to the margin of the inland-ice dome, these observations considered with due regard to the known deviation indicate an anticyclone fed by currents above the troposphere. Barkow calls attention to the speculations of Meinardus above referred to, and shows that they are controverted by the results of his observations.

[55] L. c., p. 333. Hardly in harmony with the facts known at the time, since easterly winds, and not westerly, are here the rule (cf. "Existing Glaciers," pp. 264–265, and ante, p. 197).

[56] L. c., p. 334.

[57] E. Barkow, "Vorläufiger Bericht über die meteorologischen Beobachtungen der deutschen antarktischen Expedition, 1911–12," *Ver. d. k. preusz. meteor. Inst.*, No. 265 (Abh., Vol. 4, No. 11), Berlin, 1913, pp. 7–11.

[58] The italics are mine.—W. H. H.

Barkow also carried out kite and balloon ascents, of which a proportionately slight per cent. only failed to show strong inversions of the lower atmosphere, these inversions being proportionately both strong and frequent during the winter season. The entire lower layer of 2,000 meters height was shown to have an average higher temperature than the lowermost layer, the temperature rise from the bottom being often as much as 10° C., and in one instance 19.5° C. In the spring season an alternation of inversions (Blätterstruktur) was observed.

De Quervain's Studies in Southwest Greenland.—No less decisive in showing the absence of polar whirls are conclusions to be drawn from observations on the borders of the inland-ice of Greenland. At a number of stations on the west and southwest coasts ranging between latitudes 64° and 69°, de Quervain and Stolberg in 1909 conducted ascents of pilot balloons during the spring and early summer, carrying their observations to extreme heights often in excess of 10,000 meters (6⅔ miles),[59] and in one instance of 16,000 meters. In 1912 Drs. Jost and Stolberg supplemented these observations by a second series carried out through the winter season, with results concerning which only a preliminary statement is as yet available.[60]

As has already been explained, the prevailing surface currents at these stations are controlled by the Greenland anticyclones and blow from the southeasterly quadrant, though with considerable modification by local conditions below the level of 1,000 meters. On the basis of his balloon observations, de Quervain has declared that "at least in greater elevations a polar whirl which is in any degree unified and connects the different low pressure regions of the circumpolar latitudes, can, for the time of our observations in Greenland and Iceland, not be thought of." This conclusion was later extended to the remaining portion of the year, as clearly stated in the preliminary announcement of the results of the later series of observations.

[59] A. de Quervain, "Gleichzeitige Pilotaufstiege in Westgrönland und Island, Veranstaltet durch die schweizerisch-deutschen Grönland-expedition und das dänische meteorologische Institut," *Beitr. z. Physik d. fr. Atmosphäre,* Vol. 5, 1913, pp. 132–158.

[60] A. de Quervain, "Quer durchs Grönlandeis, Die schweizerische Grönland-Expedition 1912–13," Munich, 1914, pp. 196, pls. 15, figs. 37 and map.

Distribution of Air Circulation in Successive Levels at the Inland-Ice Margin.—De Quervain's data upon wind direction are so vitally important as to merit some further consideration, particularly as regards the distribution of circulation within the different levels; and I have therefore used them to plot the wind-roses for each of the following ranges of altitude: 0–1,000 meters, 1,000–3,000 m., 3,000–5,000 m. (also separately 3,000–4,000 m. and 4,000–

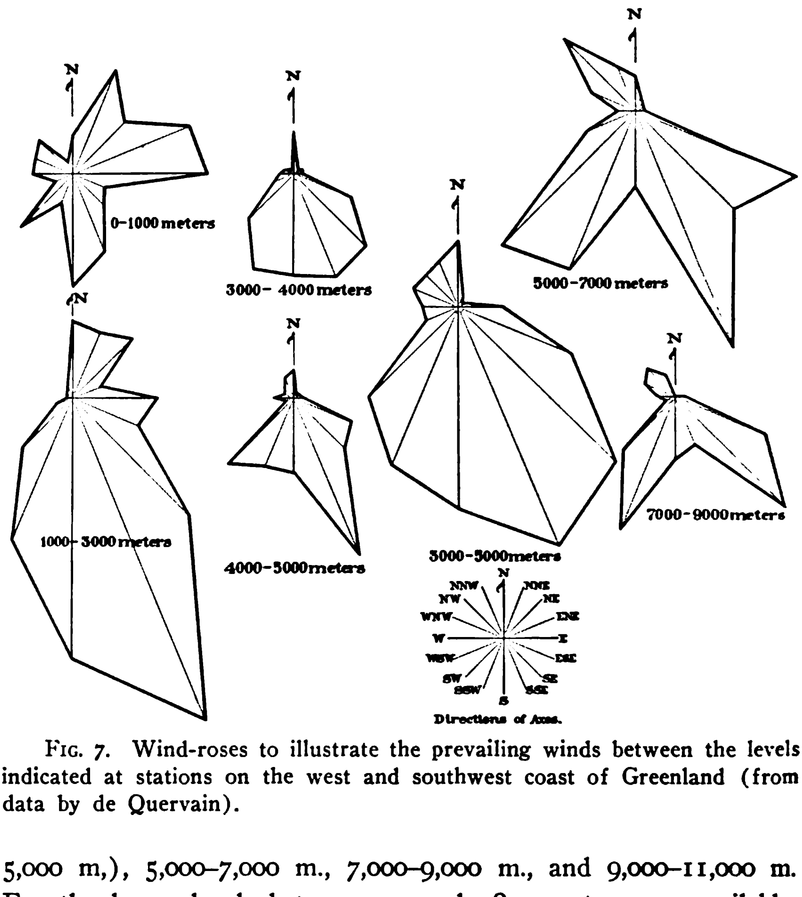

FIG. 7. Wind-roses to illustrate the prevailing winds between the levels indicated at stations on the west and southwest coast of Greenland (from data by de Quervain).

5,000 m,), 5,000–7,000 m., 7,000–9,000 m., and 9,000–11,000 m. For the lower levels between 40 and 58 ascents were available, whereas above 9,000 meters there were 13 and less. The wind-roses have been plotted with weighting for wind force (5 m.p.s. counting as one unit and the nearest unit being taken). Wind

velocities less than 5 m.p.s. were disregarded. The results, which are set forth in Fig. 7, show that below an altitude of 1,000 meters the wind, usually of low velocity, is notably variable and controlled by local conditions. At the level of 1,000 meters the outward flowing currents make their appearance in force and control the circulation up to an altitude of between four and five kilometers, above which level inward blowing currents from the southwesterly quadrant are of equal frequency and of about the same force as the outward blowing currents from the southeast. The clockwise deviation of currents in the anticyclone lead us to suppose that the outward blowing currents start from the interior in a more easterly direction, and that the inward blowing currents from the southwest are almost directly opposed, when they arrive in the interior.

The observations of Wegener made with kites and captive balloons in northeast Greenland, were not generally carried above an altitude of 2,000 meters, though in a few instances considerably higher. They agree among themselves and with those from west Greenland, in showing the presence of relatively variable winds up to about a thousand meters altitude, where these currents are replaced by the strong winds coming down the slope of the inland-ice and increasing in force and in clockwise deviation as one ascends to the limits of the observations. While they are therefore of great interest in revealing the strength and the upward extension of the glacial anticyclone, they have less direct bearing upon the question of circumpolar whirls.[61]

With the above data of Barkow and de Quervain before us, it seems that the time has arrived for laying the specter of the circumpolar whirl, and of returning to an objective basis of reasoning.

Winds About the Margin of the Inland-Ice as a Measure of the Vigor of the Antarctic Anticyclone.

The Zone of Control off "Wilkes Land."—The vigor of a glacial anticyclone may be measured, upon the one hand, by its extension upward from the glacier surface, as has been considered in the last section. Upon the other hand, it may be possible to use the exten-

[61] Wegener, "Drachen- und Fesselballonaufstiege," etc., pp. 55–59.

sion of its circulation outward beyond the glacier margin as an independent measure of its energy. This latter line of inquiry is a particularly fruitful one, for hitherto there has been a general tendency to delimit the zones of wind within the Southern ocean in terms of parallels of latitude.[62] Some years ago under the strong impression that the vigor of the Antarctic anticyclone should dominate within an extra-marginal zone upon the sea, I plotted the wind observations regularly made by the Wilkes Exploring Expedition;[63]

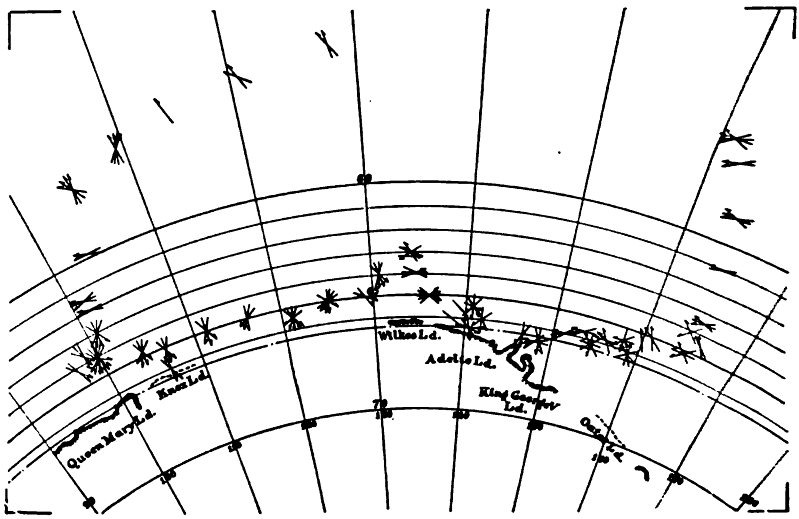

FIG. 8. Map of a portion of Antarctica on which the wind directions recorded by the Wilkes Exploring Expedition have been plotted, but with the margins of the continent corrected so as to accord with Mawson's map. *The arrows point to the wind quarter.*

but was puzzled to find that, whereas there was evident control by the anticyclone within a zone several degrees in width for all points to the westward of long. 150° E., this did not hold for the eastern portion of the route. Now that Mawson has definitely shown[64] Wilkes to have been in error in locating the margin of the continent for that portion of his route to the eastward of longitude 150° E., the apparent lack of harmony which I encountered is suffi-

[62] Cf., for example, Meinardus, l. c.

[63] "Wilkes's Exploring Expedition," Vol. 11 (Meteorology), pp. 272–296.

[64] *Geogr. Jour.*. Vol. 44 (September, 1914), pp. 257–286.

ciently explained. As will be readily seen by reference to Fig. 8, wherever Wilkes was within about three degrees, or some 200 miles, of the inland-ice, the prevailing westerly winds were replaced by southerly and southeasterly ones blowing off the ice. Mawson's own observations leave us in no doubt whatever that this rule of control holds for those margins of the continent which he explored to the eastward of longitude 150° E.

So apparent is the zone of control limited to a belt of 200 miles breadth, at the time of year when Wilkes made his observations, that the winds within and those without this zone for several degrees further, have been plotted in separate roses with results shown in Fig. 9.

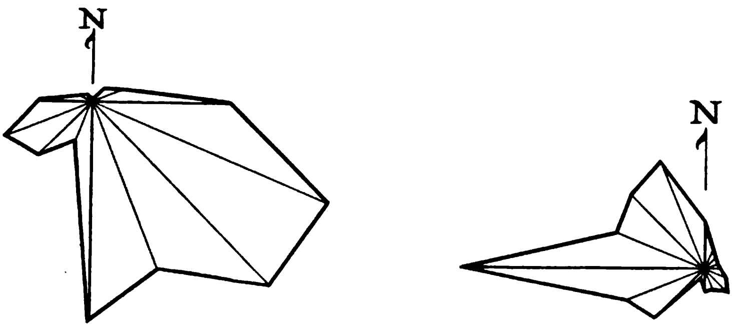

FIG. 9. At the left; wind-rose based upon Wilkes's observations at points distant less than 200 miles from the inland-ice; and, at the right; wind-rose for a zone several degrees in width lying immediately outside the zone of control.

Capt. Davis of the Australian Antarctic Expedition cites an interesting incident in the voyage of the *Aurora* off "Wilkes Land" which indicates he was at the margin of the zone of control.[64a]

The wind observations made by the "Challenger Expedition" at points which we now know to have been near the inland-ice,[65] are confirmation of this conclusion that the effect of the anticyclone extends outward from the margins. Had the observations been

[64a] Home of the Blizzard, vol. 2, p. 40.

[65] Challenger Reports, Summary of Results, First part, chart 23.

those of the first German expedition in 1901–03,[66] offer valuable

[66] W. Meinardus, "Deutsche Südpol-Expedition 1901–03," Vol. 4 (Meteor., Vol. 2), pp. 312–319.

taken in the winter season, it is well nigh certain that the zone of control would have been found much wider.

Effect of the Greenland Anticyclone Upon Migrating Cyclonic Depressions.

Supposed Passage of Cyclones Across the Continental Glacier of Greenland.—A question which has been raised in connection with the Greenland continental glacier concerns the interaction of the glacial anticyclone and the migrating cyclones which have been supposed to move in toward the continent. Upon this assumption it might be held, upon the one hand, that the cyclone temporarily overwhelms the anticyclone, and "springing over it" continues upon its course; or, upon the other, that the cyclone is extinguished by the greater vigor of the anticyclone. Evidence which is now fast accumulating shows that, if the cyclones really advance toward the anticyclone, they are at least halted at its margin, and that both become parts of a system of exchanges planetary in its scope. There is, however, upon the assumption stated the possibility that an especially vigorous cyclone in approaching the Greenland coast during one of the weaker stages in the anticyclonic strophe, may make its influence felt not only upon the near side of the anticyclone but beyond it as well.

Nansen's Observations.—Nansen's conclusion after his crossing of Greenland was, that "the plateau seems to be too high and the air too cold to allow depressions or storm centers to pass across, though, nevertheless, our observations show that in several instances the depressions of Baffin's Bay, Davis Strait and Denmark Strait can make themselves felt in the very interior. We experienced, also, one instance of the crossing of a depression in the storm center which passed over us on September 8. This must have been, according to Professor Mohn, a secondary depression which lay over Baffin's Bay some days before."[67] This was, however, in latitude 64° where the inland-ice is extended southward in a relatively narrow tongue. According to de Quervain on but one occasion during the period of his observations on the Greenland west

[67] "First Crossing of Greenland," Vol. 2, p. 496.

coast, was there "an approximation to establishing" a relationship between an extremely rare northwest wind in the upper levels and a deep low area which lay over the Greenland Sea.[68]

The High Pressure Storms and the "Tauben" Depressions Registered at Danmarks-Haven.—In connection with the series of continuous meteorological observations made at Danmarks-Haven in northeast Greenland, Wegener found that while low pressure areas of normal character arrived at the station, they appeared to proceed from the area of the Greenland Sea; and in the absence of parallel observations, he assumed from the southward. The great storms came with an expansion of the high pressure area lying above the continent—so-called "high pressure storms." During the two years over which the observations extended, there passed over the station on two occasions (October and January), what Wegener has called "tauben"[69] depressions. On these occasions the barometer took a deep plunge with reverse movement, as it does during the passage of a tropical cyclone; yet there resulted neither precipitation of any kind nor any wind worthy of mention. This rather remarkable phenomenon Wegener has sought to explain as due to a cyclonic movement which has "sprung over" the anticyclone above the inland-ice, and in so doing has been robbed of its moisture,[70] and also, it would seem, of its circulation.

In view of all the facts, there is reason to doubt that "low" areas ever get across the larger domes of inland-ice; and the storm paths which Vincent has drawn across the continent of Greenland as though it were an expanse of ocean, should be accorded little weight, though it would seem that Wegener has been somewhat influenced by them.[71]

[68] De Quervain, "Gleichzeitige Pilotballonaufstiege, etc.," p. 146.

[69] Perhaps best translated, "barren," or "sterile."

[70] A. Wegener, "Meteorol. Terminbeob. am Danmarks-Haven," pp. 328, 332–334.

[71] E. Vincent, "Sur la marche des minima barométriques dans la région polaire arctique, du mois de septembre 1882 au mois d'août 1883," *Mem. de l'acad. Roy. de Belgique,* 1910.

The Fixed Low Pressure Areas Marginal to the Inland-Ice Masses.

Antarctica.—The Filchner expedition seems to have established the fact that a fixed cyclonic depression lies off the border of the Antarctic continent covering the indentation of the Weddell Sea.[72] In the light of this discovery it now seems highly probably that a similar fixed depression lies above the indentation of the Ross Sea on the other side of the Antarctic regions and in nearly similar relationship to the inland-ice on either side.[73]

Greenland.—It is well known that a fixed low which is especially marked in the winter season lies off the southeast coast of Greenland, usually assumed to wrap itself about Cape Farewell in the form of a crescent, and extends northward into Davis Straits.[74] Recent studies of the free atmosphere by de Quervain at various points on the west and southwest coasts of Greenland indicate that a stationary area of low barometer (probably continuous with this) extends northward in Baffin's Bay as far at least as Disco Island.[75] The simultaneous studies carried out with pilot balloons at Akureyri in Iceland, indicate clearly that a stationary depression lies over the Greenland Sea to the northward of Iceland and between the Greenland and Norwegian coasts.[76] The Danes from the journeys of bottles set adrift during the expedition of 1906–08, determined that the currents within this sea are such as would indicate a stationary cyclone, since movements were southward along but off the Greenland coast until near the latitude of Iceland, where they are deflected eastward and later northward so as to follow the trend of the Norwegian coast.[77] Thus about both the glacial anticyclone

[72] L. c.

[73] See R. F. Scott, "Voyage of the *Discovery*," Vol. 2, p. 412; L. Bernacchi, "To the South Polar Regions, 1901," p. 298; W. S. Bruce, "Polar Explorations," New York, 1911, p. 187; Simpson, "Scott's Last Expedition," Vol. 2, p. 324.

[74] Cf., for example, Berghaus, "Atlas der Meteorologie," Pls. 33–34.

[75] A. de Quervain, "Gleichzeitige Pilotballonaufstiege in Westgrönland und Island," *Beiträge z. Physik. de Freien Atmosphäre*, Vol. 5, 1913, p. 145.

[76] de Quervain, l. c., p. 146.

[77] Alf. Trolle, "Danmark-Ekspeditionen til Gronlands Nordostkyst, 1906–08, under ledelse af L. Mylius-Erichsen," *Med. om Gronl.*, Vol. 41, 1913. See also, Sir John Murray and Dr. J. Hjort, "The Depths of the Ocean," London, 1912, p. 284.

groups it would now appear that the stationary "lows" are located where land barriers oppose a progressive movement.

The Rôle of the Glacial Anticyclones of High Latitudes in the General Air Circulation.

Circulation is Through Cyclones and Anticyclones, Not Merely Within Them.—In an earlier section it has been shown how the preconceived notion of a polar cyclone, the circumpolar whirl, has held back the advance of knowledge where the polar regions are concerned; and how this theory has now been effectually disposed of by the observations of de Quervain, Stolberg, Barkow and others.

The progressing cyclones within the atmosphere were by Ferrel assumed to be symmetrical in their distribution, with warm upward-moving central portions and cold marginal rims; to circulate the same body of air which repeatedly passes through certain paths; and to have their origin in areas of excessive local insolation. Instead of being symmetrical, as has now so generally been assumed, the study of isotherms in connection with cyclones has shown that these lines usually trend in the United States from southwest to northeast, crossing the cyclone by quite regular paths instead of being circular about its center. The evidence derived from international cloud observations would seem to show that the cyclone is a form of circulation *through which fresh portions of the atmosphere continue to stream;* and both cyclones and anticyclones are to be regarded as eddies which at the surface of the earth have each a hot and a cold side. The same air streams through both, its progress when projected upon the earth's surface being a sinuous line.

Belts of Progressing Cyclones and Anticyclones about the Antarctic Glacial Anticyclones.—The southern hemisphere, being less invaded by the continents, offers for the purposes of study some advantages on the side of relative simplicity, and it has in its meteorological aspects been recently comprehensively treated by Lockyer,[78] who has taken full account of the results of Antarctic

[78] W. J. S. Lockyer, "Southern Hemisphere Surface Air Circulation," etc., Solar Physics Committee under direction of Sir Norman Lockyer, London, 1910, pp. 109, pls. 15.

explorations and has endeavored to show the conjugate relationship of the Antarctic anticyclone area with successive zones of cyclones and anticyclones which migrate in an easterly direction around it. Thus it is found that between the low pressure zones lying within the tropics, and the fixed high pressure area above Antarctica, there are centered near the latitude of 40° S., a series of broad anticyclones which progress eastwardly and produce the effect of a zone of mean high pressure.[79] To the southward of this series of anticyclones and centered near the latitude of 60° S., there are a series of more vigorous cyclones of smaller diameter but progressing eastwardly at about the same angular rate. As we now know from later observations, the stationary cyclones lying over the Weddell and Ross Seas, establish further connection with the anticyclones above the Antarctic continent.

The cold outward flowing currents from the Antarctic continent upon reaching the zones of progressing cyclones are believed by Lockyer to ascend in them upon the west side, thus accounting for the cold western half of these cyclones near the ocean level.

The Australian Antarctic Expedition appears now to have supplied the evidence for such a rise of the air at the southern margin of the progressing cyclones near the borders of Adelie Land. As Mawson puts it:

> "It appeared as if we were situated on the battlefield, so to speak, of opposing forces. The pacific influence of the 'north' would hold sway for a few hours, a whole day, or even for a few days. Then the vast energies of the 'south' would rise to the bursting point and a 'through blizzard' would be the result."

At this junction zone of the glacial anticyclone with progressing cyclones, the air rises to produce rotating cumulus clouds, and it seems not unlikely that the interesting "whirlies" are connected with this uprise.[80]

The air having ascended in a cyclone on its journey northward toward the equator is believed next to pass downward through the progressing anticyclones to the northward, and to reach the ocean's surface as the warm current on the west side of these eddies.

[79] W. J. Humphreys, "On the Physics of the Atmosphere," *Jour. Franklin Inst.*, 1913, pp. 222–223.

[80] Mawson, "The Home of the Blizzard," Vol. 2, pp. 157–8 (fig.).

Mawson's demonstration through wireless communications that the hurricanes of Adelie Land preceded by some 48 hours the arrival of storms at the Australian south coast, would seem to support strongly this view (Fig. 10).[81]

FIG. 10. Map to illustrate the prevailing atmospheric conditions to the southward of Australia (compiled from maps by Lockyer and Mawson).

The Rôle of the Glacial Anticyclones in the General Air Circulation to Draw Down the Air of the Upper Stratum in the Troposphere and to Direct it Equatorward.—From these geographical relationships it appears highly probable that the glacial anticyclones above the inland-ice masses stand in a definite conjugate relationship to stationary cyclones above embayments of the continent.

[81] Cf., also, "The Home of the Blizzard," Vol. 2, fig. opp. p. 141.

The glacial anticyclones of Greenland and Antarctica through drawing down of air from the upper levels and as a consequence of a throughout centrifugal surface circulation, are a very important factor in reversing the high poleward currents within high latitudes and directing them equatorward. The source of energy which

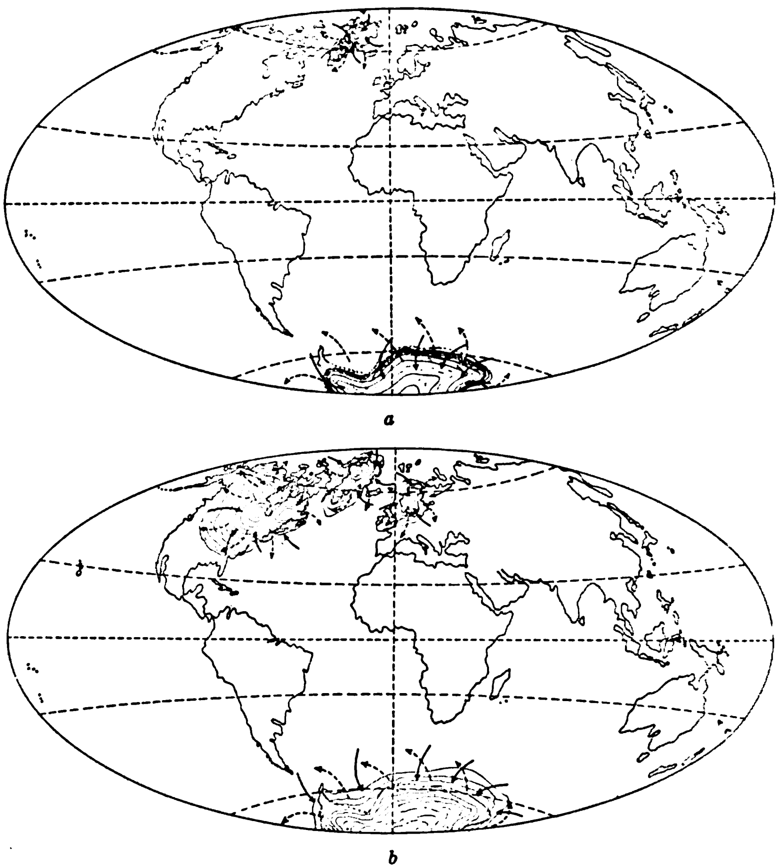

FIG. 11. (*a*) World map to show the present position of the earth's wind poles where the air of the upper stratum within the troposphere is in large part returned to the surface in glacial anticyclones. (*b*) World map to show the corresponding wind poles of the Pleistocene period.

maintains the whole system in motion, is of course the sun's heat concentrated within the tropics and in large measure absorbed over the continental glaciers (Fig. 11a). It is to be assumed that the

uplands of northeastern Siberia, the smaller masses of inland-ice within the Arctic region, and in fact any area where heat radiation is large, contribute in lesser measure to draw down the upper air currents and reverse their direction. It is the unhindered radiation of desert areas which is responsible for the anticyclonic conditions over them in the winter season. Abnormally high insolation in the summer season may, however, overbalance this effect and produce cyclonic effects. The moisture locked up in the ice needles of the cirri and related cloud forms above those areas of ocean where evaporation is large, is thus returned to the earth and especially within the glacial anticyclones. Of this moisture a portion is added to the glacier mass, but at the present time a much larger portion is blown off the glacier surface into the sea and so returned to its source in the waters of the ocean.

University of Michigan,
Ann Arbor,
March 12, 1915.

THE TEST OF A PURE SPECIES OF ŒNOTHERA.[1]

By BRADLEY MOORE DAVIS.

(*Read April 23, 1915.*)

There is probably no group of plants the genetic behavior of which has received so much study as the species of *Œnothera*. No group of plants is more prominently before the attention of experimental plant morphologists, and yet to many botanists it may appear that no group has yielded less of satisfaction. Among the workers with these forms there is the widest divergence of opinion, and of general conclusions there is little to show for the time that has passed since the appearance of "Die Mutationstheorie" in 1901 and the many years of study that De Vries devoted to the group previous to this date.

Can we find the point around which the difficulties cluster most thickly or from which the varied interpretations diverge most sharply? And, finding such a point can we formulate lines of experimentation that may clear the confusion of assumptions from which the various workers have proceeded to follow the lines of study that seemed to them to lead towards the light? To the writer the center of the difficulties lies in the fact that we have no accepted tests for the genetic purity of an *Œnothera* species.

By the genetic purity of a species we mean such a constitution of the germ plasm that a form is able to produce gametes of one type only for each sex. That is to say all male gametes of the form should have the same germinal constitution and thus be physiologically and morphologically equivalent, and all female gametes likewise should be of the same type. The male and female gametes may, however, differ in their respective effects upon the characters of a succeeding generation as shown by the marked differences exhibited by certain reciprocal crosses, for example, the reciprocals between *biennis* and *muricata*, or between *biennis* and *franciscana*

[1] Genetical Studies on Œnothera—VI.

(De Vries '13, Davis '14). The zygotes of a pure species must be uniform since the gametes of each sex are respectively similar, and a pure species, to employ that convenient expression of Bateson's, is therefore homozygous.

It has generally been held that no further proof of the genetic purity of a species is necessary than the established fact that it will "breed true," and I venture to believe that at present most workers among the œnotheras regard this test as entirely sufficient to establish the character of any material with which they work. If any line of *Œnothera* breeds true in large cultures it is confidently regarded as homozygous. Should a line fail to breed true to any considerable degree it is stamped as a hybrid if the investigator inclines towards the methods of analysis characteristic of the Mendelian school. Those who believe in mutations are so fully content with this test that to them a form need breed only reasonably true to pass as a pure species and the departures from the type, called mutations, are interpreted as due to modifications of the germ plasm not, however, the result of hybridism.

If a line of *Œnothera* fails to breed true to a very considerable degree and thus becomes suspected of a hybrid constitution, few workers would think of using it as favorable material for experimental studies to test the mutation theory. It is the lines which breed reasonably true that chiefly form the subjects of *Œnothera* discussions with reference to the theory of mutation. Such a line is the *Lamarckiana* of De Vries's cultures which when grown in large numbers in selfed families appears uniform except for certain small proportions of individuals, "mutants," which stand out clearly from the mass with distinctive characters that are readily recognized and may be clearly described. It is important to note that these new types are not connected by intergrading forms with the parent *Lamarckiana* and that they appear in successive generations of *Lamarckiana* with certain degrees of regularity.

More impressive than this history of *Lamarckiana* which has flowers open-pollinated, and consequently likely in Nature to have been crossed by insects, is the behavior reported for certain lines of *Œnothera* with flowers close-pollinated in the bud, a condition that obviously gives their own pollen the first chance to function and

thus greatly reduces the probabilities of cross-pollination. Such a plant is the *biennis* of Holland and other parts of Europe, a type of especial interest not only for its clear morphological characters but also because there is good reason for believing the line to be very old. This plant forms a large population in Holland with no near relatives and must have lived there for many years to have so thoroughly established itself. Indeed it seems probable that this *Œnothera,* the Dutch *biennis,* has come down to us essentially unchanged from the times of Linnæus who gave us its name. We know of no plant better representative of a species of *Œnothera* and we know of no *Œnothera* which better satisfies the generally accepted requirement that a species should "breed true."

Œnothera biennis L. in large cultures comes so true that hundreds of plants may be grown without finding a single departure from the type. Yet Stomps ('14) in large cultures of selfed lines from a single wild plant collected in 1905 discovered that this Dutch *biennis* throws occasional marked variants ("mutants") and he described a *biennis semi-gigas* with the triploid number of chromosomes (21), a dwarf type *biennis nanella,* and a color variety *biennis sulfurea* with pale yellow petals. De Vries ('15) at once took up the study of certain of the lines established by Stomps and grew cultures which totaled 8,500 plants. Among these were 4 plants of *biennis semi-gigas* about 0.05 per cent., 8 plants of *biennis nanella* about 0.1 per cent., and 27 plants of *biennis sulfurea* about 0.3 per cent. Since the percentages from *Lamarckiana* are for *semi-gigas* 0.3 per cent. and for nanella 1 to 2 per cent. it should be noted that with respect to these "mutants" *biennis* appears to be the more stable of the two species, although the color variety *biennis sulfurea* constitutes a new type of variant in experimental studies on œnotheras. A culture of over 1,000 plants from selfed seed of *biennis sulfurea,* all with pale yellow flowers, produced 2 dwarfs thus establishing a "double mutant" *O. biennis* mut. *sulfurea* mut. *nanella.*

As evidence for the mutation theory of De Vries this behavior of the Dutch *biennis* is to the writer much more trustworthy evidence than the behavior of *Lamarckiana* for the reason that the latter plant in his opinion does not have a clear record of long

existence, and probably is a form of comparatively recent origin. De Vries ('15, p. 173) has asserted again most vigorously his belief that *Lamarckiana* may be identified with a specimen from the United States collected by Michaux and now in the collections of the Museum d'Histoire Naturelle in Paris (De Vries, '14). With this view I cannot accord for reasons recently published (Davis, '15*a*). The showing of "mutants" from *Œnothera biennis* can hardly be considered very encouraging for the mutation theory of organic evolution when it is remembered that *biennis semi-gigas* is self sterile, that *biennis nanella* is frequently weakly or diseased, and that *biennis sulfurea* is clearly a retrogressive type having lost the power of producing normal yellow flowers.

Although *O. biennis* of all the œnotheras brought into the experimental garden still seems to me the form most free from suspicion of gametic impurity, nevertheless the line of Stomps has not, so far as we know, been subjected to the tests of a pure species summarized at the conclusion of this paper. De Vries ('15, p. 173) is mistaken in quoting me as conceding for this species a pure origin. I regard it simply as the safest material yet known on which to conduct studies in mutation, and with which other forms may be crossed to determine by the constitution of the F_1 hybrid generation whether or not their gametes are uniform. If in such a breeding test the F_1 progeny fall into two or more classes the assumption is justified that the form crossed with *biennis* must produce different classes of gametes. If the F_1 hybrid generation is uniform then it is clear that the functioning gametes male and female are respectively uniform. The fact that *Lamarckiana* crossed with *biennis* produces the "twin hybrids" *laeta* and *velutina* is, as has frequently been pointed out, one of the most important facts favoring the hybrid nature of *Lamarckiana*. It seems to me not improbable that other species of *Œnothera* will eventually be isolated more stable than the Dutch *biennis*.

Some exceedingly interesting observations have recently been reported by Bartlett ('15 *a, b, c*) on the behavior of certain small-flowered, self-pollinated American œnotheras. When grown in selfed lines these forms exhibit a behavior similar to that of *Lamarckiana* and *biennis* in throwing off in successive generations

certain new types. Thus from one of the species, *Œnothera stenomeres,* a mutant *gigas* appeared with the diploid number of chromosomes, and from another species, *O. Reynoldsii,* certain individuals throw from 60 per cent. to 80 per cent. of dwarfs. It is too early to discuss the remarkable peculiarities of these forms since the material of Bartlett has not yet been tested for its purity along the lines presently to be discussed. Bartlett regards the new types as "mutants" in the sense of De Vries. The important point for our consideration at present is the fact that these wild plants apparently continue to reproduce themselves from generation to generation even while giving rise to the new forms.

With respect to the taxonomic status of the plants which we have just considered the writer sees no alternative but their recognition as clear species. The *Lamarckiana* of De Vries, the *biennis* of Linnæus, and most of the types which Bartlett has segregated from the American wild œnotheras breed true as to the mass of their progeny. What further qualifications can taxonomy in reason demand? Species they are by virtue of their morphology and by the test of the experimental garden which shows their characters to be stable to an extent that renders it certain that each line self-pollinated will maintain itself unchanged, indefinitely as far as we can see, through successive generations.

The argument that will follow as to the genetic constitution of these species of *Œnothera* does not in the least affect the matter of their recognition in taxonomy as species. It may be prefaced by two questions stated as follows: Are the types pure species, homozygous because the plants develop male gametes of one type only and because their female gametes have a uniform germinal constitution? Or, are the types heterozygous developing different types of male gametes and different types of female; briefly expressed have they in some degree a hybrid constitution?

But it will at once be asked, how can a species be hybrid even to a small degree and yet breed as true as do these forms under consideration? Where in their behavior is evidence of a hybrid constitution such as might appear in the splitting off of numerous different forms varying from the parent type, some in small degrees and some in larger degrees? Where is evidence of an orderly segre-

gation of characters such as has been demonstrated by the Mendelian research of recent years? To these questions it must frankly be answered that only here and there are glimpses of situations which may possibly be interpreted in terms of Mendelian analysis. For example the characters of the "mutants" are frequently clearly retrogressive which indicates that gametes are formed lacking certain factors and suggests phenomena characteristic of segregation from heterozygous stock and very common in Mendelian behavior. Again, the repetition of the same "mutants" in a series of generations suggests a mechanism of precision such as we have come to associate with Mendelian inheritance. It is not, however, my purpose to argue at present this phase of the discussion for the experimental data before us is not in such shape that it can be handled to the best advantage. We admit that the "mutants" themselves do not establish their parents as in their nature hybrids. If they did there would of course be no discussion.

Under two conditions and apparently two only can a heterozygous species be conceived as breeding true.

First, if of the varied possible types of gametes *only such unite and produce fertile zygotes as will perpetuate the same germinal constitution as the parent,* then from such zygotes a heterozygous line might continue indefinitely as an impure or hybrid species. Under such conditions gametes which might in varied combinations give a series of different forms (segregates) are either not matured or if matured fail to function. Some degree of pollen and ovule sterility must be expected as the result of such conditions.

Second, if of a varied assortment of zygotes formed by the union of different types of gametes, *only those develop which have the germinal constitution of the parent* then again a heterozygous line might continue indefinitely and constitute a species, although impure or hybrid in its nature. Since all of the zygotes which result from other combinations of gametes either die or fail to develop beyond some early stage in the life history this condition would result in some degree of seed sterility or in the production of weak plants that must soon perish.

Now the œnotheras as a group exhibit a very remarkable amount of pollen sterility and also a high degree of ovule abortion, and

these plants frequently give extraordinarily low yields of fertile seeds although seed-like structures may be formed in abundance. These facts we are just beginning to appreciate as offering problems for study. They seem to the writer of vital importance to the discussion of *Œnothera* genetics, facts which the Mutationists cannot ignore and behind which the Mendelians can maintain at present a very strong defence for their interpretations of the peculiarities of *Œnothera* behavior.

With respect to pollen sterility it has for many years been known that *Lamarckiana* and other species of *Œnothera* present large proportions of abortive pollen grains. Bateson (1902) early seized on the point and suggested that the high degree of pollen abortion in *Lamarckiana* indicated a hybrid plant exhibiting partial sterility. Geerts ('09) in an excellent account of the cytology of *Lamarckina* showed that approximately one half of the pollen grains fail to mature and that one half of the ovules fail to develop embryo sacs. Geerts ('09, p. 89) also made an examination of more than one hundred species of the Onagraceæ, giving us the conditions of pollen and ovulue fertility represented in some fifteen genera. He found generally in species of *Œnothera* and allied genera a degree of sterility similar to that in *Œnothera Lamarckiana*, about 50 per cent. for both pollen and ovules. On the other hand certain species of *Jussieua, Zauschneria, Epilobium, Boisduvalia* and *Lopezia* are wholly or almost wholly fertile.

My own examination of conditions in the material of *Œnothera* with which in recent years I have worked has shown some remarkable differences in the amount of pollen and seed sterility. Such close pollinated types as the Dutch *biennis,* the Dutch *muricata,* American *muricata* (from Woods Hole), *Tracyi,* and a number of American small-flowered species (for example *biennis* A and *biennis* D of my cultures (Davis, '11, p. 197 and '12, p. 385)), have very large amounts of sterile pollen. In the case of the Dutch *muricata* much more than 50 per cent. of the pollen has been sterile. Yet these are types which by virtue of their long history of close pollination might be expected to be among the purest of the species. On the other hand the race *grandiflora* B (Davis, '11, p. 203), and the western species *franciscana* and *venusta,* all open pollinated

species show hardly more than a trace of pollen abortion, and *Jamesii* from Texas only a small amount of sterile pollen. I have this winter tested the seed fertility of some of these species by germinating the seeds in Petri dishes after the method recently described (Davis, '15*b*). The Dutch *biennis* gave a germination of about 96 per cent., the Dutch *muricata* about 72 per cent., *grandiflora* B about 95 per cent., *franciscana* about 61 per cent., *venusta* about 87 per cent., and *Jamesii* about 91 per cent.

It is interesting to note in the above list that the Dutch *biennis* with its very high percentage of fertile seeds (96 per cent.) has extensive pollen abortion and the Dutch *muricata* with seed germination of about 72 per cent. has an even lower degree of pollen sterility. On the other hand there are species of *Œnothera* with both high seed and pollen fertility as illustrated by some races of *grandiflora, venusta* and *Jamesii.* I was especially interested in the conditions shown by my race *grandiflora* B with its almost perfect fertility both as to pollen and seeds. This race isolated from a collection of mixed seeds gathered by Tracy in 1907 at Dixie Landing, Alabama, has always seemed to me to present a type of unusual purity. The line was started in 1908 by a cross of two similar plants (Davis, '11, p. 203) representing the broader-leaved forms of *grandiflora* that were present at Dixie Landing and I have grown in small cultures several generations of the plant without noting departures from the type. I cannot accept the criticism of De Vries ('14, p. 348) that my race *grandiflora* B is impure because from the same collection of mixed seeds of Tracy's he obtained a diversified culture as I also reported (Davis, '11, p. 203) when the line was first isolated, and because De Vries and Bartlett found the Dixie Landing station "desolate" five years after the visit of Tracy. This type may prove to be nearer to the desired pure species than the Dutch *biennis.*

Jeffrey in recent papers ('14*a,* '14*b,* '15) has taken the position "that in good species the spores or pollen is invariably perfect morphologically" and from this standpoint refuses to consider *Lamarckiana* and other œnotheras as suitable material on which to base experimental studies on mutations. To him the mere presence of

abortive pollen suffices to stamp a form as hybrid in character. This represents an extreme view which in consideration of our ignorance of possible physiological reasons for pollen sterility can at present scarcely be claimed as more than an hypothesis. For the œnotheras we are greatly in need of cytological and physiological studies on pollen sterility more detailed than the incidental observations that have so far been published.

With respect to the abortion of ovules among the œnotheras our information is practically confined to the observations of Geerts ('09), mentioned above. It appears that in *O. Lamarckiana* and a number of other species only about 50 per cent. of the ovules develop embryo sacs. Other species also show varying degrees of ovule abortion. The ovules that fail to mature are represented in the capsules by a fine light brown powder known to all who work with œnotheras. Such powder is very common in the capsules of various species and their hybrids, and it seems probable that ovule sterility is as widespread in this group of plants as is the degeneration of the pollen. As in the case of pollen sterility we do not know to what extent physiological conditions may also be responsible for the abortion of ovules.

Pollen and ovule sterility involve of course the elimination from the life history of immense numbers of gametes and raise the following questions. Can it be that this elimination throws out of the life cycle types of gametes with germinal constitutions different from the gametes that matured and that function? It is possible that some of the *Œnotheras* species, in hybrid condition, regularly mature for the most part particular classes of gametes which in conjugation will perpetuate the genetic line of the parent plant? Gametes even when normally developed may still not function as when pollen grains fail to germinate upon the stigma because its secretions are not suitable. It must also be borne in mind that there are yet other phases of the life history when gametes may become ineffective as through failure to conjugate or because of a high mortality among zygotes, embryos, or young plants; such forms of infertility are expressed in sterile seeds or in weak offspring which never mature. Possibly the so-called "mutants" arise when unusual gametes from hybrids, occasionally surviving the ex-

tensive process of degeneration, form zygotes also able to survive and to develop plants diverging from the parents.

The subject of seed sterility among the œnotheras has scarcely been touched by the students of the group and yet it seems likely to become a factor of prime importance in its bearings on the problems of *Œnothera* genetics. Any worker among these plants shortly becomes aware of the fact that very many of the seed-like structures which he sows fail to germinate even though seed pans are kept for many weeks. De Vries makes frequent reference to the facts of seed sterility and the writer has in recent years recorded the number of seeds sown in cultures and the number of seedlings that develop. The results are most surprising and must have significance although what that may be remains for the future to disclose. A line of research has opened before us that will demand a special technique, for it is not enough to know merely that certain proportions of the seeds germinate within the time practicable for keeping seed pans under observation.

Seed-like structures sown on the earth are obviously lost for further enquiry as to the facts of their viability; a proportion of seedlings appear but as for the residue, that cannot be examined. The residue may contain viable seeds the germination of which is delayed, or it may consist wholly of sterile structures. We must develop methods that will ensure the rapid and complete germination of seeds in convenient receptacles such that the residue of sterile structures may be left for study after the seedlings have been removed and set in the earth. By such methods cultures of *Œnothera* may be grown in which one may feel confident that all of the viable seeds have germinated since by an examination of the residue it may be determined whether or not the seed-like structures have embryos. It is probably safe to say that no culture of *Œnothera* has as yet been described in which we may feel certain that the progeny of the sowing is complete. During the past winter I have tested the percentage of seed fertility in some fifty species and hybrids of *Œnothera* germinating the seeds on pads of wet filter paper in Petri dishes. With this method may advantageously be combined the clever practical suggestion of De Vries ('15, p. 190) of forcing water into wet seeds by air pressure thereby greatly

hastening their germination. A description of a method of seed germination which will, I think, prove to be satisfactory in genetical work on *Œnothera* may be found in the Proceedings of the National Academy of Sciences, Vol. I., p. 360, 1915.

The first investigator to make use of the facts of seed sterility in suggesting Mendelian interpretations of the behavior of *Lamarckiana* and certain *Œnothera* crosses has been Renner ('14) and his line of investigation has opened a field of research and speculation that must be reckoned with in the future. Renner has studied the seed structure in *Lamarckiana, biennis* and *muricata,* and in certain crosses among these forms. His conclusion on the genotype of *Lamarckiana* will illustrate the principles underlying the method of attack. Since *Lamarckiana* when crossed with *biennis* and certain other species gives in the F_1 hybrid generation the twin hybrids *læta* and *velutina* it may be assumed to develop two classes of gametes which function. These may be spoken of as the *læta* and *velutina* gametes and are produced in about equal numbers. When *Lamarckiana* is self-pollinated the *læta* and *velutina* gametes may combine in proportions to give 1 pure *læta*: 2 *læta-velutina*: 1 pure *velutina.* It is a fact that more than one half of the seeds of *Lamarckiana* fail to develop normal embryos and Renner concludes that these sterile seeds represent zygotes homozygous respectively for the *læta* and *velutina* factors. The fertile seeds develop from the heterozygotes with both *læta* and *velutina* factors combined and this combination gives the characters of *Lamarckiana.* *Œnothera Lamarckiana* may thus be an impure or heterozygous species breeding true because of the death of such zygotes as carry the factors for *læta* and *velutina* in homozygous conditions. This simple Mendelian explanation of the behavior of *Lamarckiana* points a line of interpretation and study certain to be fruitful in *Œnothera* research.

Among hybrids of *Œnothera* the seed sterility sometimes runs extraordinarily high. The most remarkable illustrations of this fact so far known appear in the second generations of crosses involving the Dutch *biennis* and the Dutch *muricata* which exhibit certain remarkable morphological peculiarities discovered and described by De Vries ('13). First generation hybrids of reciprocal crosses

between these species grown by the writer in 1913 gave data on seed germination in the earth as presented in Table I.

TABLE I.

F_1 Hybrids of Reciprocal Crosses between *O. biennis* and *O. muricata.*

Culture.	Cross.	Seeds Sown.	Sown in	Seedlings.	Germination.	Duration of Experiment.
13.33	F^1 *biennis* × *muricata*	673	Earth	139	20%	6 weeks
13.34	F^1 *muricata* × *biennis*	153	Earth	97	63%	7 weeks

It is probable from my experience with other species crosses that the viability of the seeds of these F_1 hybrids is really high and that the relatively low percentages recorded above are due to delayed germinations. But the figures for germination in the earth of F_2 hybrids and of double reciprocals, sesquireciprocals, and iter-

TABLE II.

F_2 Hybrids of Reciprocal Crosses between *O. biennis* and *O. muricata*, Including Certain Double Reciprocals, Sesquireciprocals, and Iterative Hybrids.

Culture.	Cross.	Seeds Sown.	Sown in	Seedlings.	Germination.	Duration of Experiment.
14.41 (13.33a)	F_2, *biennis* × *muricata*	466	Earth	8	1.7%	9 weeks.
14.42 (13.34c)	F_2, *muricata* × *biennis*	205	Earth	35	12%	9 weeks.
14.43 (13.33a × 13.34)	double reciprocal (*b* × *m*) × (*m* × *b*)	73	Earth	8	11%	9 weeks.
15.31 (14.33 × 14.16)	sesquireciprocal (*b* × *m*) × *b*	267	Earth	25	9%	9 weeks.
*15.31 (14.33 × 14.16)	sesquireciprocal (*b* × *m*) × *b*	282	Petri dish	132	46%	6 weeks.
15.32 (14.16 × 14.33)	iterative *b* × (*b* × *m*)	22	Earth	1	4%	9 weeks.
15.33 (14.33 × 14.20)	iterative (*b* × *m*) × *m*	212	Earth	2	0.9%	9 weeks.
*15.33 (14.33 × 14.20)	iterative (*b* × *m*) × *m*	292	Petri dish	42	14%	7 weeks.
15.34 (14.34 × 14.16)	iterative (*m* × *b*) × *b*	217	Earth	47	21%	9 weeks.
*15.34 (14.34 × 14.16)	iterative (*m* × *b*) × *b*	373	Petri dish	73	19%	4 weeks.
15.35 (14.34 × 14.20)	sesquireciprocal (*m* × *b*) × *m*	246	Earth	43	17%	9 weeks.
*15.35 (14.34 × 14.20)	sesquireciprocal (*m* × *b*) × *m*	498	Petri dish	198	39%	7 weeks.
15.36 (14.20 × 14.34)	iterative *m* × (*m* × *b*)	198	Earth	51	25%	9 weeks.

ative hybrids are most surprising in the degree of sterility or delayed germination shown. They are given in Table II., where are also presented the records of four cultures sown in Petri dishes in which the germination was complete as proved by an examination of the residue.

A comparison in Table II. of the record for culture 15.31 with *15.31, 15.33 with *15.33, and 15.35 with *15.35 will illustrate the gain in germination that may come through sowing seeds in Petri dishes. The percentages of germination presented above for the hybrids of *biennis* and *muricata* must not be regarded as expressing exactly the degree of seed fertility under the conditions of the experiments since with the harvests of seed are frequently found very many structures too large to be abortive ovules and too small to be counted as "seeds" in the sense of falling within the limits of seed size. These structures are probably undeveloped seeds but only a microscopical examination can determine this point; if so, their presence of course always lowers the percentage of zygotes capable of giving progeny.

Bearing in mind the fact that pollen sterility in *biennis* and *muricata* is 50 per cent. or more and that pollen abortion in the F_1 hybrids is very much higher (in fact very little good pollen is produced) the total amount of sterility both gametic and zygotic is simply amazing. Under such conditions how can the behavior of these hybrids be looked upon as indicative of anything but a most unusual situation, in itself very interesting, but far beyond the expectations of normal hybrid behavior. This remarkable degree of sterility among the hybrids of *biennis* and *muricata* is perhaps extreme for the œnotheras, but it serves to illustrate conditions extensively present in the writer's experience and doubtless also in the experience of others.

De Vries has described the hybrids between *biennis* and *muricata* as breeding approximately true which in the main has also been my observation. Apparently largely upon this behavior and that of certain other crosses he has reached the conclusion that hybrids between species of *Œnothera* are stable. In this opinion of De Vries I cannot agree for my crosses between *grandiflora* and certain small-flowered American species (Davis, '12 and '13), and between

biennis and *franciscana* have in the F_2 generations given abundant evidence of that extensive variation interpreted as segregation. I believe that the apparent stability of the very small progenies produced by hybrids of *biennis* and *muricata* simply means that the remarkably high mortality among gametes and zygotes of these hybrids, or the delayed germination of their seeds, has prevented the appearance in our cultures of the diverse types which theoretically would be expected. Any general conclusions on genetic behavior in the œnotheras which fails to take into account the phenomena of sterility rests upon insecure foundations.

It is true that we do not know to what extent physiological factors may affect seed sterility as well as pollen and ovule abortion. Nevertheless a main fact is clear, namely that seed sterility eliminates in certain *Œnothera* species and hybrids immense numbers of zygotes which fail to develop seeds. And, furthermore, we know for œnotheras that large classes of weak offspring are sometimes produced that are unable to reach maturity. Seedlings with white or yellow cotyledons, which quickly die, are not uncommon in my experience with *Œnothera* cultures; in certain cases they have appeared in very large numbers (Davis, '11, p. 222) and probably have important genetical significance. This situation in *Œnothera* finds a close parallel in the behavior recorded for a number of animals and plants. Thus Baur's "golden" variety of *Antirrhinum* is an impure or heterozygous form which besides reproducing itself throws a class of normal green plants and a class represented by weak yellow seedlings that shortly die. The yellow mice studied by Castle and Little although interbred always remain impure giving progeny heterozygous for yellow because of the death of zygotes with a double dose of the factor for yellow. A dwarf wheat isolated by Vilmorin cannot be fixed since it always remains heterozygous throwing talls but never producing homozygous dwarfs. The white female form of the clover butterfly, *Colias,* was found by Gerould always to give yellow offspring either because of the failure of the gametes carrying white to conjugate or because zygotes homozygous for white fail to develop. A form of *Drosophila* characterized by confluent wings has been found by Metz only in the heterozygous condition, always throwing normals and never breed-

ing true; flies homozygous for confluent wings are apparently not viable. Is it not possible that parallel or related phenomena are extensively present among the œnotheras? The mortality as shown by sterile seeds may indicate the elimination of large groups of forms divergent from the parent types, and some of the curious dwarfs and aberrant plants which again and again have been reported in *Œnothera* lines may be from zygotes barely able to survive the death-producing conditions that eliminate so many of their companions.

So far we have considered evidence chiefly of a negative character for the contention that many of the species of *Œnothera* are impure or hybrid species. We have tried to show that pollen, ovule, and seed sterility must all be reckoned with as conditions which may eliminate Mendelian classes of gametes and hold a line to a history of relatively true breeding even though the stream of germ plasm remain heterozygous or impure in character. The natural corollary of such behavior, if proven, might be the interpretation of so-called "mutants" as segregates from a hybrid stock that were able to survive the destruction meted out by conditions that produce sterility. To what extent the causes of sterility may lie in the history of gametogenesis or may be due to unfortunate combinations of gametes, or to what extent sterility is the result of physiological factors, these are problems that lie before us.

Let us now examine some positive evidence that certain species of *Œnothera* do form distinct classes of gametes and in consequence seem likely to be heterozygous in their constitution. That which first demands attention is the situation discovered by De Vries in certain first generation hybrids and by him named "twin hybrids." We have already referred to this phenomenon first described by De Vries ('07) for the behavior of *Lamarckiana* which as a pollen parent in crosses with other species of *Œnothera* gives not uniform F_1 generations but the two types *læta* and *velutina* (twin hybrids), produced in about equal numbers. Certain "mutants" of *Lamarckiana* also give twin hybrids under the same conditions as those produced by *Lamarckiana*. The behavior is so exact that the simplest hypothesis must suppose that *Lamarckiana* and these "mutants" form two classes of gametes which are fertile in these par-

ticular crosses. De Vries ('09) has also described "triple hybrids" when the "mutants" *scintillans* and *lata* are pollinated by such species as produce the twin hybrids from *Lamarckiana*. In such cases two of the forms have the characters of *læta* and *velutina* combined with those of the other parent, and the third form resembles the mother, either *scintillans* or *lata*. The phenomena of twin and triple hybrids is treated in detail by De Vries ('13) in "Gruppenweise Artbildung."

From a Mendelian standpoint the production of twin and triple hybrids is strong evidence that *Lamarckiana* and such of its "mutants" as behave in this manner are impure or hybrid since the male or female gametes are not uniform, a point which has been emphasized by several critics of the mutation theory. De Vries assumes that *Lamarckiana* forms its different classes of gametes as a result of its mutating instability but the precision of the process falls completley in line with what we know of Mendelian behavior. The remarkable studies of Shull show that crosses between *Lamarckiana* and *cruciata* give in the first generation polymorphic progenies of much greater complexity than the twin hybrids of De Vries. Shull's results have not been published in full but, as I understand them, they indicate the interaction of several classes of gametes, a condition very far from what would be expected if genetically pure species had been crossed.

Very interesting are the observations of Atkinson ('14) on first generation crosses between *Œnothera nutans* and *O. pycnocarpa*. These two forms are American species recently segregated by Atkinson and Bartlett from the *biennis* alliance. They have bred true in garden cultures. When *pycnocarpa* is pollinated by *nutans* twin hybrids appear in the first generation. In the reciprocal cross *nutans* × *pycnocarpa* the same twin forms are produced and in addition a third type, making this generation a compound of three distinct forms, triple hybrids. Atkinson, apparently confident of the genetic purity of *nutans* and *pycnocarpa* assumes that the determination of the twin and triple hybrids takes place through a differential division in the zygote by which factors representing certain characters are side tracked in the suspensor cell and only those responsible for the twins and triplets pass on to the embryo. There is no

cytological evidence that the first mitosis in the zygote of a higher plant is ever a differential division. To the writer the situation indicates that one or both of the two species is heterozygous and that for this reason classes of gametes are formed, appropriate combinations of which give the twins and triplets. No data has been published respecting the sterility of these two species, either of pollen or ovules, and nothing of seed abortion. An understanding of the genetic constitution of the species is likely to be a difficult matter, but it does not seem probable that both are pure.

What shall be said of the probable purity of the plants of *Œnothera* and *Raimannia* with which MacDougal worked in his experiments designed to create new species by the injection of certain fluids into the ovaries. The parent material was reported to breed true, but the cultures were small and not long continued and there is no reason to suppose that a complete germination of the seeds was obtained. No information is given on the fertility of the species either with respect to the abortion of gametes or the proportion of good seeds. The material was not tested by cross breeding with other forms (the purest known) to determine whether the F_1 hybrids were uniform, a most necessary test in the establishment of a stock as homozygous. Thus from our present viewpoint we cannot accept MacDougal's conclusion since the probabilities are very great that the new types which appeared in his cultures were produced not as the result of the injections but because of the genetic impurity of the plants themselves.

In the above discussion the writer has taken definitely a Mendelian attitude in sympathy with the criticisms of Bateson and the studies of Heribert-Nilsson ('12) and of Renner ('14). There are constant suggestions of order in the phenomena of inheritance among the œnotheras which while they may not fall into simple schemes of Mendelian notation nevertheless do indicate system even though masked by complexities. That the complications at least in great part are due to the genetic impurity of the *Œnothera* material which has been so far the subject of study is the writer's belief. The difficulties that surround the analysis of *Œnothera* inheritance are probably in very large measure due to the extraordinary amount of sterility, gametic or zygotic, or both, that is present in the group.

Upon students of this genus rests the responsibility of obtaining data on this sterility and, if possible, of discovering its causes. The assumption that a line represents a pure species because it breeds true is not a safe foundation upon which to conduct experimentation in the œnotheras. This is the assumption upon which have been based many of the conclusions of the Mutationists, and from it we must dissent. We cannot depart from the principles underlying Mendelian methods of research which have so brilliantly opened the present century of biological investigation.

Finally what are the tests that must be applied to an *Œnothera* species to determine whether or not it is pure.

First.—There is the breeding test and that must be applied with such experimental methods of seed germination (Davis, '15) as will insure a complete progeny from the sowing, a progeny wholly representative of all types of viable seeds. Even then the breeding test is negative rather than affirmative in its conclusions. Should the form throw off numerous variants it naturally becomes a subject of suspicion, but should it breed true or relatively true that does not in this group of plants prove it to be homozygous in its germinal constitution.

Second.—Information must be obtained on the character and degree of sterility present, both gametic and zygotic. Sterility, unless shown to be strictly physiological in its character, suggests genetic impurity.

Third.—Cross-breeding tests must be planned and followed in which the form under observation is mated with material of known genetic purity. If the hybrid plants of the first generation are essentially uniform and the result of a normal germination of the seeds the indications are strong that the form is truly pure provided that the gametes are likewise normally fertile. If the hybrids of the first generation fall sharply into classes the material must develop gametes of different germinal constitutions and is consequently heterozygous. One favorable cross with a pure species may not be sufficient to establish the purity of a form; a number of favorable tests with pure types will carry increasing conviction.

It is thus not an easy matter to determine the fact whether or not a species of *Œnothera* is pure, and yet this is fundamental to

experimental studies in the group. On the assumption of specific purity the Mutationists rest their conclusions. This condition with respect to the characters studied is also basic to Mendelian experimentation. It need scarcely be emphasized that no species of *Œnothera* has as yet passed the tests for genetic purity outlined above and that consequently we have at present no standard material with which forms may confidently be mated in the test of cross-breeding. It should become the concern of *Œnothera* geneticists to find and isolate pure material as the starting point of further studies in experimental morphology. Whether such pure forms will be found among the wild species or as products of the garden time will determine.

University of Pennsylvania,
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CONCRETIONS IN STREAMS FORMED BY THE AGENCY OF BLUE GREEN ALGÆ AND RELATED PLANTS.

By H. JUSTIN RODDY, M.S., Ph.D.

(*Read May 7, 1915.*)

In 1898, I discovered that concretionary formations occurred in Little Conestoga Creek, Lancaster County, Pa. At that time, however, I was engaged in other studies and gave the concretions only a passing notice. But in the late summer of 1914, my attention was directed to the subject again by the reading of Dr. Walcott's paper on "Pre-Cambrian Algonkian Algal Formations" which appeared July 22, 1914. This paper made me realize the importance of a careful investigation of these particular stream formations as to characteristics, distribution, origin, etc. I began at once a careful and extended search in the Little Conestoga as well as in other streams for concretionary structures of recent formation. My search was amply rewarded by finding them in great quantities, and distributed throughout nearly the entire length of the Little Conestoga. I found also that they not only occur in the creek itself, but that quite large deposits of the concretions underlie the flood plain meadows along the creek banks. One of these in Kendig's Woods, two miles southwest of Millersville, Pa., is made up wholly of concretionary materials on the top of which forest trees of large size and considerable age are growing. This deposit covers nearly an acre to the depth of about 8 feet in the middle thinning out lenslike toward its edges. Another deposit along the same stream near Fruitville in Evan's Meadow, more extensive in area but of slighter depth, forms a substratum under a thick soil cover and has an average depth of about two feet. Deposited concretions occur under similar conditions in many other of the meadows along the stream as is shown by weathered concretions occurring in the soil and wash wherever wet-weather stream gullies have been torn through the soil cover.

Though these structures, as I shall show later on, are without doubt due to Algoid agency in the stream waters, it may be well to premise the full discussion of their origin by somewhat complete descriptions of their characteristics as to form, size, structure, etc. In this way the attention of botanists and geologists will be directed to their study and distribution, so that their significance as agents of rock formation and the flora, responsible for their growth, may be fully worked out.

Size and Shape.—The concretions both in the stream and in the deposits vary in size from peas to masses nearly a foot in diameter (see Fig. 1). The latter size is not very common in the

FIG. 1. A group of the concretions showing their size, shape, surface appearance and color. No. 1 is 7½x10 inches; No. 2 is about 5 inches in face diameter and 3 inches thick; No. 3 is 8x7x5 inches. The two smaller concretions above are typical, both in color and surface appearance, of growing specimens.

stream but many large concretions occur in the deposits probably because the smaller ones after deposition in land forms have been carried away in solution by percolating waters leaving only the larger forms. In the flood deposits in Kendig's Woods thousands of the concretions when I found the deposit last summer measured nearly a foot in length and six inches or more in transverse diameter.

The smaller concretions are invariably ellipsoidal in shape (see Fig. 1), and quite symmetrical unless broken by flood action. The larger sized concretions, though of the same general shape, are less symmetrical. Those in the stream are nearly always more regularly ellipsoidal than those of the deposits in flood plains and stream bars. This is, no doubt, due to their weathering through solution or to their having been broken by flood waters during their transportation to their present positions.

The concretions in the stream are quite firm in texture; those in the deposits are less compact. Both are porous and roughly coralline in general appearance and internal structure.

In color they vary from bluish green to whitish. The growing specimens in the stream are generally bluish green. All specimens after exposure for some time to sun, air, and rain or to the action of soil waters become grayish white.

Composition and Hardness.—Though the composition varies slightly from place to place yet all are limy deposits concentric around a nucleus. The main constituents in the concentric layers are calcium carbonate, silica and organic matter of vegetable origin. Upon dissolving out the limy constituents with dilute hydrochloric acid, a mat is often left of vegetable materials composed of the matted stems or tissues and cells of low type plants such as mosses and algæ.

Few of the specimens tested had a hardness as great as that of common calcite, most of them being about two in the scale of hardness. The weathered concretions are generally less coherent than those now forming in the stream.

The following table shows the main constituents of the concretions:

Constituents.	*A.*	*B.*
Organic matter	10% to 15%	1 to 12%
H_2O	1%	1%
SiO_2	12%	12%
$CaCO_3$	60% to 75%	70 to 80%
Fe	1%	2%
Al	Trace	Trace
$MgCO_3$	Trace to 1%	Trace to 1%

A of growing specimens.
B of specimens from flood plain deposit.

Structure.—Most specimens have as the nucleus a quartz or limestone pebble of the country rock. Near Millersville, where the stream flows for a mile or two parallel to an igneous dyke, the nuclei are diabase pebbles. But some specimens lack the stony nucleus having instead the limy layers concentric around a dark spot which proves upon close examination to be carbonaceous matter resembling nearly structureless peat. Probably this was originally a piece of wood or other vegetable tissue that carbonized after the concretionary laminæ had accumulated around it. This supposition has been verified in a number of cases by finding concretions with organic matter as nuclei (see Fig. 2).

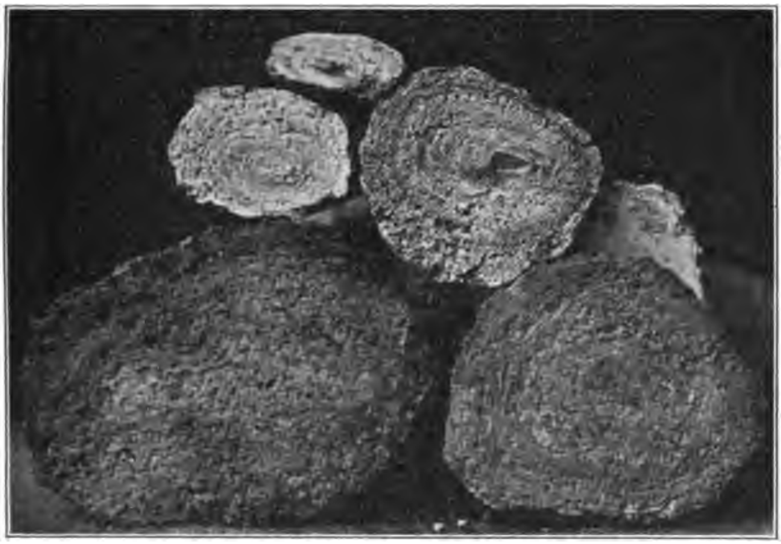

FIG. 2. Sections of a group of the concretions showing the laminae, concentric arrangement of the laminæ, the nucleus or nuclear point, and eccentric manner of growth. One-third natural size. The nucleus in the small upper specimen is a small water worn quartz pebble. The larger upper specimen shows where the nucleus was broken out when the section was made.

The concretions with stony nuclei may always be detected by their higher specific gravity.

Around the nucleus of a specimen is layer on layer of the limy matter each lamina from one eighth to one fourth of an inch in

thickness. The laminæ are not equally compact throughout their thickness, but are open and porous within and quite solid without. A polished section of any concretion exhibits many concentric ellipsoidal layers with the nucleus nearly always eccentric and the successive layers with a greater thickness on the one side and two ends than on the other side. The thickness of the successive laminæ in any one direction out from the nucleus is nearly uniform. In other words, along any radius the inner layers are just as thick as the outer ones. When found in place in the stream where the concretions have not been disturbed for a long time, the down side laminæ are invariably a little thicker than those on the upper side. This indicates that the greater growth is downward.

In appearance and structure, the concretions of the Little Conestoga are very similar to the "Lake Balls" from Lake Canandaigua, New York, so vividly described by Dr. Clarke, under the name of "Water Biscuits." They are also somewhat similar though much larger in size to the oölitic sands found forming in great numbers in the waters of Great Salt Lake by A. Rothpletz and traced by him to the agency of blue green algæ.

Where Found.—Upon recognizing the importance of a thorough study of the Algoid concretions, I began a systematic search in all parts of the Little Conestoga as well as in other streams of both Lancaster and York Counties, Pennsylvania. My search showed that these objects abound in all parts of the Little Conestoga nearly from source to mouth. But no other streams in this part of the statė have so far yielded any specimens. Those found in the sand bar in Lake Canandaigua near the mouth of Sucker Brook are probably also of stream origin, and I feel confident that a careful search in the brook would reveal at least some, if not many, of the concretions. Substances somewhat similar in composition occur in other lakes than Canandaigua though they do not have the concretionary form. Thus laminated reef-like accumulations of Algoid origin occur in Round Lake, New York, while marly or tufaceous deposits have accumulated for ages and are still forming in many lakes in Michigan, Wisconsin and Indiana. The tufa and thinolite described by Russell as forming in Pyramid Lake, Nevada,

are now regarded as of similar origin though differing much from the Little Conestoga concretions in both form and structure.

That concretions similar to those found in the Little Conestoga occur in other streams is evident from observations made in Center County, Pennsylvania, by Dr. Wieland, who, however, had not recognized them as of Algoid origin until I called his attention to the well known activity of some algæ in precipitating calcium carbonate. In a recent personal letter to me Dr. Wieland describes concretions that he found in 1888 in a stream near Lemont, Center County, Pa. He, however, says, "I just thought of them as very interesting objects from the viewpoint that they showed once more how abundant is CO_2 whether derived from plants or other sources. In short I knew too much and too little to make the least use of what I found."

Origin.—In 1854, W. Ketchell in the First Annual Report of the Geological Survey of New Jersey refers to *Chara* as active agents in the formation of fresh water marl. In 1864 Frederick Cohn found that a number of aquatic plants, especially *Chara* Mosses and Algæ, caused the deposition of travertine at the waterfalls of Tivoli. The deposition he attributed to the activity of the plants in absorbing carbon dioxide and so setting the lime carbonate free. That is, these low type plants consume carbon dioxide and exhale oxygen. When this is done in water containing calcium bicarbonate they deprive that salt of its second molecule of carbonic acid and the insoluble neutral carbonate of lime is precipitated.

W. S. Blatchley and G. H. Ashley in their report on the lakes of Indiana in 1900 also refer to the activity of plants in the precipitation of insoluble lime carbonate. But they also thought that the dissolved lime brought into the lakes by streams and deposited mechanically by evaporation was a more important agency than the plants.

In 1900 C. A. Davis discussed the origin of the marls of the lakes of Michigan and came essentially to the same conclusion as Cohn. He says:

"But in water containing amounts of salts, especially of the calcium bicarbonate, so small that they would not be precipitated if there were no free carbon dioxide present in the water at all, the precipitation may be consid-

ered a purely chemical problem, a solution of which may be looked for in the action upon the bicarbonates of the oxygen set free by the plants. Of these calcium bicarbonate is the most abundant, and the reaction upon it may be taken as typical and expressed by the folllowing chemical equation, $CaH_2(CO_3)_2 + O \rightarrow H_2O + CaCO_3 + CO_2 + O$, in which the calcium bicarbonate is converted into the normal carbonate by the oxygen liberated by the plants and both carbon dioxide and oxygen set free, the free oxygen possibly acting still further to precipitate more calcium monocarbonate, $CaCO_3$."

Dr. F. W. Clarke in "Data of Geochemistry" says:

> "That Dr. Davis' theoretical equation (given above) rests on no experimental basis."

In an article in *Science* dated December 14, 1914, J. Claude Jones, of the University of Nevada, says that the tufas of Salton Sea and of Pyramid Lake owe their origin to blue green algæ. He shows that wherever these plants are present in Pyramid Lake the gravels are cemented together and wherever the algæ are absent no trace of the tufas can be found.

Dr. Clarke ascribes the origin of the "Water Biscuits" of Lake Canandaigua to the same agency.

Miss Josephine Tilden in *Minnesota Algæ* (1910) says that *Gleocapsa calcarea* forms a calcareous crust (with other lime secreting forms) on boards where spring water from a trough drips down constantly.

Weed in his classic report (1889, U. S. G. S.) on the rock formations of the hot springs of the Yellowstone National Park shows that travertine as well as siliceous sinter are deposited through the aid of algæ.

Dr. B. M. Davis, of the University of Pennsylvania in a very interesting paper (*Science*, Vol. VI., July 30, 1897) describes the algæ and bacteria active in the formation of the travertine and siliceous sinter deposits in Yellowstone Park.

Dr. MacFarlane, of the University of Pennsylvania, in speaking of the activities of thermophilic algæ of hot spring and geyser regions, ascribes many rock formations throughout the earth's history as due to the work of fresh water algæ especially of the group Cyanophyceæ.

Evidences That the Active Agents of the Concretionary Formations in the Little Conestoga are Blue Green Algæ.

That the concretions described in the first part of this paper are the result of life processes of plants may be proved in a number of different ways. (1) The color of all growing specimens in the stream is the characteristic bluish green color of the Cyanophyceæ, while those exposed to rain and sunshine are grayish white. Careful microscopic examination also of such growing specimens reveals a varied thallophytic flora mainly of the Cyanophyceæ. Species of the genera *Gleocapsa, Gleotheca, Aphanocapsa, Nostoc, Oscillatoria* and *Rivularia* have been identified. Associated with these are several of the green algæ (Chlorophyceæ). Many species of the Diatomaceæ and Desmidaceæ which generally live in close association with blue green algæ have also been identified and have, no doubt, contributed the siliceous matter which is disseminated through the calcareous matrix. Among the diatoms, species of the genus *Navicula* both in free forms as well as stalked forms on algæ are quite prominent. The *Charas* are also occasionally present, contributing a small percentage of so-called marly material. Some bacteria have also been found in association with the other plants but the bacteria have probably had little to do with the calcareous deposition, but may contribute the iron which I find present in every concretion that I have analyzed.

(2) The arrangement and structure of the laminæ also favors the view that these concretionary accumulation are due to life processes. That periodic accretion alternates with a period of quiescence is shown plainly by the concentric laminations of nearly uniform thickness. The open porous nature of each lamina within and the more solid character without, like the concentric arrangement, is due without doubt to the seasonal conditions of the region. Since algæ are essentially thermophilic plants, each winter destroys many of them and stops the growth of most of the rest and thus at the beginning of the plant year (spring) few and widely scattered algæ at first produce slow and scattered accretion of the limy matter; later the plants become more abundant and by summer they are crowded over the surface of each mass. This distribution of the algæ seasonally would naturally have its effects upon the struc-

ture and arrangement of the limy matter giving a decided though rough coralline appearance to the inside portion and a more compact texture to the outer part. The theory just given has been confirmed by a study of the distribution of the algæ on the concretionary bodies through the seasons. The fact also that when the limy matter is dissolved out with acids, a mat of vegetable chains and cells remains nearly as large as the original concretion is also confirmatory. Even in the concretions which are centuries old as those in the forest covered deposit in Kendig's Woods the dead cells and chains of blue green algæ may be found.

(3) Lime secreting algæ are found in the Little Conestoga during the entire year but abound from May till December. They occur not only in the water but encrust many objects, in a few places forming small reef-like accumulations similar to those in Round Lake, New York.

(4) Quite an array of investigators, among whom we may mention Agassiz, Bigelow, Gardiner, Murray, Finckle, Vaughan, Walther, Drew, Matson, Dall, and Sanford, have studied at first hand the activities of algæ of the genera *Lithothamnion* and *Halimeda* and also some of the bacteria in various parts of the ocean and in many seas. All have come to the conclusion that many of the so-called coral reefs owe their existence partly and often largely to the activities of these lowly plants. The Bermudas, the Bahamas, the Laccadive and Maldive Archipelagoes, Funafuti, and extensive rock beds in the Floridian Peninsula have all originated through plant agency as much as through coral polyps. If this be true, it is not only possible but probable that fresh water blue green algæ throughout all the ages have caused and are still causing the precipitation of rock materials from minerals in solution in streams and fresh water lakes.

(5) Weed has proved that the concretions formed in geyser basins and known as Geyserites are formed by algæ which through life processes cause the precipitation of the siliceous matter held in solution in the hot water.

(6) The observation that the laminar accretion seems to proceed more rapidly on the under side of a concretion proves that the formations are not due to mechanical precipitation of lime carbonate

through evaporation or change of temperature. It does, however, suggest that the secretion or precipitation is chemical and dependent on a life process that produces conditions for chemical reaction where the plants or animals are most abundant.

(7) Conway MacMillan in *Minnesota Plant Life* says:

> "Some slime moulds have the power of incrusting their tiny fruit bodies with lime which they extract from their soil or from rain water which falls upon them. Such forms are often observed in Minnesota upon dead wood or fallen leaves, generally, in moist shady places in the deep forest. Some of the blue green algæ have the power of encrusting themselves with lime and in watering troughs and tanks there sometimes occurs a calcareous formation reminding one of the deposit in old tea-kettles. Such a crust is true limestone extracted from the water by the chemical activities of the algæ."

Upon a larger scale the blue green algæ have been conclusively shown by Weed to be important factors in travertine formation in the hot springs and geysers of Yellowstone National Park.

Dr. MacFarlane without knowing of my discovery in the Little Conestoga Creek has expressed the opinion that these apparently insignificant plants have throughout all the ages played and are still playing in all waters an important part in the formation of limestones and dolomites.

(8) The fact that many more or less ancient rocks have been demonstrated to be of algoid origin by various scientists and are similar to the Little Conestoga concretions in their concretionary or laminated structures or both is favorable to the view that algæ are just as important agencies in rock formations in the present geological epoch as in the past. The similarity of *Cryptozoön proliferum,* Ozarkian oölitic formations, *Newlandia frondosa, Camasia spongiosa, Collenia compacta, Collenia undosa* and other structural forms in rock formations to the work of recent algæ in hot spring and geyser regions has been vividly shown by Walcott, Wieland, B. M. Davis and others. Some, at least, of the above-named formations can be strikingly duplicated in their structural peculiarities by the Little Conestoga concretions and reef-like masses of Round Lake,—the Potsdam-Hoyt formation of New York state being especially like what would result were infiltrating waters, cementation, and other solidifying agents or processes to act for a long time upon the great mass of flood deposited concretions of the Little Conestoga in Kendig's Woods.

Mineral Content of the Little Conestoga Waters.

One would infer from the number of concretions growing in the Little Conestoga and also from the thickness of each lamina in a concretion that the mineral content of this stream's waters is high. I have verified this by determining the salinity of the stream under varying conditions. The salinity in a wet month was 330 parts in a million, while in a dry month this rose to 365 parts in a million. Streams in which I have found no trace of concretionary structures have a much lower salinity, the Big Conestoga Creek for example having a salinity of 190, the Pequea Creek 195, and the Susquehanna, in March, above the mouth of the Pequea and below the mouth of the Big Conestoga, about 200 parts in a million. The various springs flowing into the Little Conestoga have an average salinity nearly as high as that of the Little Conestoga itself.

The basin of the Little Conestoga is underlain with much more soluble limestone than any of the other streams so far investigated. This accounts for the high salinity of its waters and also for the distribution of the concretions so far as we know that distribution. Further search and study will certainly reveal that many streams of the world contain concretionary structures and determine the conditions of their distribution and formation. I trust the beginning I have made in the investigation of stream concretions will lead to a wide and thorough study of this interesting and important biological as well as geological problem.

The various facts tabulated on page 257 and correlated with the fact that the blue green algæ are about equally abundant in the various streams mentioned in the table would seem to indicate that deposition of $CaH_2(CO_3)_2$ is always going on in all the streams during the growing season, but that when the salinity is low solution by the stream waters balances deposition and no concretions are formed. When, however, the salinity is high, solution can not take place and laminated structures due to seasonal or other changes are formed either in concretionary form or more rarely as reefs. This is put forward as a working hypothesis, many more observations and analyses are needed however before the various problems connected with these formations can be fully solved.

TABLE SHOWING RELATION BETWEEN THE SALINITY OF STREAMS AND THE PRESENCE OF CALCIUM CARBONATE CONCRETIONS.

Stream or Spring.	Month.	Salinity, Parts in One Million.	Nature of Salinity (Chiefly).	Concretions Present in Stream.
1. Little Conestoga	Feb. 5	330	$CaH_2(CO_3)_2$	Abundant
2. Little Conestoga	March	300	"	"
3. Little Conestoga	April	365	"	"
4. Branch Run, tributary to Little Conestoga	April	91	"	None
5. Big Conestoga	Feb.	152	"	None
6. Big Conestoga	March	100	"	None
7. Big Conestoga	April	150	"	None but many gasteropods
8. Duing's Run, tributary to Big Conestoga	April	195	"	None
9. Pequea Creek	April	195	"	None
10. Donegal Run	April	404	"	Abundant
11. Nissley's Dam in Donegal Run, further upstream than 10	April	400	"	Many but small
12. Donegal Run near source	April	230	"	None
13. Bellaire Branch of Donegal Run	April	208	"	None except near mouth
14. Little Chickies	April	170	"	None
15. Big Chickies	April	171	"	None
16. Big Chickies farther upstream	April	174	"	None

FURTHER NOTES ON CONCRETIONARY FORMATIONS IN STREAMS.

Since writing the above I have been fortunate enough to find a new locality for concretions. Knowing that Donegal Township, Lancaster County, comprised a notably large area of Cambro-Ordovician limestones, I judged that its streams would be favorable to the growth of calcareous concretions through the agency of blue green algæ. Search on April 25, in Donegal Creek, revealed these objects in greater abundance than in the Little Conestoga. One meadow of fully 12 acres bordering the stream about one mile northeast of Marietta was found to be underlain with a bed of concretions not less than a foot in average thickness throughout its entire extent. And this was under a soil cover of more than a foot in depth that had, apparently, resulted from the weathering and disintegration of the same objects. The great flood deposits of concretions in this and neighboring meadows were paralleled by large quantities in the stream itself, fully one fifth of the stones in some

places in the stream channel being of concretionary origin as shown by their shape, laminated structure, and composition.

The finding of the new locality is of great interest. It shows that a careful, intelligent, and systematic search will reveal these formations in many other regions of the world wherever the proper conditions exist for calcareous and siliceous precipitation through the life processes of plants.

But the geological significance of the great meadow deposits also needs emphasis. The large accumulation in the Donegal Township Meadow represents a comparatively long period and this indicates a considerable antiquity of the plants which form the concretions. Then too, such a bed of closely packed concretions is highly suggestive of the manner in which some ancient rock beds originated. For were such accumulations of concretions as those in the Donegal Meadows to be consolidated by the action of infiltrating waters, pressure, heat and chemical change solid rock beds would result nodular in appearance and concretionary in structure hardly distinguishable from the Hoyt Potsdam beds of New York.

Species of the following genera of the Cyanophyceæ are found associated with the calcareous concretions occurring in Donegal Creek, Lancaster County, Pa..: *Glœocapsa, Microcystis, Cœlosphœrium, Aphanocapsa, Oscillatoria, Rivularia, Nostoc, Chroococcus.* There are also species of *Protococcus,* many species of Diatoms, several species of Desmids, various species of the Chlorophyceæ, several species of Phæophyceæ, and species of Rhodophyceæ.

THE CONDITIONS OF BLACK SHALE DEPOSITION AS ILLUSTRATED BY THE KUPFERSCHIEFER AND LIAS OF GERMANY.

By CHARLES SCHUCHERT.

(*Read May 7, 1915.*)

Stratigraphers do not agree as to the conditions under which the black bituminous shales so often met with in American Paleozoic marine deposits were laid down. Among the more striking of such formations may be mentioned the Quebec, Martinsburg, Collingwood, Utica, Maquoketa, Genesee-Portage, Ohio, Chattanooga, and Caney, formations ranging from the Ordovician to the Pennsylvanian. To aid in the interpretation of such black shales, the writer presents herewith the main results set forth by Professor J. F. Pompeckj, of the University of Tübingen, in a publication that will not be of wide distribution in America.[1] The following is a decided condensation and in part a free translation of his exhaustive paper, which is replete with bibliographic references.

The Kupferschiefer of Germany are of Middle Permian age, and occur near the base of the Zechstein, the time of marine invasion over the previous continental series known as the Rotliegende. In general, the bituminous dark shales occur above the basal Zechstein conglomerate and below the Zechstein dolomite, and occupy an area of at least 60,000 square kilometers in middle and western North Germany. The average thickness of the copper shales over wide areas is about 30 inches, but varies from nothing to a maximum and exceptional local thickness of 35 feet. However, in many places there are no black shales and then the equivalent deposits, or the basal strata of the invading Zechstein, may be conglomerates, sands, shaly limestones, or dolomites. In other words, the black bituminous shales do not prevail everywhere, and the same is true of the metal sulphides.

[1] "Das Meer des Kupferschiefers," Branca-Festschrift, 1914, pp. 444–494.

The copper-bearing shales usually succeed the basal conglomerates or sands and finally become gradually more and more calcareous, passing upward into the normal Zechstein dolomite of wider distribution. The latter has an abundant though monotonous fauna indicative of peculiar marine conditions and not much like that of the Tethyian mediterranean to the south, which is of normal sea environment. The paleogeography indicates an inland sea, bounded by continuous land, in the north by Fennoskandia across to England, thence south to France and Belgium, and east over South Germany to Bohemia. In the east only were there limited connections with the Russian and Arctic Zechstein sea. The previous orogenic movements resulting in the Paleozoic Alps of central Europe had been greatly reduced, so that the streams flowing into this Permian sea were sluggish and delivered only the finest of muds and solution materials, while those flowing out of regions of igneous rocks were charged in addition with copper, zinc, and silver.

The Kupferschiefer are fissile, tough, dark to black, highly bituminous (6 to 20 per cent.), clay shales with considerable calcareous material that increases in amount upward (locally to 45 per cent.). Copper sulphides variable in quantity and nature are present, and because of this ore the strata have been mined in Germany for seven hundred years. Under the microscope the shale is seen to be made up of finest clay substance colored yellow-brown to black by bitumen. Throughout the clay there are scattered, layered, or aggregated in the form of thinnest lenses varying amounts of tiny crystals of calcite and needle-like splinters of quartz. Black coaly dust is also more or less abundant and especially among the clay particles.

The flora and fauna of the Kupferschiefer are small and at best do not include more than 1 land stegocephalian, 2 land reptiles, 17 fishes (5 selachians, 1 crossopterygian, the rest ganoids) with structures indicating forms that lived on or near the bottom of the waters, 1 nautilid, 1 gastropod, 1 scaphopod, 10 bivalves, 3 bryozoa (Fenestellidæ), 5 brachiopods, 1 problematic starfish, and 11 species of land plants. This assemblage is brought together from many localities and the species of fishes are usually based on single specimens, indicating that the biota is not a natural assemblage, but is

made up of land and marine forms plus fishes, most of which appear to be of fresh water habitat. The only common fossils are the ganoid *Palæoniscus freieslebeni, Lingula credneri, "Asterias" bituminosa* (problematic), and the small bivalves *Nucula beyrichi* and *Bakewellia antiqua* (sometimes in colonies). In other words, the life consists of land-derived forms (3 vertebrates and 11 plants), fishes (5 probably marine and certainly bottom-feeding, and 12 apparently of river origin), and 22 marine invertebrates all but one of which are forms living on the bottom of the sea, attached to it or to floating objects. While the invertebrates indicate plainly that the copper shales were laid down in the sea, the great scarcity of fossils shows that the forms recovered are in the main not in their normal habitat. It appears that only 3 species (the invertebrates cited) were able to adapt themselves to the peculiar conditions of the copper-depositing seas. Not a single scavenging animal is found, and the fact that so many fishes (17 species) were present as food (*Palæoniscus freieslebeni* is often more or less decomposed by sulphur bacteria) indicates that the bottom had no scavengers and that it was not a favorable place for any kind of life.

Pompeckj has carefully studied the fishes, and as all or most of them are carnivorous (some are shell-feeders) the question is raised: On what could they have fed, since there was so little bottom life? He admits that there may have been present an abundance of soft-bodied and shell-less invertebrates on which they preyed, but finally concludes that it is much more correct to assume that most of the fishes (at least 12 species) were drifted into the sea from the rivers. If they also lived in the sea, it must have been in the oxygenated surface waters or the shallow shore regions. On the other hand, the invertebrates present indicate that nearly all of them fed on microscopic plants and animals (no ostracods are present, however) and it is perfectly natural to assume that the surface and sun-lit waters abounded in a varied plankton, as do the seas and oceans of today. It was this world of minute forms, the plankton, that rained into the depths, feeding the sparse brachiopod and molluscan life and the common sulphur bacteria.

Moreover, it is the abundant surface plankton that in all probability has furnished most of the bituminous matter, assisted further

by the land-derived fishes, while the coaly substance has resulted from the land plants. Along the shores, in the oxygenated waters, there probably also was an abundance of sea-weeds and among them doubtless lived most of the invertebrates preserved in the Kupferschiefer. The marine plants are broken up by the storms, and the water currents plus the undertow generated by the waves and tides drag this material into deeper waters, where it is slowly rotted and further altered by the sulphur bacteria. There results a foul bottom, free of oxygen, and reeking with carbonic acid and sulphuretted hydrogen gas. The chemical reactions set up here (diagenesis) result in the deposition of the metal sulphides (copper, zinc, silver) and the bituminous alteration products.

The paleogeography, as stated above, indicates an inland and almost land-locked sea. Into such a basin the currents generated in the oceanic areas can at best enter but little, and that such did not enter in any marked degree is seen in the almost complete absence of floating and swimming invertebrates. As for the general physical conditions, Walther thinks of stagnant waters, with marine swamps; Kayser of quiet bays of inland seas with foul bottoms; and Dosz of stagnant places like the present bays around the island of Oesel, where the bottoms are rich in iron sulphide deposits, the healing or medicinal muds. Pompeckj, however, finds more or less valid objections to all of these suggestions, and thinks the best present analogue to be the Black Sea, whose physical and organic conditions are now well understood through the work of Andrussow and Lebedintzew. In other words, the Kupferschiefer sea is "a fossil Black Sea" in nearly all its characteristics except depth.

With regard to the conditions of the Black Sea, it is an inland, relic sea, which was once a part of the Tethyian mediterranean. Its greatest length is about 715 miles and its maximum width 380 miles (making its area 170,000 square miles), and it attains 7,360 feet in depth. Flowing into it are many rivers, among the largest of which are the Danube, the Dnieper, and the Don. Its only outlet of surface water is through the strait and over the barrier of the Bosporus into the Sea of Marmora and thence through the strait of Dardanelles into the Ægean Sea and the Mediterranean. A compensating but smaller inflow of salt water (salinity 3 per cent.)

occurs at greater depths. The shores are high and bold on the northeast, east, and southwest, and flat on the north and northwest.

Andrussow[2] has described the physical and bionomic conditions of the Black Sea as follows: Beyond the shallow marginal waters of 600 feet depth there is no bottom-living life (benthos), while in the surficial fresher waters down to about 750 feet there is a more or less great abundance of floating, usually microscopic, open-sea forms (plankton) and the larger, free-swimming life (nekton), collectively also spoken of as the pelagic biota. This upper layer of freshened water and its peculiar life conditions are brought about by the enclosed nature of the deep basin, the inflowing of immense quantities of less dense fresh water that remains at the surface or is there evaporated, and a deep-seated, partially compensating current of salt water from the Sea of Marmora through the strait of Bosporus. It is estimated that it takes about 1,700 years to renew the entire salt-water content of the Black Sea.

Because of these differences between the lighter surface and the heavier bottom salt waters, there is no vertical streaming nor convection currents beyond 750 feet of depth, and therefore no replenishing of the deeper marine waters with the oxygen that is so necessary for the maintenance of benthonic life. At the depth of 600 feet, hydrogen sulphide begins to form (33 c.c. in 100 liters of water) and increases rapidly with the depth to 3,000 feet (570 c.c.) and then more slowly to the bottom of the sea. The formation of the H_2S is in the main due to the sulphur bacteria. Hand in hand with the increase of the H_2S goes the decrease of the sulphates in the sea water and the precipitation of the carbonates and iron sulphides.

That the aëration of marine waters, and also the generation of sulphuretted hydrogen may be better understood, a digression into the studies of oceanographers becomes necessary. The atmospheric gases, oxygen and nitrogen, are absorbed at the sea surface more abundantly in cold than in warm latitudes, and the quantity absorbed is again variable under varying pressures and chemical conditions of the water. This complex subject, too long to state here, may be

[2] "La Mer Noire," Guides des Excursions, VII[e] Cong. Géol. Internat., St. Pétersbourg, 1897, Art. XXIX.

studied in Krümmel's "Handbuch der Ozeanographie," I., 1907, pages 292–317. Furthermore, the amount of oxygen is increased when there is an abundance of assimilating plants, as in the areas of the sea-weeds and diatoms. The gases are then distributed by the general water circulation to most parts of the oceans and even into the greatest depths. In general, there is an abundance of oxygen down to 350 feet, but in the tropics it is wanting in the greater depths of the shelf seas. The oxygen is consumed by the animals and by various hydro-chemical processes and consequently diminishes in quantity as it is carried down from the surface and over the bottom, but the quantity of nitrogen remains constant. Sir John Murray states further that in the streaming open ocean of today there is usually an abundance of oxygen even at the greatest depth, due to the sinking heavier and colder polar waters, but this is not the case in partially enclosed seas which are more or less cut off by barriers and where the water is said to be "stale," and in the deeper layers of which vertical circulation is restricted.

Similar stagnant conditions "prevail in several Norwegian 'threshold fjords,' or on a smaller scale in the oyster-'polls.' In such places the bottom is thickly covered with organic matter; a slimy black mud is formed, swarming with bacteria that produce sulphuretted hydrogen, which spreads through the water, combining with the oxygen to form various sulphates. This causes the oxygen to decrease and finally to disappear altogether, when the sulphuretted hydrogen begins to appear free in solution. It gradually spreads upwards, until the water is devoid of oxygen and contains free sulphuretted hydrogen, at a depth of only 100 fathoms in the Black Sea, and in the oyster-basins in autumn often at merely a couple of meters below the surface. In summer the 'bottom-water' of the oyster-'polls' lies stagnant, but in the course of the autumn and winter it is generally renewed by the supply of comparatively heavy water from without; then the sulphuretted hydrogen disappears and the oxygen returns, producing thus an annual change in the gaseous conditions of the deeper parts of the oyster-'polls.' In autumn the state of things may become critical for the oysters, which are suspended in baskets at a depth of 1½–2 meters; it hap-

pens occasionally that the animals all die at this time by suffocation through want of oxygen or by sulphur poisoning."[3]

Johnstone[4] states that "In some parts of the sea, as for instance in the 'dead grounds' of the [very shallow] Bay of Kiel, in some parts of the Black Sea, and perhaps in parts of some of the Norwegian fjords, where the water circulation is defective, and where there may be a deficiency of oxygen, very remarkable bacteria are to be found. These are the sulphur bacteria, the occurrence of which is not, however, confined to these habitats. In the places I have mentioned sulphuretted hydrogen is evolved from the decomposition of dead organic matter, and this sulphuretted hydrogen, to us a vilely smelling and poisonous gas, is utilized as food substance by the bacteria. Such a microbe as Beggiatoa takes in the SH_2 and oxidizes it so that the sulphur is deposited in the cells of the bacterial colony, and the hydrogen appears as water. This is the form of assimilation of the organisms. Then some of the sulphur thus resulting from the decomposition of the SH_2 is oxidized to sulphuric acid. This is the form of respiration of the organism. It requires some source of nitrogen for the formation of its living proteid and this it obtains from the minute quantities of nitrates and nitrites which exist in solution in the water in which it lives. But it requires very little nitrogen compound, for whereas a higher animal may require to oxidize some of the living nitrogenous tissue of its own body in order to obtain its energy, the sulphur bacterium oxidizes the sulphur stored in its cells as the result of the assimilation of the SH_2. Thus the proteid part of the cell is protected from waste, and the minimal quantity of nitrogenous food-stuff suffices."

Krümmel states that the troughs of the Baltic Sea renew their deeper water irregularly and periodically. In the Rügen and Bornholm troughs (about 325 feet deep) the renewal takes place at least once and more rarely twice each year, in the Danzig trough (about 325 feet deep) nearly every year, and in the deeps off Gotland and in the Gulf of Bothnia usually only after many years. All these troughs get the new deeper water from the western Belt Sea and more rarely also from the Öresund east of Denmark.

[3] Sir John Murray, "The Depths of the Ocean," 1912, pp. 257–258.

[4] "Conditions of Life in the Sea," 1909, p. 264.

To return to the Black Sea and its sediments, these are of three categories: (1) from the shore to about 120 feet occur the accumulations of sandy detritals; (2) from 120 to 600 feet is found a gray-blue sticky ooze, often replete with small fragile shells, mainly of Modiola; and (3) in the greater depths the bottom is covered with (*a*) a tough, sticky, black ooze, with much precipitation of iron sulphide, an abundance of diatoms and fragments of the youngest stages of bivalves, all of which organisms are from the plankton, and (*b*) the dark blue ooze poor in iron sulphide and richer in the finest-grained $CaCO_3$, which in places forms thin banks, and an abundance of pelagic diatoms. Zones 1 and 2 alone have benthonic organisms, with the greatest abundance between 210 and 600 feet; the latter is the zone of *Modiola phaseolina* and a great variety of bivalves and gastropods (68 species occur in the shallower waters).

The Kupferschiefer sea, like the Black Sea, had bottom waters with about the average normal salt content, as proved by the typical Zechstein invertebrates. However, because of the lack of oxygen and the high content of sulphuretted hydrogen and CO_2 an abundant bottom life was impossible. That the top water of the Kupferschiefer sea was also fresh is proved by the wide distribution of the freshwater fishes in the sediments, the widely uniform spreading of the thin zone of shale, and the presence of land plants and land vertebrates. If all the water had been salty, the fine muds should have been laid down in a narrow zone bordering the margin of the sea, and this is not the case in the Kupferschiefer sea. The slow decomposition of the organic remains (mainly the plankton) and the lack of oxygen in the depths led further to the formation of the bituminous content (from 6 to 20 per cent.).

As the Black Sea goes down to 7,360 feet, the question must be asked: What was the depth of the Kupferschiefer sea? A positive and exact answer can not be given, but the small thickness of the shale over wide areas, combined with its intimate and variously modified connection below with the Zechstein conglomerate and above with the Zechstein dolomite, and its shallow-water life, show that "it is a deposit of the shallowest and shallower seas." To the reviewer, the depth seems to be well within that assigned the

continental shelf seas, *i. e.,* less than 600 feet. The freshwater covering Pompeckj thinks was thin.

Just as in the Black Sea the marginal fresh waters are depositing sands and other littoral sediments that are free of bitumen, so in the Kupferschiefer sea there is some evidence of marginal sands, sandy and clayey limestones, and regions free of metal sulphides.

Later, the black sea of Permian time gradually changed, first locally and finally everywhere, into the limestone-dolomite or Zechstein sea, still, however, an inland sea but devoid of muds and bituminous materials. In the shallow regions nearer the shores arose reefs of bryozoa, but at best the Zechstein sea, even when in widest connection with the ocean, had a small and monotonous fauna.

In an earlier paper Pompeckj[5] discusses a similar deposit, the zone of *Posidonomya bronni* of the Upper Lias of Germany. It seems desirable to cite also some of the details given in this paper, because they are somewhat different from those concerning the Permian. The deposits are fissile, calcareous, bituminous, dark shales rich in iron pyrite. Locally there are also horizons of sandstone, barren of life, and layers of stinking limestone. These deposits are found in northwestern Germany (about 40 feet thick) and France.

In Germany (Swabia and Franconia) the fossils consist of diatoms and coccoliths, horn sponges, very rarely a sea-urchin, crinids (sometimes with stalks over 50 feet long), a few forms of brachiopods, about 18 species of bivalves (of which the only common one is *Posidonomya bronni,* but this very thin-shelled form is at times exceedingly abundant; also *Pseudomonotis substriata, Inoceramus dubius, Pecten contrarius*), and rarely a gastropod or crustacean (Eryon). Besides the common bivalves mentioned, there are many ammonids, belemnids, sepias, fishes (selachians, many ganoids, teleosts), ichthyosaurs, plesiosaurs, and crocodiles. With the marine forms are associated drifted land plants (cycads, and often a great abundance of conifer logs, now carbonized), beetles, and dragons of the air (pterosaurs).

[5] "Die Jura-Ablagerungen zwischen Regensburg u. Regenstauf," Geognos. Jahresheften 1901, XIV. Jahrg., pp. 178–186.

It is apparent from the above that the common fossils are here again those of the nekton (saurians, fishes with most of the ganoids probably of freshwater habitat, belemnids, sepias) and drifted land plants. Of the benthos, only a few species of bivalves are common, and, while the ammonids are also bottom-dwellers and occur commonly as fossils, their empty shells were probably drifted into this black sea. The crinids were also drifted in, for the only specimens found attached are on conifer wood, hanging head downward; otherwise roots of these pentacrinids do not occur.

In general it may be said that the Liassic deposits and the habitat of the fossils of the time of *Posidonomya bronni* agree best with those of the present Black Sea. Since this is true, it follows that the physical conditions of the *bronni* sea must have been very much like those of the Black Sea, *i. e.,* it was a Liassic Black Sea into which drained rivers, causing the surface waters to be more or less freshened, and bringing land plants, logs, and ganoid fishes. However, there are also marked differences, chief among which is the far less amount of decomposition of the soft parts of ichthyosaurians and sepias, of which fleshy parts are often preserved, a condition that never occurs in the Kupferschiefer. Finally, the abundance of the Liassic bivalves points to the shallow waters of the Modiola ooze of the Black Sea, and therefore to depths of less than 600 feet.

It seems to the reviewer that the present Black Sea, with its great depth and widespread foul conditions, is an exceptional example, and that in all of its features it may have no fossil analogue. The Kupferschiefer and *P. bronni* seas along with the American Ohio sea of Upper Devonian time and the Chattanooga sea of the Mississippian period appear to agree with the essential conditions of the Black Sea, except as to depth. All of the fossil Black Seas appear not to have been deeper than 600 feet.

Foul bottoms are clearly due to a lack of water circulation, either because there is no wide connection with the oceanic areas or because there are inadequate vertical or convection currents. The latter conditions may have been more abundantly attained in warm climates than in cool ones, due to the fact that the heavier colder waters sink to the bottoms and so oxygenate them. In this the present is the exceptional condition when compared with most of

geologic time. In such stagnant areas, be they small or large in area, or shallow or deep, the oxygen is soon consumed by the organisms of the benthos and the depths become stale and lifeless. As the sulphur bacteria are ever present, but thrive best in the stale bottoms, they soon take the ascendancy there and fill the waters with an ever greater quantity of sulphuretted hydrogen, provided they are furnished with the dead organisms on which to feed and thus to increase in number. On the other hand, the sun-lit, aërated surface waters are the realm of the green and assimilating micro-plants, the free algæ, which convert the inorganic carbon dioxide into their organic bodies, and these upon their death rain into the deeps to form the essential food of the bacteria of the foul bottoms.

That depth of water is not the first essential for the production of foul bottoms has been shown by the examples cited (almost from the surface down), but it does seem that large areas must have depths greater than 300 feet, for otherwise the high waves generated by the storms would set up a vertical circulation and so at least periodically replenish the oxygen and take away the foul gases of these depths. Therefore it would seem that Black Seas of large size should be deep (300 feet or more) and land-locked basins whose oceanic connections are more or less cut off by submerged barriers. Smaller areas are the elongated troughs and rounded holes below the general level of the sea floors, while the smallest and shallowest areas are the bays that are more or less separated from the seas by closely approaching headlands, banks, and bars, or the marine swamps that are filled with eel-grass, mangroves, and other modified land plants.

ON THE RATE OF EVAPORATION OF ETHER FROM OILS AND ITS APPLICATION IN OIL-ETHER COLONIC ANESTHESIA.

By CHAS. BASKERVILLE, Ph.D., F.C.S.

(*Read April 23, 1915.*)

It is conceded that the anesthetic agent must get into the blood for distribution and for eventual elimination, whatever theory of general or central anesthesia one may support. The anesthetic agent has normally been introduced into the blood by inhalation or intravenously. It is normally eliminated mainly via the lungs.

The intestinal mucous membrane of vertebrates is well known as an efficient transmitter of gases to and from the blood. Pirogoff[1] appears to have been the first to mention the administration of ether by this route. Liquid ether was used until Magendie gave warning as to the danger of its use and ether vapor was substituted. During the same year Roux,[2] y'Yhedo[3] and Duprey[4] employed liquid ether or aqueous mixtures to induce complete anesthesia. Although Pirogoff's enthusiasm prompted him to predict the supplanting of the inhalation procedure by the rectal method, references to it disappeared from the literature until 1884. Then Mollière[5] revived interest in the method by using a hand bellows for forcing the ether vapor into the intestine. Variations in the technique were introduced during the same year, but the experiences of Yversen, Harter, Bull,[6] Weir,[7] Wancher[8] and Post[9] showed more or less diarrhœa

[1] "Recherches pratique et physiologiques sur l'etherization," St. Petersburg, 1847.

[2] *J. d. l'academie d. Sciences*, 1847, 18.

[3] *Gazette med. d. Paris*, 1847.

[4] *Academie royale de medicine*, March 16, 1847.

[5] *Lyon Medical*, 45, 1884.

[6] *N. Y. Med. J.*, March 3, 1884.

[7] *Med. Rec.*, 1884.

[8] Cong. internat. d. Sciences med., 1884.

[9] *Boston Med. and Surg. J.*, 1884.

and melena as after-effects. These after-effects, which one case of death directly attributable to the procedure, caused the method to again fail in securing serious recognition until 1903 when Cunningham[10] employed air as a vehicle for sweeping the ether vapor into the colon. In 1909 Leuguen, Money and Verliac[11] used oxygen as the vehicle for the ether vapor. Buxton[12] in his splendid book on "Anesthesia" says that he found the procedure most satisfactory for certain operations, for example, those having to do with the mouth, nose, etc., but he remarks "Deaths have occurred." Sutton's[13] introduction of a return flow tube for these gases introduced and unabsorbed constituted a distinct advance in anesthesia by colonic absorption.

In an effort to avoid certain well-known difficulties in intravenous anesthesia, Gwathmey experimented with mixtures of normal saline solution and ether per rectum. The concentration of ether in the aqueous solution was so small that excessive volumes of liquid were needed, and furthermore the ether parted from the solution so very rapidly that experimentation along those lines was abandoned. Gwathmey then applied a solution of ether in olive oil. As oil and ether make perfect solutions in all mixtures, it was his hope to reduce the total bulk of the fluid introduced into the colon by using a stronger solution of ether in oil than is possible with any known aqueous mixture. As oils are lubricants, it was also hoped to avoid the irritation of the mucous membrane previously noted. The ether may always be separated from the oil by warming, but unless the temperature of the mixture is suddenly raised to an excessively high point, the ether passes off deliberately. It was thought that the evaporation of the ether would induce some cooling of the mixture with a consequent checking of the evaporation and its absorption. These premises coupled with slow absorption by the colon in comparison with the rapid elimination by the lungs would auto-

[10] Cunningham and Leahy, *Boston Med. and Surg. J.*, April 30, 1905; *Vide* also Dumont, *Correspond. Bl. f. Schweitzer Aerzte*, 1903; 1904; 1908; Krugeline, *Wiener klin. Woch.*, Dec., 1904.

[11] *Compt. rend. Soc. Biol.*, June, 1909.

[12] "Anesthesia," London, 1907.

[13] For full account of technique and literature, see "Anesthesia," by Gwathmey and Baskerville, Appleton, New York, pp. 431–457, 1914.

matically regulate any anesthesia that might be induced in this manner. As a result, Gwathmey presented a paper before the seventeenth International Medical Congress in London in 1913 on the work with animals done by himself and Wallace.

At the request of my co-laborer, Gwathmey, I undertook an investigation on the rate of evaporation of ether from oils to secure the following information that might be of service to him in his further application of his ideas with human subjects:

1. A comparison of the rate of evaporation of ether from different mixtures of ether and the same oil.

2. A comparison of the rate of evaporation of ether from the same per cent. mixtures of different oils and ether.

3. The influence of surface on the rate of evaporation was determined.

As the result of much preliminary experimentation, the following mode of procedure was settled upon. Large glass tubes were calibrated to 1 c.c. from 20 c.c. to 105 c.c. The mixtures of 25, 50 and 75 per cent. of oil and ether were carefully placed in the tubes. The tubes were weighted with lead and placed in a thermostat, whose temperature was so regulated as not to vary more than $\pm 0.03°$ C. from 37° C., the same being controlled by a toluene + mercury temperature regulator. All connections (gas, water, etc.) were made with lead pipe for safe use over night, as occasion arose. The water in the bath was stirred by a system of paddles and shaft operated through belt and pulleys by a small hot air engine. The tubes were immersed in the bath to within 2 cm. of the tops. During the first five minutes two readings were made in each case to get the highest point to which the volumes expanded upon heating up to 37° C. After that readings were made every five minutes for two or three hours.

Since the evaporation of any liquid depends upon the partial pressure of that liquid at its surface, the higher the glass wall above the surface of the oil-ether mixture, the heavier the column of ether vapor resting on the surface of the mixture, the slower will be the evaporation, consequently the different oil mixtures with the different percentages of ether were experimented with in the same tube filled to the same height in each experiment.

In the experiments to determine the influence extent of surface played upon the rate of evaporation, the same precautions were taken as to height of walls of the containing vessels. In the largest areas worked with, this involved using as much as 600 c.c. of the mixture. As the 75 per cent. mixture had been found most satisfactory clinically, this was determined with that mixture only.

The ether used was that prepared under my supervision and was 97 per cent. absolute with 3 per cent. absolute alcohol, being free from acids, aldehydes, and water.

The oils used were of three types, vegetable, animal and mineral, being respectively, olive, cotton seed, corn, peanut and soya-bean; cod-liver and lanolin (anhydrous); and Russian mineral oil. All the vegetable oils, except olive, were refined by a process devised by

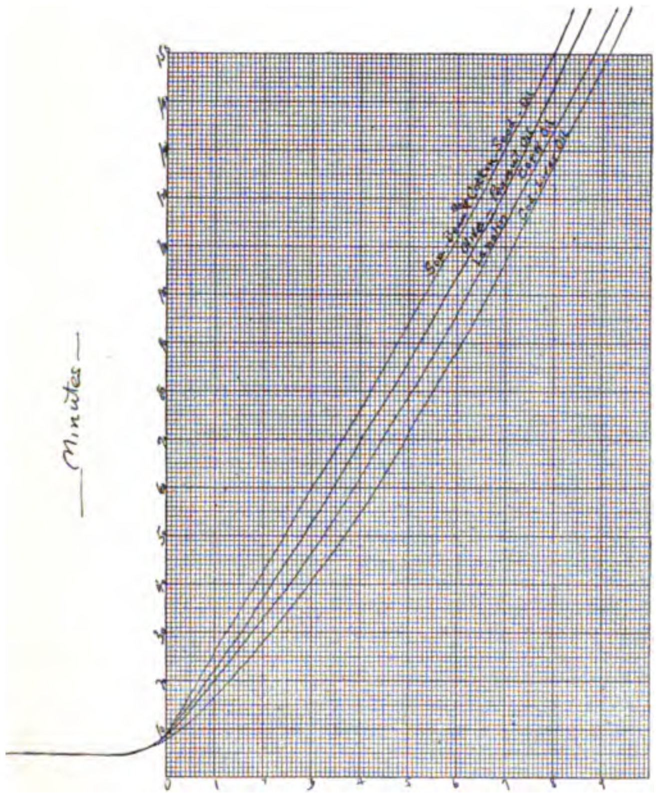

CHART I.

the author[14] and were neutral. The other oils were purchased in the open market.

The experimental work was carried out by Mr. Hyman Storch, under my direction.

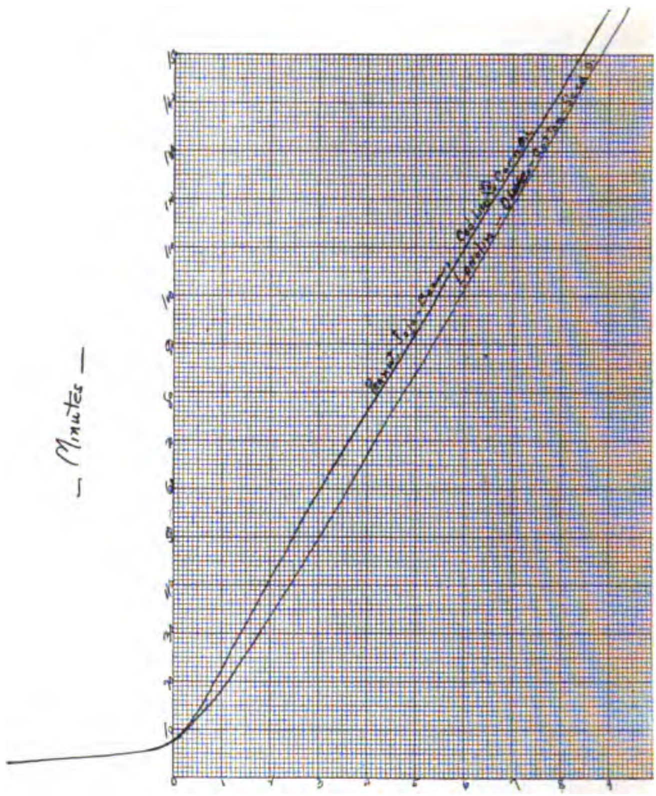

CHART II.

The data obtained for the 25, 50 and 75 per cent. mixtures vegetable and animal oils are shown graphically in Charts I., II. and III. In the curves the abscissæ show the percentage of ether evaporated (based on volume measurements) and the ordinates time of the evaporation.

Chart IV. (selected at random from charts made for each oil) shows the difference in rate of evaporation 25, 50 and 75 per cent. mixtures with one oil.

[14] "Refining Oils," *Oil, Paint and Drug Reporter*, May, 1915.

Chart V. shows the effect of increased surface on the rate of evaporation. One oil only was selected to show the principle, which is: the rate of evaporation bears a direct ratio to the surface exposed.

These experiments were made in glass, hence they do not disclose all the factors in the conduct of such mixtures in contact with the walls of the colon, for there the principles of osmosis and diffusion are involved. But these observations demonstrated several striking facts:

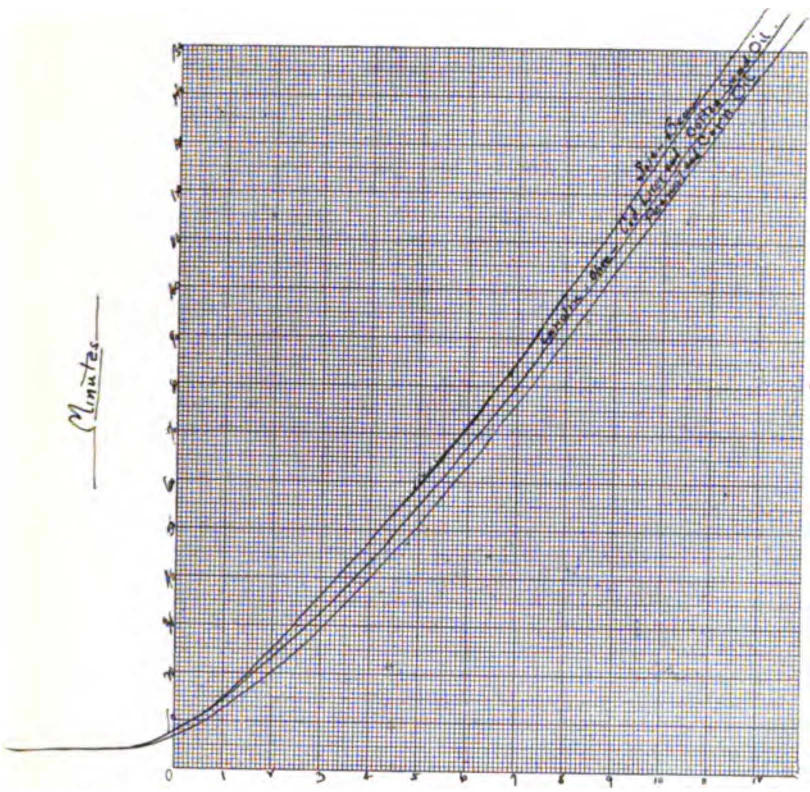

CHART III.

1. While ether boils at 34.6° C., it does not escape violently from an oil-ether mixture, as from an aqueous mixture when the mixture is heated higher, namely, to the body temperature of 37° C.

2. The *rate* of separation of ether from the oil quickly acquires a definite and fairly fixed speed.

The significance of this conduct cannot fail to be of great importance, for by this means the proper content of ether may be main-

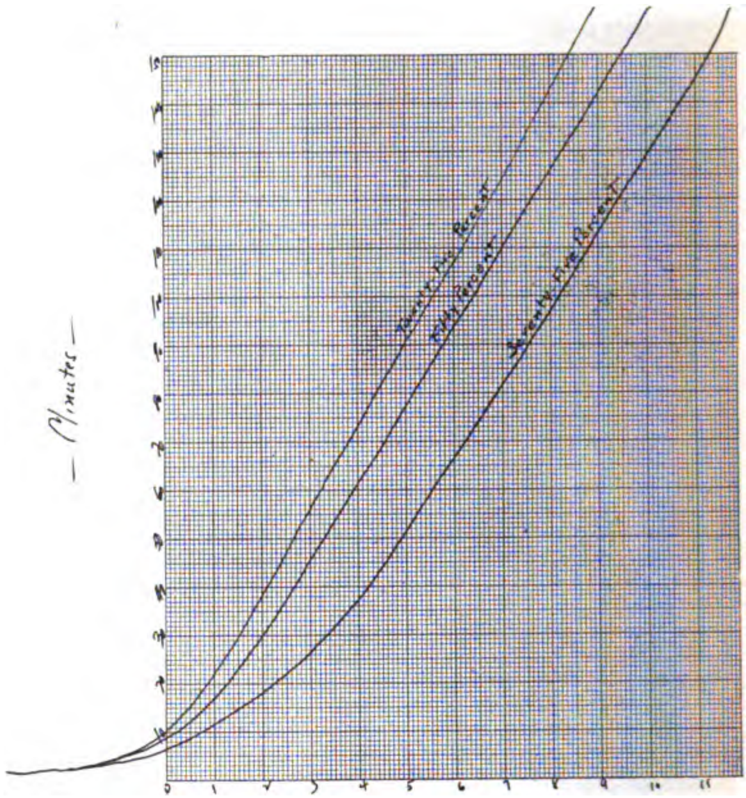

Chart IV.

tained in the blood to produce any desired physiological effect that has a quantitative relation thereto, for example, the third or surgical stage of anesthesia.[15]

The last mentioned has been demonstrated clinically by Wallace, who found respiration and blood pressure fully maintained, and Gwathmey and others with records to date of about 1,000 human cases. So far, not a case of post-ether pneumonia has been encoun-

[15] In this connection it may be stated that about 30 mils of a 75 per cent. mixture to 20 lbs. of body weight is administered as an enema.

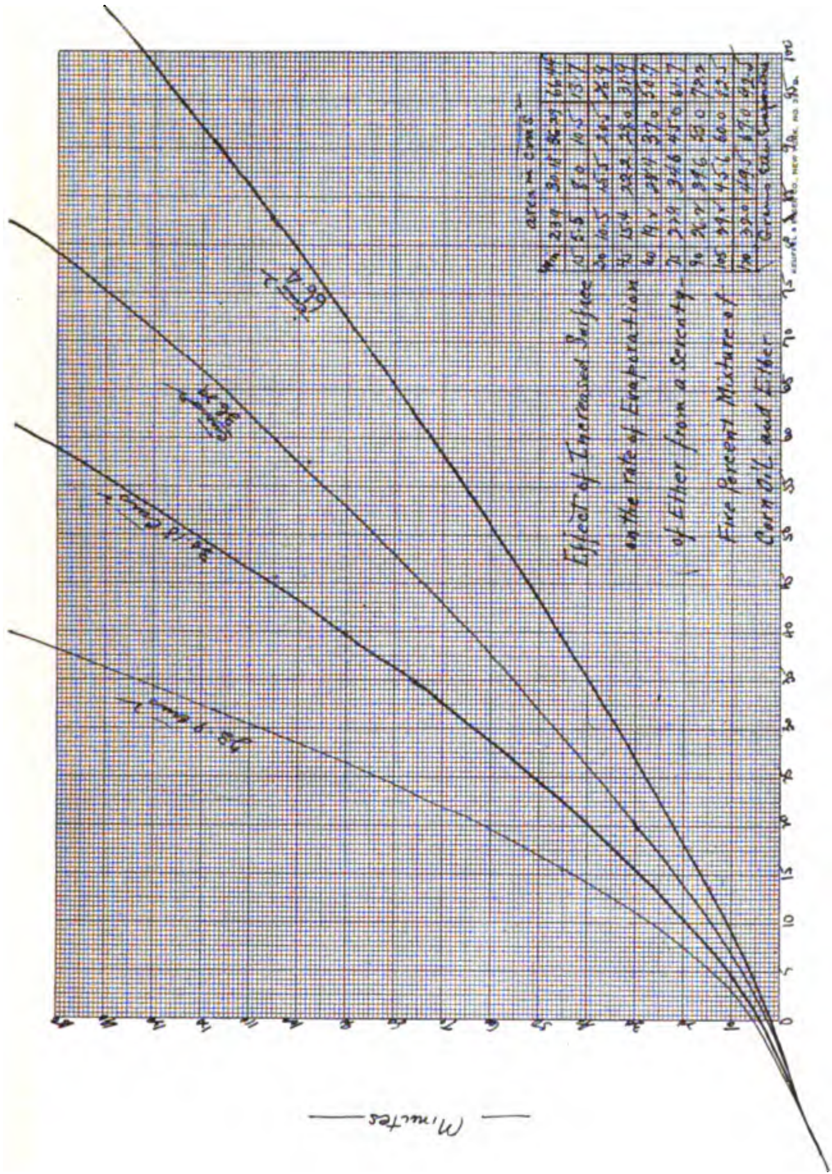
Effect of Increased Surface on the rate of Evaporation of Ether from a Seventy-Five Percent Mixture of Corn Oil and Ether
Minutes

CHART V.

tered. The after-effects usually associated with inhalation anesthesia, unless induced by the most improved modern technique, are virtually absent, including post-anesthetic nausea. Its use for special cases involving the head, breathing passages, etc., is superior. Although having had the privilege of attending clinics, I am not qualified to pass judgment upon its value, but from what I have learned, if necessary, "Give it to me that way."

College of the City of New York.

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2. Persons of any nation, sect or denomination whatever, shall be admitted as candidates for this premium.
3. No discovery, invention or improvement shall be entitled to this premium which hath been already published, or for which the author hath been publicly rewarded elsewhere.
4. The candidate shall communicate his discovery, invention or improvement, either in the English, French, German, or Latin language.
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TO THE SECRETARIES OF THE

AMERICAN PHILOSOPHICAL SOCIETY

No. 104 SOUTH FIFTH STREET

PHILADELPHIA, U. S. A.

PROCEEDINGS

OF THE

American Philosophical Society

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FOR PROMOTING USEFUL KNOWLEDGE

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THE AMERICAN PHILOSOPHICAL SOCIETY

104 SOUTH FIFTH STREET

1915

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SYMPOSIUM ON THE EARTH: ITS FIGURE, DIMENSIONS AND THE CONSTITUTION OF ITS INTERIOR.

I.

THE INTERIOR OF THE EARTH FROM THE VIEWPOINT OF GEOLOGY.[1]

BY T. C. CHAMBERLIN.

(*Read April 24, 1915.*)

For some time past there has been a marked drift of geologic opinion from the older tenet of a molten earth toward the conviction that the earth is essentially solid. This has been quite as much due to the contributions of kindred sciences as to the growth of geologic evidence, but this has made its important and concurrent contributions.

The great granitic embossments that constitute the most distinctive feature of the oldest known terranes were formerly regarded as solidified portions of a primitive molten earth and seemed to serve as witnesses of the verity of the former liquid state. A few years ago, however, it was determined—almost simultaneously in several countries where critical studies on these formations were

[1] The discussion of this topic at the session of the Society was without manuscript or notes and this paper, prepared some weeks later, is less a reproduction of the original discussion than a substitute for it.

in progress—that these granitic masses are not only intrusive but that they were thrust into formations that had previously been formed at the surface of the earth. These surface formations have thus come to stand as the most ancient terranes now known. These earliest accessible depositions imply the preëxistence of a substantial foundation formed at a still earlier date. Neither of these gives any clear intimation that lower formations are different from themselves. So far then as the record runs back, it testifies to substantial solidity in the outer part of the globe at least. The record implies, indeed, that molten matter was then present within the earth, but it gives no certain measure of the ratio of the molten to the solid part. There is no determinate evidence that a molten condition was a preponderant state, even in the interior, at any stage covered by the lithographic record. The interior conditions of the earliest stages that antedate the lithographic record are to be reached only by indirect and remote rather than direct and immediate inference. Under the influence of inherited presumptions, it may seem to many still probable that the interior of the mature earth was once dominated by a molten condition at some remote stage, but the phenomena of powerful inthrusting, so often shown in the intrusions of the igneous element into the early terranes, seems to imply that at the Archean stages the molten element was in the strong grasp of such stresses as are natural to a rigid globe and was therefore then but a minor and passive factor, not a controlling one.

When it is considered that, if the earth were once wholly molten, the material for all the stratified rocks of later ages must have been derived from the primitive crust after it was formed and forced into positions of erosion—or from matter extruded through it—the absence, according to present knowledge, of any great area of rocks bearing the distinctive characteristics of the congealed surface greatly weakens the assumption that the postulated molten state ever obtained in the mature earth.

A study of the stress-conditions of the interior of the earth seems to call for a similar reversal of the inferences once drawn from the igneous rocks. From the earliest well-recorded ages, the exerior of the earth has given evidence of broad topographic reliefs

in the form of great embossments and basins. These surface configurations must have conditioned the localization of extrusions and the deployment of the effusive material. If the lavas arose from a general and abundant source of supply which was responsive to general and powerful stresses, vestiges of this vital relation should be found in the volume and deployment of the lava floods. If, on the other hand, the molten material was but a fraction of the environing mass, variously distributed through it, the result should be a multitude of driblets squeezed out here and there in such special situations as the controlling stresses required, or else forced into weak portions of the earth-body where the stresses were less imperative. Now there is abundant geological evidence that the earth-body has been subjected at repeated intervals to strong compressive stresses by which its outer portion has been folded into mountainous ranges, or pushed up into great plateaus, while masses of continental dimensions have been raised, relatively, to notable heights, and the bottoms of basins and deeps have sunk reciprocally to even greater relative depths. The internal stresses which these deformations imply should have made themselves felt proportionately on any great mass of liquid in the interior—if it were in existence—and extrusions proportionate to the great deformations of the rigid material should have accompanied such diastrophism. But, while liquid extrusions took place somewhat freely at the times of great diastrophism, it was not, at least in my judgment, at all commensurate with the deformative stresses implied by the diastrophic results in the solid material.

Nor was the concentration of the extrusions indicative of origin from a molten interior or from great residual reservoirs of liquid rock. If such ample sources of liquid had existed they might naturally have been expected to have given forth, under the great stresses then seeking easement, correspondingly great floods of lava. Yet no single lava flood seems to have attained more than an extremely small fraction of the mass of the earth or of the known solid matter of its region. Even when the sum total of the most massive series of successive floods in a given region are taken together—though the successive issues stretched over a considerable period—they rarely rise above a most insignificant fraction of earth-mass or even of the

regional segment of it with which they are associated. Instead of really massive flows, implying ample sources of supply and great forces of extrusion, the record shows rather a multitude of little ejections or injections of more or less sporadic distribution. The logical implication of these is the preëxistence of a multitude of small liquid spots, or liquifiable spots, scattered widely through the stressed earth-masses and yielding to stress as local conditions required.

This inference is supported by the great variations in altitude at which lavas are given forth. The most impressive illustrations of this are found in current volcanic action whose relations in altitude are precisely known. So far as ancient conditions can be restored, they appear to fall into the same general class as existing conditions. Current outpourings of lava range from the sea bottom to altitudes of many thousands of feet above sea level, a vertical range of several miles. Extrusions occur at these significantly diverse altitudes simultaneously or alternately or in almost any time-relations, and sometimes in the most marked independence of one another in spite of the natural sympathy of such events in a common stressed body. A multitude of facts of detail, some of which are singularly cogent, imply that the lava sources of present volcanoes are disconnected from one another in the interior, and hence independent in action, as a rule, though sometimes they show sympathy without showing liquid connection. The sources of lava seem to be meager in general, and the eruptive agencies seem to be controlled by narrowly local conditions. There is an absence of evidence that the lavas in the craters and necks of volcanoes are parts of great liquid masses below, responsive to the common stresses of a large region.

Thus geological evidence, when critically scrutinized, seems to be distinctly adverse to the existence of even large reservoirs of molten matter within the earth; it points rather to the presence of scattered spots, very small relatively, on the verge of liquefaction, which pass by stages into the liquid form and are then forced out by the differential stresses that abound in the earth body, each such local liquifying center commonly giving forth driblets of lava and gas, at intervals, none of which often rise to more than an extremely minute fraction of the earth mass or even of the subterranean mass contiguous to the volcano.

A revised view of the nature and location of earth-stresses seems also to be required by what is now known of earth-conditions.. Under the former dominance of the tenet of a molten globe, it was natural to assign to the stress-differences of the earth a distinctly superficial localization and limitation; they were thought to be affections of "the crust" almost solely. Hydrostatic pressures were of course recognized as affecting the deep interior, but these were obviously balanced stresses, they were ineffective in deformation. The stresses supposed to give rise to the great reliefs of the earth's surface were thought to be very superficial. But the stresses imposed by known deformative agencies are not all superficial, nor are their intensities always greatest at the surface. According to Sir George Darwin, the stress-differences generated in the earth by the tidal forces of the moon are eight times as great at the center of the earth as at the surface. So also, according to the same authority, the stresses engendered by changes in the rotation of the earth are eight times as great at the center as at the surface and are graded between center and surface. The tidal stress-differences are relatively feeble but are perpetually renewed in pulsatory fashion. Those that arise from rotation belong to the highest order of competency. The stress-difference that would arise at the center of the earth from a stoppage of the earth's rotation, would, according to Darwin, reach 32 tons per square inch. Changes of the rate of rotation are almost inevitable when great diastrophic readjustments take place. Such periods are to be regarded as critical times at which great floods of lava should be poured forth from the interior if liquid material were there in great volume ready to respond to the changes of capacity which the deformation of the earth's sectors and the change in the spheroidal form would inevitably impose.

Not to detain you with other considerations, the foregoing seem best to comport with an essentially solid state of the earth's interior, if they do not point rather definitely to such a state. Even if they stood alone, they would seem to make a prevailing solid state the most tenable working hypothesis.

But they are far from standing alone; the geological evidences are strongly supported by considerations that spring from several kindred lines of inquiry. The testimony of astronomic evidence

has been given by Dr. Schlesinger. The import of seismic studies, the subject of Dr. Reid's contribution, lends very special support to the view that the interior of the earth is elastico-rigid at least to the extent that distortional waves have been shown to pass through its interior. It seems certain already that this condition prevails throughout much more than half the volume of the earth; concerning the rest, the deep interior, the seismic evidence is perhaps still to be regarded as indeterminate. But on the seismic evidence it does not fall to me to dwell.

The tidal studies of Hecker, Orloff and others lend support to the tenet of a rigid earth but they fall somewhat short of conclusiveness. The brilliant experimental determinations of Michelson and Gale, correlated with the computations of Moulton, have carried the evidence to the point of preliminary demonstration. They need only to be adequately repeated and verified to become final, so far at least as elastic rigidity can be indicated by the response of the earth-body to solar and lunar attractions. The special feature of most critical value in the demonstrations of Michelson and his colleagues is the high degree of elasticity shown by the almost instantaneous response of the earth to the distorting pull of the tide-producing bodies. This cuts at the very base of concepts founded on the supposed properties of a viscous earth. These tidal determinations of elasticity are in close accord with the seismic evidences of elasticity. The two are happily complementary to one another. The one deals with the earth as a whole under rhythmical series of increasing and diminishing stress-differences springing from external attraction; the other deals in an intensive partitive way with earth substance by sharp short stresses that call into action its most intimate structural qualities. While it is wise, no doubt, to refrain from resting too much on these early results of relatively new and radical lines of inquiry, until their results shall be more mature, their prospective import is radical and decisive in favor of a solid earth not only, but of an elastico-rigid earth. Assuming that the present import of these inquiries will be amply justified by more mature research, it is pertinent to bring into consideration the corollary they so distinctly imply, viz.: that the molten and viscous material in the earth, or at least in its outer half, if not throughout

its deep interior, is negligible in general studies, and enters into general terrestrial mechanics only as a subsidiary feature. It seems necessary to limit liquid and viscous lacunæ—if there are lacunæ in any proper sense at all—to such moderate dimensions that they do not seriously kill out distortional waves passing through the outer half of the globe in various directions; for seismic instruments show that these waves retain their integrity with surprising tenacity through long traverses. It seems equally necessary to limit the liquid and viscous factor rather severely if the interior structure is to be consistent with so prompt a response of the earth to twelve-hour stress-pulses as to imply almost complete elastic fidelity.

In the light of these determinations, strengthened not a little by their concurrence with the later geological determinations, the working hypotheses of the earth-student can scarcely fail to give precedence to dynamic tenets founded on a rigid earth.

The limitation of liquid and viscous matter, thus imposed, quite radically conditions all tenable views of magmas and of vulcanism, and thus bears upon the igneous nature of the interior. No small part of petrologic effort in past decades has been spent on the differentiation of magmas. To a notable degree these efforts have proceeded on the assumption, conscious or unconscious, that differentiation took its departure from an original homogeneous magma such as might arise from residual portions of a molten earth. Indefinite lapses of time, and such conditions of quiet as are naturally assignable to residual reservoirs of lava, have been freely assumed as working conditions without much question as to their reality. Under the hypothesis of a molten earth passing slowly into a partially solid earth, and retaining residual lacunæ of molten matter as an incident of the change, these assumptions are quite natural. On the other hand, under the hypothesis of a pervasively rigid earth, affected by stress-conditions that are constantly varying in intensity and in distribution—and subject to more radical changes at times of periodic readjustment—the existence of such residual magmas becomes at least questionable, perhaps improbable. Still more questionable is the assumption that the multitude of little liquid spots supposed to arise within the elastico-rigid mass, always have conformed to one type or set of types. The inherent proba-

bilities of the case seem to point strongly to wide variation in nature due to selective solution or differential fusion. The liquefying action that brings magmas into being, under this view, is presumably controlled by the same chemical and physical principles as the solidifying phases of the same cycle. The logical presumption is that at all stages of a magma's career from its inception through its growth, climax and decline to its final solidification, selective action will be in progress more or less and that no stage will be entitled to be regarded as original or parental in a special sense, such a sense for example as might be appropriate if the lava were the residue of an inherited original state and were merely differentiated by fractional crystallization as it passed toward solidification.

While these contrasted views of the history of magmas are naturally connected with views of the genesis of the earth, they are not limited to this relation. They are inherent in the very relations of solid and liquid matter and have a more or less important place irrespective of the earth's genesis.

An element of no small importance to a revised concept of the interior of the earth has arisen from geodetic studies on the distribution of densities within the earth. As the geodetic point of view is to be presented by its foremost exponent, Dr. Hayford, it is permissible for me merely to refer to certain geologic bearings.

On the assumption that the earth was once in a molten state, the inference is unavoidable that a perfect state of isostatic equilibrium was originally assumed by the surface, and that its configuration was at first strictly spheroidal. The material must have been arranged in concentric layers according to specific gravity and each layer should have had the same density at every point. All such reliefs of the earth's surface, and all such differences of specific gravity in the same horizon as have since arisen, must have been superinduced upon this originally perfect isostatic surface. With good reason therefore these inequalities have heretofore been supposed to be relatively shallow. On the hypothesis that the earth grew up by heterogeneous accretions, it is an equally natural inference that differences of specific gravity extend to great depths. In an endeavor to find out the bearings of geodetic data on the distribution of densities, Dr. Hayford tested four assumptions, all of

which he found measurably compatible with his geodetic data. From these he derived the respective depths of 37, 76, 109 and 179 miles as the horizons to which differences of density extended and below which they vanished or became negligible. Now all these depths are greater than had been assigned for probable differentiation in the traditional molten earth. On the other hand, the highest figure, 179 miles, was derived from a curve drawn specifically to represent the probable distribution of densities in an earth of planetesimal growth. The distribution represented by this highest figure fits the geodetic data quite as well as either of the other assumptions of distribution, though drawn on a strictly naturalistic basis If it could be said that geodetic data demonstrate that the actual differentiation of specific gravities has its sensible limits somewhere between 37 and 179 miles below the surface, such considerable depth would distinctly favor an accretionary origin as against a molten origin. But a conclusive determination is yet to be reached by geodetic inquiries.

While it is possible, within the broad terms of the planetesimal hypothesis, to suppose that the rate of accretion was so fast as to give rise to a molten planet, such a result seems to me extremely improbable under the actual conditions of the case. The growing planet should have become capable of holding a considerable atmosphere by the time it attained one tenth of its present mass, *i. e.*, about the mass of Mars. After this the protective cushion of the atmosphere should have greatly checked the plunge of the planetesimals and largely dissipated them into dust in the upper atmosphere where the inevitable heat of impact would be promptly radiated away. The dust presumably floated long and came gently to earth, so that, while the total heat generated by impact was large, the temperature of the earth body was probable never very high during the later stages of growth, and perhaps not at any stage of growth. Following out as well as may be the probable rates and conditions of growth, the most tenable concept of the state of the earth's interior under the planetesimal hypothesis is as follows:

The condition of the nuclear portion supposed to be formed from one of the knots of the parent spiral nebula and constituting a minor fraction of the mass of the earth, say thirty or forty per cent., is

left indeterminate by present lack of knowledge of the physical state of the knots of spiral nebulæ. If these are gaseous—which is rendered doubtful by their lack of strict sphericity—the nucleus was doubtless originally molten. If the constituents of the knot were held in orbital relations, their aggregation might have been slow enough to permit a solid state of even this portion. The matter added to the nucleus as planetesimal dust, or as planetesimals reduced in mass and speed by the atmosphere, probably retained its solid condition, with negligible exceptions, throughout the process of accretion except as selected portions passed into the liquid state and became subject to extrusive action. An intimate heterogeneity naturally prevailed throughout the whole mass so aggregated. A selective process, however, probably brought in the heavier matter faster and earlier than the lighter matter, for the magnetism of the earth should have aided gravity in gathering in the magnetic metals while the inelastic planetesimals, predominantly the heavy basic ones, when in collision destroyed the opposing components of their motions and hence yielded to the earth's gravity sooner than the more elastic ones. Relatively high specific gravity in the material of the deep interior is thought to have arisen at the outset and to have been increased by the selective vulcanism that came into action as growth proceeded. Special emphasis is laid on the *selective* nature of vulcanism under this hypothesis. The intimate mixture of planetesimals and planesesimal dust gave rise to a multitude of minute contacts between particles of different chemical and physical properties and hence there arose wide differences in the solution points. As the temperature in the growing planet rose, the more soluble portions passed into the liquid state by stages long before the remaining larger portion reached the temperature of solution. In a stressed globe certain of whose stresses are more intense toward the center than toward the surface, the solutions worked in the direction of least resistance, for them generally outwards, carrying heat of liquefaction and leaving the less soluble larger portion behind with temperatures inadequate for further liquefaction until there was a renewed accession of heat. The mechanism thus automatically tended to remove the most soluble constituents by progressive stages, while it tended to preserve the solid condition of the

main mass. The hypothesis thus supplies a working mechanism whose results fall into full accord with the states of the interior implied by tidal investigations and by seismic data, while the postulated distribution of specific gravities accords fairly well with geodetic determinations, as they now stand.

The adaptation of such an earth to isostatic adjustment can scarcely be more than hinted at here. The growth of the earth should have given it a concentric structure, while its highly distributive vulcanism, together with some of its deformative processes, should have given a vertical or radial structure, the two conjoining to give a natural tendency to prismatic or pyramidal divisions converging toward the center. The most powerful of all the deformative agencies, rotation, required for the adaptation of the earth to its changes of rate, such divisions of the earth-body as would respond most readily to depression in the polar and bulging in the equatorial tracts reciprocally. As urged elsewhere, this accommodation seems best met by three pyramidal sectors in each hemisphere with apices at the center and bases at the surface, the sectors in opposite hemispheres arranged alternately with one another. Very simple motions of these sectors on their apices at the earth's center would satisfy the larger demands of rotational distortion, while the sub-sectors into which these major sectors would naturally divide, as stresses required, would easily accommodate the nicer phases of adjustment. This primitive segmentation to meet rotational demands—which were most urgent during the stages of infall—furnished a mechanism suitable for the easement also of a portion of the deformational stresses that arose from other sources, among them gravitative stresses arising from loading and unloading by erosion and sedimentation. A gravitational adjustment by the wedging up and down and laterally of such sectors is thus offered tentatively as a working competitor to theories of adjustment by fluidal or quasi-fluidal undertow. The necessary brevity of this statement leaves this new hypothesis little more than a crude suggestion that gravitative adjustment (= isostasy) may perhaps take place as fully as the case requires in a highly rigid elastic earth without resort to flowage or even quasi-flowage.

The University of Chicago.

II.

CONSTITUTION OF THE INTERIOR OF THE EARTH AS INDICATED BY SEISMOLOGICAL INVESTIGATIONS.

By HARRY FIELDING REID.

(Read April 24, 1915.)

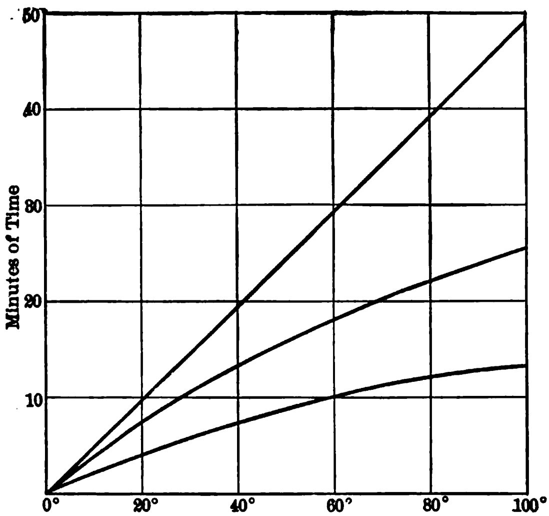
In 1883 Milne predicted that earthquake disturbances would be registered by seismographs at great distances from their origin, a prediction first verified when the earthquake of April 18, 1889, whose origin lay off the coast of Japan, affected the horizontal pendulum which von Rebeur-Paschwitz had set up at Potsdam to study the attraction of the moon. Milne was so convinced of the correctness of his idea and of the importance of the results to be obtained that in 1893 he established an observatory on the Isle of Wight to record earthquakes from distant regions; and he also succeeded in having instruments of similar model set up at observatories very widely scattered in various parts of the world.

Wertheim in 1851 showed that a disturbance in the interior of an elastic solid would break up into two groups of waves, longitudinal and transversal, which would be propagated at different rates, and as their velocities are so great that they cannot be separated from each other in the laboratory he suggested with rare insight that their separation might first be noticed in connection with the propagation of earthquake disturbances.[1] A few years later Lord Rayleigh showed that a third kind of wave could be propagated along the surface of the earth.[2] Seismologists naturally looked for indications of these three groups of waves in their

[1] "Sur la propagation du movement dans les corps solides et liquides," *Ann. de Chimie et Phys.*, 1851, Vol. XXI., p. 19.

[2] "On Waves Propagated along the Plane Surface of an Elastic Solid," *Proc. London Math. Soc.*, 1855, Vols. XLVII., L.

seismograms, but it was not until 1900 that Oldham succeeded in showing definitely that the seismograms of a number of Milne instruments gave clear evidence of the existence of three groups of waves. Oldham also published a diagram, which was an extension of Seebach's so-called "hodograph," showing the relation between the time of transmission of each group and the distance from the earthquake origin, measured along the surface of the earth. Milne soon improved these curves by adding observations of a large number of recorded shocks.[8] The general forms of the transmission curves are shown in the diagram. It will be seen that the curves



of the first and second "preliminary tremors," as Milne called the first two groups of waves, are curved, indicating that the velocity of transmission increases with the distance from the origin; a conclusion which had already been drawn from earlier, but less accurate, observations. Milne attempted to explain this by assuming that the path of the seismic disturbance lay along the chord and not along the earth's surface; this practically shortens the distance to the observing stations, and if the curves are plotted, with distances

[8] Rep. of the Com. on Seismol. Investig., B. A. A. S., 1902, p. 7.

measured along the chord, the curvature is considerably diminished; but later and more accurate observations show that even under this assumption the velocity still increases with the distance. The conclusion is unavoidable that as the path of the disturbance sinks deeper into the earth the velocity increases. The interior of the earth then is not a homogeneous but a refractive medium, and the path of the disturbance cannot be straight but must be curved with the concavity turned upward. This condition had been described by A. Schmidt as early as 1888.[4] Seismologists now believe that the three groups discovered by Oldham are respectively the longitudinal, the transverse and the surface waves. The transmission curve of the latter is a straight line indicating that the waves are transmitted with uniform velocity along the surface of the earth. They have affected seismographs after having passed completely around the earth. It cannot be said that the evidence, that the first two groups are respectively longitudinal and transverse, is complete; but it is sufficient, in connection with theory, to make seismologists fairly confident that the conclusion is correct; and the passage of transverse waves through the earth to great depths is proof that, to those depths, the earth is solid; for transverse waves cannot exist in a liquid. Further, since the velocity of transmission depends on the ratio of the elasticity to the density of the medium, and since both the longitudinal and transverse waves increase in velocity with the depth below the surface, both the elasticity of volume and the elasticity of figure of the earth, not only increase, but increase more rapidly than the density as we penetrate below the surface. The earth therefore is not only rigid, but its rigidity increases towards its center; though seismological evidence does not yet prove that this characteristic extends to the very center itself.

The next step was to determine the path of the waves in the earth and their velocity at different depths; the data for these determinations were the times of arrival of the earthquake waves at various distances from the origin; these times are collected in the transmission curves. At first sight this seems an insoluble problem;

[4] "Wellenbewegung und Erdbeben," *Jahreshefte für Vaterlands Naturkunde in Württemberg*, 1888, p. 248.

but, thanks to a remarkable mathematical theorem of Abel, it is not. It is clear that the time of arrival of an earthquake disturbance at a distant station will depend on the path followed and the velocity in different parts of the path, and if we make the reasonable assumption, which is borne out by observation, that the velocity is everywhere the same at the same depth, then it is evident, if the velocity increases continuously with the depth, that the transmission curves will be continuous without breaks, and their curvatures will nowhere make a sudden change. The mathematical solution of the problem has been obtained by Wiechert, Bateman and others, and concrete results have been obtained by Wiechert and his assistants, so that we now know the paths of the waves and their velocities with a fair degree of accuracy, at least to a considerable distance below the surface. But the questions arise: do the velocities increase continuously with the depth; and if so, how? questions which could be answered by the study of perfect transmission curves; but even imperfect curves yield some information; which, however, may be so faulty that it must be received with great caution. Milne, who has done such excellent pioneer work in seismology, was the first to propose and attempt to answer these questions.[5] He thought the transmission curve could be satisfied by supposing the earth to consist of a solid core having a radius of nineteen twentieths of the earth's radius, and surrounded by a thin shell. The core was of uniform density and elasticity, so that the velocity of propagation in it was uniform, and the paths of the rays would be straight lines. The velocity in the shell was much less than in the core. These conditions satisfied fairly well the very imperfect transmission curve of 1902, but they may be dismissed without further consideration, for such an earth could not satisfy the astronomic requirements, which exact, at the same time, the proper mean density and moment of inertia.

Benndorff in 1906 thought he found evidence of a central core of about four fifths the earth's radius, surrounded by two shells, the outer one having the same thickness as Milne's.[6] In the same

[5] Rep. of the Com. on Seismol. Investigation, B. A. A. S., 1903, p. 7.

[6] "Ueber die Art der Fortpflanzungsgeschwindigheit der Erdbebenwellen in Erdinnern," *Mitt. d. Erdbeben Com. k. Akad. Wiss. in Wien*, 1905, Nos. XXIX. and XXXI.

year Oldham deduced from the transmission curves a central core of not more than four tenths the earth's radius in which the velocity was distinctly less than in the surrounding shell.[7] Neither of these arrangements have been shown to conform to the astronomic requirements. Oldham's conclusions are based on what he considers a distinct break in the transmission curve of the transverse waves at distances between 120° and 150° from the origin; but when we remember that fully 95 per cent. of the energy of an earthquake shock comes to the surface within the hemisphere having the origin as its pole, we see that the data for great distances must be too imperfect to yield very reliable deductions.

Many years ago Roche showed that it was quite possible to determine a distribution of density in the earth which would be discontinuous at several levels, but which would still be astronomically satisfactory. Wiechert, in 1897,[8] showed that such a system might consist of a central core of radius about 4,900 km. or three fourths of the earth's radius, consisting of iron with a density of about 8.3, surrounded by a stony shell about 1,500 km. thick and with density varying from 3 to 3.4. It was natural that he should examine the transmission curves to see if they supported his ideas; and at the Hague meeting of the International Seismological Association in 1907 he announced that they did. At the Manchester meeting of the same association in 1911 he announced the existence of two shells around the central core. In 1914 Gutenberg (one of Wiechert's assistants) announced the existence of three shells.[9] In addition to ordinary times of transmission, Gutenberg also used the times of waves reflected at the earth's surface and the variations in the amplitude; it is evident that a wave which crosses the boundary of the core will experience reflection and refraction; and whichever part is later observed at the surface of the earth will have a dis-

[7] Constitution of the Interior of the Earth, *Quart. Jour. Geol. Soc.*, 1906, Vol. LXII., p. 456.

[8] "Ueber die Massenvertheilung im Innern der Erde," *Nachr. k. Gesells. Wiss. Göttingen*, 1897; *Math.-phys. Kl.*, p. 221.

[9] "Ueber Erdbebenwellen," VIIA. *Nach. k. Gesells. Wiss. Göttingen; Math.-phys. Kl.*, 1914, p. 1; references to the earlier numbers of the series are given in this paper.

tinctly smaller amplitude than the wave which just missed penetrating into the core. The following table shows the positions of the boundaries of the shells and of the core, and the velocities of the longitudinal waves *P* and of the transverse waves *S*; it will be noticed that it is only at the boundary of the central core that any marked sudden change in velocity occurs.

	Veloc. km./sec.	
Depth, kms.	*P*.	*S*.
0	7.17	4.01
1200	11.80	6.59
1700	12.22	6.86
2450	13.29 13.15	7.32 7.20
2900	13.15 8.50	7.20 4.72
6370	11.10	6.15

The remark regarding Oldham's results applies also here, namely that it is questionable whether the observations at distances greater than 100° or 120° are sufficiently accurate to justify such definite conclusions. Gutenberg had the advantage, however, of more accurate observations than Oldham, and also of measures of amplitudes. There is no *a priori* reason why the earth might not be made up of a number of shells, but there should be satisfactory evidence for any proposed system; and it must be shown to satisfy the astronomic requirements; or, at least, not to contradict them. Gutenberg's system does not correspond with Wiechert's system of 1897. In the latter a marked change in physical properties occurs at a depth of 1,500 km.; in the former, at a depth of 2,900 km.; and in crossing into the core, the ratio of the elasticity to the density, according to Gutenberg, rapidly loses six tenths of its value. This change might be the result of a great increase in density or a great decrease in elasticity; it may be questioned whether the former is compatible with the astronomic requirements, and whether the latter is compatible with the high rigidity which we know the earth, as a whole, has. So far no answer has been given to these questions.

In 1879 George and Horace Darwin attempted to determine the rigidity of the earth by measuring the deviation of the vertical under

the attraction of the moon. If the earth yielded like a fluid, its surface would always remain at right angles to the vertical, and a pendulum would remain relatively stationary for all positions of the moon; if the earth were absolutely rigid, the moon's attraction would deflect the pendulum an extremely small amount, but an amount capable of being measured. The Darwins did not obtain definite results because the disturbances of their pendulum were greater than the deflections they attempted to determine.

A little later von Rebeur-Paschwitz attacked the same problem with better success, using a horizontal pendulum.

Hecker, in Potsdam, and Orloff, in Dorpat, have repeated von Rebeur-Paschwitz's experiment; and both found values for the average rigidity of the earth comparable with that of steel. But, what was most remarkable and what is still unexplained, the rigidity was apparently greater in an east-west than in a north-south direction. Orloff, experimenting at a greater distance from the ocean, found a smaller difference than Hecker did, and it has been suggested that the tides of the ocean are the cause of the difference. The International Seismological Association, at its Manchester meeting in 1911, made plans to repeat the experiments in Paris, in central Canada, in the middle of Southern Africa and in the middle of Russia; but no reports have yet come from these stations.

In the autumn of 1913, Michelson attacked the same problem by a new method, which seems capable of yielding more accurate results than the horizontal pendulum. He measured the deflection of the vertical under the influence of the moon by what was practically a water level 500 feet long, sunk six feet in the earth.[10] Michelson's results for the E-W rigidity do not differ greatly from those of Orloff; but his N-S rigidity is somewhat less than Orloff's. Michelson's experiments also show that the viscosity of the earth must be as great as that of steel. These experiments are of great interest; they should be repeated at various places, and especially at places symmetrically situated with respect to the great oceans, and on mid-oceanic islands, in order to determine how far they are affected by the oceanic tides.

[10] "Preliminary Results of Measurements of the Rigidity of the Earth," *The Astrophysical Journal*, 1914, Vol. XXXIX., p. 97.

We can say in conclusion, that the transmission of transverse earthquake waves shows that the earth is solid, at least to a great depth below the surface; and that experiments on the deflection of the vertical show that it is quite as rigid and as viscous as steel. There are still difficulties in the interpretation of the observations, but their elucidation cannot alter the general character of the conclusions.

III.

THE EARTH FROM THE GEOPHYSICAL STANDPOINT.

BY JOHN F. HAYFORD.

(Read April 24, 1915.)

This is a broad topic on which much intensive thinking has been done by many men. It is impossible to treat it adequately or comprehensively in the short time available.

In this address an attempt will be made to so concentrate attention on a certain few points as to tend to clarify existing ideas and to correlate them. An attempt will also be made to help in locating the lines of least resistance to future progress in the study of the earth.

The size of the earth, as well as its shape, is now known with such a high degree of accuracy that the errors are negligible in comparison with the errors in other parts of our knowledge of the earth. The probable error of the equatorial radius is less than 1/300000 part, and of the polar semi-diameter is about the same.

The three physical constants of the earth, and of its different parts, on which you are now asked to concentrate your attention are the density, the modulus of elasticity, and the strength.

It is important to know as much as possible about the density. The more one knows about the density in all parts of the earth the more surely and safely one may proceed in learning other things about the earth.

The modulus of elasticity at each point in the earth controls the behavior of the earth under relatively small applied forces.

The strength of the earth, at each point, as measured by the stress-difference at that point necessary to produce either slow continuous change of shape or rupture, decides the behavior of the earth under the greater forces applied to it.

As to density we know that the earth's surface density is about

2.7, that the density probably increases continuously with increase of depth, that the density at the center is probably about 11, that the mean density is about 5.6, and that within a film at the surface of a thickness of about one fiftieth of the radius of the earth there is isostatic compensation which is nearly complete and perfect as between areas of large extent.

The manner of distribution of the isostatic compensation with respect to depth, and the limiting depth to which it extends are but imperfectly known. Nevertheless it appears that above the depth, 122 kilometers, the compensation is nearly complete even though there may be some compensation extending beyond that depth.

Two general lines of evidence are available in determining the modulus of elasticity of the earth, that from earthquake waves, and that from earth tides.

There are many inherent and extreme difficulties in the way of securing reliable evidence as to the modulus of elasticity from earthquake waves.

To 1913 the accuracy of available observations of tides in the solid earth was insufficient to furnish a basis for reliable conclusions. Nevertheless the estimates of the modulus derived from these early observations were a fair approximation to that given by the very recent and much more accurate observations.

Dr. Michelson and those associated with him in the observation of earth tides at the Yerkes Observatory since 1913 have developed a method of observing which is of a new order of accuracy such that the minute changes of inclination at a given point due to earth tides may be determined with an error of less than one per cent.

These observations make the modulus of elasticity of the earth as a whole about like that of solid steel, namely (8.6) (10^{11} C.G.S.).

It is the modulus of elasticity of the earth as a whole which is measured in this case.

It is eminently desirable to determine if possible whether the modulus of elasticity varies with increase of depth. The Michelson apparatus possibly opens the way to such a determination. Suppose that the apparatus is used on the shore of the Bay of Fundy. Twice a day a large excess load of water is placed in the bay by the tidal oscillation and as frequently the water load is reduced below

normal. The stresses produced in the body of the earth by these changes of load applied over an area only about 30 miles wide are probably confined almost entirely to the first 100 miles of depth. The magnitude of changes of inclination produced at an observing station on the shore by the changing water load would, therefore, be dependent primarily on the modulus of elasticity of the material below and around the bay to a depth of less than 100 miles. The observations might serve, therefore, to determine a modulus of elasticity of the surface portion of the earth rather than of the whole earth.

Turn now to the third of the physical constants which it was proposed to examine, namely the strength.

Among the forces which we may consider as furnishing tests of strength are: (1) the forces involved in earthquakes, (2) the weight of continents, and (3) the weight of mountains.

The forces which produce the more intense earthquakes evidently cause stress-differences locally which are beyond the breaking strength of the material. However from earthquakes we may obtain but little information as to the strength of the earth material because the intensity of the stress-differences cannot be reliably determined. We know simply that the intensity exceeds the breaking strength of the material, at the points of rupture.

It is uncertain how great are the maximum stress-differences produced by the weight of continents. One great difficulty in computing these stress-differences arises from the fact that the isostatic compensation of continents, now known to exist, reduces the stress-differences much below what they would otherwise be. Love computed the maximum stress-differences thus reduced as .07 ton per square inch. Darwin computed the greatest stress-difference due to the weight of the continents, without isostatic compensation, as 4 tons per square inch. If each of these computations were based upon assumptions which correspond closely with the facts one should be warranted in drawing the conclusion that the maximum stress-difference caused by the actual continents supported in part by the actual isostastic compensation is between .07 and 4 tons per square inch, and that it is much nearer to the smaller than to the larger value. But a close examination of either of these computations shows that it is based

upon assumptions made to simplify and shorten the computations, which assumptions depart widely from the facts and tend strongly to make the computed stress-differences much smaller than the actual. For example, both Darwin and Love used in their computations hypothetical continents represented by regular mathematical forms in the place of the actual continents with their many irregularities. The maximum stress-difference caused by the actual continents is necessarily much greater than would be produced by the assumed smoothed out, regular, symmetrical continents.

Similarly, no adequate computations have been made to determine the maximum stress-difference due to the mountains. Darwin computed the maximum stress-difference produced by two parallel mountain ranges, of density 2.8, rising 13,000 feet above the intermediate valley bottom, to be 2.6 tons per square inch. Love, for the same mountain ranges, but with isostatic compensation taken into account, computed the maximum stress-difference to be 1.6 tons per square inch. In this case the computation indicates that the isostatic compensation reduced the maximum stress-difference to but little more than one half what it would otherwise be. Here again both the computed maximum stress-differences have been greatly reduced by substituting hypothetical smoothed-out mountains in the place of the actual irregular unsymmetrical mountains.

To the person who is trying to get a true picture of the present state of stress in the earth, two very important facts are made evident by a comparison of the Love and the Darwin computations. First, the existence of isostatic computation greatly reduces the stress-differences which would otherwise be produced by the weight of the continents and mountains. Second, the depth at which the maximum stress-difference tends to occur is evidently very much less with isostatic compensation than without it. These two conclusions, based on the differences between the two computations, are apparently reasonably safe even in spite of the same wild assumptions on which both the computations were based.

Note that even a little information as to the distribution of densities—a little information about isostatic compensation—profoundly modifies the conclusions as to the state of stress in the earth. It should, therefore, be clear why it was so emphatically stated in

an earlier part of this address that information as to the distribution of density in the earth is necessary in order to make safe progress in learning other things about the earth.

Is the earth competent to withstand without slow yielding the stress-differences due to the weight of continents and mountains, the isostatic compensations being considered? From the computations by Darwin and Love, considered in the light of the assumptions made by them to simplify the computations, I estimate that it is probable that the actual mountains and continents with all their irregularities of shape and elevation possibly produce stress-differences in some few places as great as four tons per square inch, and certainly produce stress-differences at many places as great as two tenths of a ton per square inch. The material would certainly yield slowly under such stress-differences especially when they persist continuously over long periods of time and throughout large regions. Four tons per inch is the breaking or rupture load for good granite, one of the strongest materials existing in the earth in large quantities. Two tenths of a ton per square inch is the safe working load used by engineers for good granite. There is abundant evidence from laboratory tests that the so-called yield point on which the engineer bases his estimate of safe working load for a given material is a function of the length of time the load is applied and the delicacy of the test. The longer the time of application and the more refined the test to determine the permanent yield the lower the observed yield point. In the case of the test in progress in the earth the time of application is indefinitely long and the test is extremely refined inasmuch as the minimum rate of yielding which may be detected is exceedingly small.

If an engineer wishes to know whether a bridge, or foundation, or building, or railroad rail is yielding under stress-differences which have been brought to bear upon it he looks for evidence of distress, for rivet heads popped off, scaling from the surface, settling, cracks, or even changes in microscopic structure. The geologists have made very extensive corresponding examinations of the earth. Everywhere they find evidence that the earth has yielded. On the one fourth of the earth's surface exposed to examination, the land, there is no part for which the evidence does not indicate

past uplift, or subsistence, or horizontal thrust, or cracking under tension, or cracking produced by shear, or microscopic yielding in detail such as produces schistosity for example, or some other form of past yielding to stress-differences. The physicist studying the earth must take this overwhelming mass of evidence into account and must conclude that the earth habitually yields slowly to the stress-differences brought to bear upon it. Please note that I do not assert that the stress-differences are all due to gravity.

I propose now to state what are in my opinion probably the lines of least resistance to future progress in studying the earth from the physical standpoint. I propose to outline what I believe to be the most effective methods of attack, and to indicate some of the conclusions which will probably be reached. I am led to this procedure by two considerations. First, I find it possible to state certain of my opinions as to the net outcome of past investigations most clearly in that form—and time presses. Second, I indulge the hope that such an outline which is frankly an expression of judgment based on evidence much too weak and conflicting to be proof, may possibly kindle the imagination of some man or men, and so lead to vigorous attacks upon the problem and to future progress.

In attacking the problems of the earth one should assume at the outset that the phenomena exhibited are very complicated, that they are probably due to various simultaneous actions, and that the various actions are probably closely interlocked, modifying each other, though some are probably primary in importance and others secondary. Hence the most effective method of attack is probably one which includes a general correlation of apparently widely separated ideas and facts gathered from physicists, engineers, geologists, chemists, etc., and at the same time includes intensive attacks in detail on one after the other of single features of the problems which arise and an intensive working out of the possible consequences of said features.

It should be recognized at the outset that no observed behavior of the earth clearly warrants the assumption that the material of which it is composed differs radically in any way from that accessible at the surface. It should be assumed, therefore, that throughout the earth the materials are a mixture differing from the mixture

found at the surface only as the extreme pressure and temperature conditions at great depths directly and indirectly produce differences.

It should be kept clearly in mind that the geodetic evidence from observations of the direction and intensity of gravity indicates simply the present location of attracting masses, the present distribution of density. It furnishes no direct evidence whatever as to past distributions of density, or as to changes in density now in progress. But an understanding of the present distribution of density within the earth, especially near the surface, is so necessary to a true understanding of the present state of stress and of viscous flow in the earth that an understanding of the geodetic evidence is fundamental to progress.

Computations should be made in extension of those which have been made by Darwin and Love. The new computations should, however, deal with the actual irregular continents and mountains, not with regular substitutes. The computations should also take into account the bulk modulus of the materials composing the earth, that is these materials should be assumed to be compressible. Such computations will no doubt be both difficult and long. I believe that even a moderately vigorous attack along this line will show conclusively that the earth does not behave as an elastic body under the large loads superimposed upon it by the continents and mountains. I believe that the computed stress-differences will be found to be so large that the computation will be essentially a proof of viscous yielding.

Next make the contrasting assumption that the material composing the earth is competent to withstand but little shearing stress, and that the pressure at any point is that due to gravitation acting on the mass in the column extending from the point vertically to the surface. Let it be assumed that isostatic compensation exists, is uniformly distributed with respect to depth, and is complete at depth 122 kilometers. Consider the actual topography and form a mental picture as accurately as possible of the viscous flows which would take place on the assumption that at each level the material would flow horizontally from regions of greater pressure to regions of less pressure along lines of maximum rate of change of pressure, and that the time rate of such viscous flows would tend to be pro-

portional to the space rate of change of pressure. The flows would all be found to be away from beneath high regions toward low regions, from continents toward oceans, from mountains toward valleys.

After such a picture has been clearly formed assume that the isostatic condition is disturbed by long-continued erosion and deposition producing changes in the surface elevations and surface loads. On the same assumptions as to the nature of the viscous flows as before, form a new picture of the viscous flows which would now be in progress. It will be found that under the new conditions the viscous flows near the surface would still be away from high areas and toward low areas, but in general they would be slower than before. At greater depths, however, it will be found that the viscous flows would be undertows from regions of recent deposition toward regions of recent erosion. These undertow flows would in general tend to be in the direction opposite to recent surface transportation of material. This picture would serve as a first approximation to an understanding of the mechanism of isostatic readjustment. The undertows would be found on these assumptions to extend to a considerable depth, certainly more than 122 kilometers.

Next one should picture the changes in density which would be produced by the viscous flows. The density should be pictured as decreasing in regions from which material is being carried away by the flow and increasing in regions to which the material is being carried. It will be noticed as soon as such a picture is formed that every undertow flow at any level tends to equalize pressures at lower levels. This will have a strong tendency to make the prevailing undertows occur at much higher levels than they otherwise would.

Let it be assumed that the viscous material offers some small resistance to shear and still has elastic properties to a slight degree. The condition assumed originally that the pressure at a point depends simply upon the weight of the material above that point will be disturbed thereby. Form as clear a conception as possible of these disturbances and the modifications of the flows produced by them. I believe the modifications will be found to be important, and that they will be found to be such as tend to confine the effects of surface changes of load to a depth which is a small fraction of the radius.

So much for the direct effects of gravity which it seems important to picture clearly. Next study other effects, some of which are indirectly produced by gravity.

First study the modifying effects of changes of temperature. Wherever viscous flow takes place in the quasi-solid portions of the earth there heat is necessarily developed in amount equivalent to the mechanical energy expended in overcoming the resistance to flow. This will tend to increase the volume of the material, to increase the pressure, and to raise the surface above the region of viscous flow. It is probable also that the increase of temperature will tend to weaken the material, thus emphasizing the weakening produced by the damaging mechanical effects of the flow.

This temperature effect is probably locally important.

Beneath areas of recent deposition the temperature of a given part of the buried material will slowly increase for long periods of time, on account of heat conducted up from below and prevented by the new blanket of deposited material from rising to the surface so freely as before. Conversely, beneath the areas of recent erosion the temperature of a given portion of material will decrease. The ultimate limit of change will tend to be in each case not greater than about one degree Centigrade for each thirty-two meters of depth of erosion or deposition. These temperature changes tend ultimately to lower areas of recent erosion and to raise areas of recent deposition, possibly as much as one thirtieth of the thickness of the erosion or deposition,—the temperature effect taking place much later than the erosion or deposition which initiated it.

Study next the effects which may be computed from the bulk modulus of elasticity. Beneath areas of erosion a given particle of matter tends to rise by an amount which may be computed from the bulk modulus of material, and similarly a particle tends to fall beneath an area of deposition. If the depth to which the elastic phenomena extend is as great as 122 kilometers and the bulk modulus is 500,000 kilograms per square centimeter (corresponding to granite) the rise or fall of a particle near the surface will tend to be at least 1/50th part as great as the thickness of the material eroded or deposited. This is a change so large as to have considerable effects in modifying or magnifying the actions which would

otherwise occur. Possibly this elastic change is much larger than the estimate here given. Of course if the erosion or deposition takes place in a small area only, such elastic response will be largely inhibited by surrounding material on which the load has not been directly changed. But under large areas of erosion or deposition such action must take place and extend to depths possibly as great as 122 kilometers.

Study next the modifying effects, on the phenomena already pictured, of chemical changes which are probably produced in the earth by changes of pressure. The expression "chemical changes" is here used in the broadest possible sense. A relief of pressure at any given point in the earth necessarily favors such chemical changes as are accompanied by increase in volume and reduction of density. Increase of pressure tends to have the reverse effect. Such changes tend to reinforce and extend in time the effects just referred to which may be computed from the bulk modulus of elasticity. It is important to estimate such changes as well as possible from all available evidence, such for example as that furnished by chemists, by geologists, and by such investigations of rock formation as have been conducted at the geophysical laboratory in Washington. I believe the possible effects of this kind will be found to be so large as to be of primary importance.

Evidence has accumulated during the past few years which makes it reasonably certain that with increased pressure, as at the great depths in the earth, the rigidity and the viscosity of the material also necessarily increase. This tends to cause the viscous flows to take place at higher levels than they otherwise would. This should be taken into account.

Next a reëxamination of the conceptions so far formed should be made to ascertain to what extent and how they would be modified if one started with some other reasonable assumption as to the limiting depth of present isostatic compensation or some other reasonable assumption as to the law of distribution of the compensation with regard to depth.

Next full and extensive comparisons should be made between the hypothetical phenomena on the one hand pictured as made up primarily of viscous flows, modified by some elastic effects, ini-

tiated in part by surface transfers of load, modified by changes of temperature, modified by chemical changes and in the other ways, and on the other hand the facts of the past as to the behavior of the earth recorded in the rocks and read by geologists and others. This comparison should be used to the fullest possible extent to evaluate the relative importance of the various elements in the actions.

In making this comparison of various hypothetical phenomena with the great accumulated mass of geological facts it should be recognized at once that it is false logic to reason that if a given hypothesis does not account for all the observed facts the hypothesis is necessarily erroneous. On the contrary it is true logic in dealing with such a problem as the earth seen from a physical standpoint to reason that the more facts are accounted for by a given hypothesis the more certain it is that said hypothesis is a statement of a controlling element in the complex phenomena and then to study the facts which appear neutral, or conflicting, with reference to the hypothesis, considering them as indicators of other elements of the phenomena which one should attempt to embody in other supplementary hypotheses.

I submit that in studying the earth it is a mistake to think that there is any necessary conflict between the idea that the earth behaves as an elastic body and the idea that it is yielding in a viscous manner. A body may behave in both ways at once. The earth is probably acting largely as an elastic body under small forces which change rapidly and at the same time is yielding in a viscous manner to forces of larger intensity which are applied in one sense continuously for long periods.

The object of this address will have been accomplished if it serves in time to arouse the imagination and interest of some one and to guide him to greater effectiveness in attacking the problems presented by the earth as seen from the geophysical standpoint.

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MORPHOLOGY AND DEVELOPMENT OF AGARICUS RODMANI

(PLATES VII.-XIII.)

BY GEO. F. ATKINSON.

(*Read April 23, 1915.*)

INTRODUCTION.

Agaricus rodmani[1] was described by Peck in 1885, from specimens growing in "grassy ground and paved gutters" at Astoria, Long Island. As to its habitat and occurrence a more specific statement is made in 1897, in that it "grows in grassy ground and even in crevices of unused pavements and paved gutters in cities,"[2] from May to July, and is said to be rare. It has been observed in the city of Ithaca, N. Y., for a number of years, where it is usually found growing in the parking between the sidewalks and street curbing, or even in the crevices of stone paved streets and gutters, and also in grassy ground along the street railway or along walks on the border of groves. The material for this study was collected in August, 1914, along the Ithaca street railway and by the side of paths along the border of groves on the campus. In these places the mycelium in spots was often very abundant so that lumps of soil resembling a fine quality of spawn were exposed in digging for the young stages. The young fruit bodies collected were scattered on these cords of mycelium, the material and conditions offering very clear evidence of the normal development of the basidiocarps. The material was fixed in chrom-acetic fluid and sectioned in paraffin.

The features of interest in the morphology and development of *Agaricus rodmani* which I have considered in the present study are as follows: (1) the duplex character of the annulus, or ring, on the stem, and its significance; (2) the origin of the hymenophore funda-

[1] N. Y. State Mus. Nat. Hist. Rept., 36, 45, 1885.
[2] N. Y. State Mus. Nat. Hist. Rept., 48, 139, 1897.

ment; (3) the differentiation of parts in the primordial ground tissue; and (4) the origin and development of the lamellæ. The peculiar form and position of the annulus on the stem has suggested a resemblance to a volva, a structure not admitted in the genus *Agaricus* as now limited; while the subject of the origin and development of the lamellæ has acquired new interest in all of the Agaricaceæ since the accuracy of observations and the correctness of the statements covering a period of more than a half a century, in regard to this topic, have recently been called in question. Without further preliminary remarks we may proceed to an account of the present investigation, and to a consideration of the various matters involved.

I. The Duplex Annulus and its Significance.

The Annulus.—The annulus is situated near the middle of the short stem, or even near its base. It is usually very thick next the stem and is divided into an upper and lower limb by a deep marginal groove as is clearly seen in the photographs reproduced in Plate I. In those cases where the annulus is near the base of the stem, Peck was impressed by its suggestion of "the idea of a volva" (*l. c.*, 45). Before the expansion of the pileus, while the veil is still attached to the stem and pileus margin, a longitudinal section of the plant shows very clearly that the lower limb of the annulus lies on the outer (upper) side of the pileus margin (see Plate VII., upper right hand and lower left hand figures). The marginal veil is very thick and the epinastic growth of the pileus margin crowds the latter into the veil tissue and against the stem. The position of the lower limb of the annulus therefore corresponds to that of the volva limb of the *Amanitas*.

The plates represented in the upper group of Plate VII., were collected on the Cornell University campus, those in the upper group during August, 1911, along a path in the edge of a small wood not far from the street; those in the lower group, July, 1913, along the street railway and parking by East Avenue. In the expanded specimens, the pileus ranged from 6 cm. to 8 cm. in diameter. The plants were smaller than those represented in Plate VIII., but since they were abundant and in all stages of development they present in

an excellent way the different details of the veil and annulus during expansion of the plant. Those represented in Plate VIII., were collected by Mr. Wood, June 28, 1915, in the parking between the sidewalk and street, on Stewart Avenue, in front of the Town and Gown Club, Ithaca, N. Y. They were very robust specimens, and show the great distance between the upper and lower limb of the annulus. They are reproduced here real size.

A thin outer layer of the lower limb of the annulus is continuous below with the outer layer of the stem, and also with a very thin surface layer of the pileus. As the stem elongates at the time of the expansion of the plant, this outer layer of the stem lags behind and is thus torn into irregular patches shown very clearly in the two upper left-hand figures of Plate VII. The edges of these patches are frequently warped away from the stem, thus showing a tendency to exfoliation. This is especially marked in the case of the surface layer of the stem next the lower limb of the annulus. The warping upward of this layer, after it has been severed from its connection below, often gives the appearance of a double edge to the lower limb of the annulus, as shown in the lower right-hand figure of Plate VII., where the upper limb of the annulus has not yet broken away from the pileus margin.

The very thin layer on the pileus which is also continuous with a thin outer layer of the lower limb of the annulus often shows a tendency to exfoliation. This partial exfoliation of the stem and pileus surface is clearly marked where the basidiocarps are somewhat soiled by contact with particles of earth, as they are likely to be during the period of subterranean growth.

The outer portion of the lower limb of the annulus, as well as the corresponding thin, and partially exfoliating surface layer of the pileus and stem are derived from the outer layer of the blematogen. The blematogen layer, as I have interpreted it, is present in the genus *Agaricus* as well as in *Amanita*. In the species of *Amanita* thus far studied,[8] the blematogen at length is clearly separated from the pileus by a cleavage layer, arising from the gelatinization, or other kind of disintegration, of the external layer of the pileus primordium, thus

[8] Atkinson, Geo. F., "The Development of *Amanitopsis vaginata*," *Ann. Myc.*, 12, 369–392, pls. 17–19, 1914.

giving rise to the teleoblem, or finished volva. But in the genus *Agaricus*[4] no such cleavage layer is formed, and the surface of the pileus primordium becomes consolidated with the blematogen layer which here does not form a true volva, or teleoblem.

The lower limb of the annulus of *Agaricus rodmani* is not, therefore, strictly homologous with the volva of the *Amanitas,* not even including the thin layer of the stem and pileus which sometimes tends to peel off, since it does not comprise all of the blematogen layer, nor is it separated from the pileus by a distinct cleavage layer. If it were homologous with the volva of the *Amanitas,* then this species would represent a generic type distinct from *Agaricus* (*Psalliota*). In fact other species of *Agaricus* frequently show a similar condition of the annulus, *i. e.,* where the margin is "grooved," due to the inset of the pileus margin into the veil where the conditions for the robust development of the veil are favorable. In *Agaricus campestris* the annulus frequenlty presents a grooved margin, not only in the case of cultivated forms, but more rarely in the feral state. This condition is well shown in Plates 11 and 12 of my article on *Agaricus campestris.*[5] In Fig. 20 of that article the lower limb of the annulus has broken away from the outer surface of the incurved pileus margin, while the upper limb is still attached to the edge of the pileus. In Figs. 18 and 19 the upper limb has also become freed from the pileus margin and the grooved character of the edge of the annulus is very distinctly shown. In Fig. 15 of the same article, sections of the young basidiocarps show very clearly the position of the lower limb of the annulus extending over the outer (upper) side of the pileus margin. Fig. 20 also shows very clearly that the annulus as a whole is ripped off from the lower part of the stem, being an exaggerated case of the slight peeling up of the thin surface layer of the stem mentioned above in *Agaricus rodmani.* That the

[4] Atkinson, Geo. F., "The Development of *Agaricus arvensis* and *A. comtulus,*" *Am. Jour. Bot.,* 1, 3–22, pls. 1, 2, 1914.

Atkinson, Geo. F., "Homology of the Universal Veil in *Agaricus,*" *Myc. Centralb.,* 5, 13–19, pls. 1–3, 1914.

Atkinson, Geo. F., "The Development of *Lepiota clypeolaria, Ann. Myc.,* 12, 346–356, pls. 13–16, 1914.

[5] Atkinson, Geo. F., "The Development of *Agaricus campestris,*" *Bot. Gaz.,* 42: 241–264, pls. 7–12, 1906.

lower limb of the annulus in *A. rodmani* is merely a part of the marginal veil is clearly seen in the sectioned plants shown in the lower groups of Plate VII., where the connecting portion between the two limbs is clearly differentiated from the surface of the stem with which it is in contact, a situation very different from that in *Amanita* where the volva has no such relation to the annulus.

Comparison of Agaricus rodmani with other Species of Agaricus. —This extensive peeling, or ripping upward of the annulus from the lower part of the stem in *Agaricus campestris* is the cause of the more extensive, *i. e.*, broader, veil and annulus than is characteristic for *Agaricus rodmani.* Peck regards this species as intermediate between *Agaricus campestris* and *A. arvensis,*[6] resembling the former in size, shape and general appearance; the latter in the "whitish primary color of the lamellæ," in the occasional yellowish tints of the pileus, and the occasional rimose under surface of the annulus. The robust character of the annulus of *Agaricus rodmani* and the thick flesh of the pileus margin crowded by epinastic growth against the stem deepens and widens the groove on the edge of the annulus. This, together with the very short stem, in comparison with the longer stem of *Agaricus campestris* and *A. arvensis,* is, I think, largely responsible for certain differences in the character of the under surface of the annulus in the different species. In the species with the longer stem more stretching of the stem occurs and the annulus (or veil) is ripped upward from a greater extent of the stem surface. The radiately grooved character of the under surface of the annulus, in certain species (*A. arvensis* Schultz, *A. abruptibulbus* Pk., *A. placomyces* Pk., *A. hæmorrhoidarius* Schultz), or the coarsely floccose or scaly character in certain others (*Agaricus subrufescens* Pk., *A. augustus* Fr., or both features contained in some) is largely due to the fact that this part of the annulus is stripped from the stem and then brought under greater tension than the upper surface as the expansion of the pileus stretches the veil outward. All things considered *Agaricus rodmani* is much more closely related to *Agaricus campestris* than to any other of the species. It is very probably identical with *Agaricus campestris* var.

[6] N. Y. State Mus. Nat. Hist. Rept., 36, 45, 1885.

edulis Vitt.,[7] as I have elsewhere suggested[8] (1900, 1901, 1903, p. 20). Excellent figures of this variety are given by Vittadini (*l. c.*, pl. 6) and by Bresadola[9] (pl. 54).

II. Origin of the Hymenophore Primoridum

Primordium of the Basidiocarp.—The primordia of the basidiocarps are elliptical or oval in outline, and reach a diameter of 3 mm. or 4 mm. before there is any internal evidence of a differentiation of parts. The length is usually somewhat greater than the transverse diameter. In specimens not so well nourished differentiation may begin before the primordia have reached this size. The primordium, from the size of 2 mm. to 4 mm. in diameter, consists of a homogeneous interlacing of stout mycelial threads with rather thick walls. In primordia 3 mm. to 4 mm. in diameter the hyphae average about 5 μ to 7 μ in thickness, occasionally stouter ones are seen which measure up to 10 μ. More slender threads are also intermingled, but all sizes are so indiscriminately interwoven that no structural differentiation is perceptible. In smaller primordia the hyphæ average less in diameter. In most of the primordia examined, the sections are evenly stained throughout, but in a few a narrow zone a short distance from the surface stains more deeply than the external and internal tissue (Fig. 2). This suggested the possibility of a differentiation of an outer zone distinct from the bulk of the fruit body, which is sometimes present in *Agaricus campestris* and which I have called the *protoblem.*[10] A similar zone is found in some of the basidiocarps after the origin of the hymenophore fundament, but in the material which I have examined it is the exception rather than the rule, and I am inclined to the belief that it is due to some condition which affects the rate of growth or increase of cer-

[7] Vittadini, C., "Funghi Mangerecci," 44, 1835.

[8] Atkinson, Geo. F., "Studies of American Fungi; Mushrooms, Edible, Poisonous, etc.," 1st edition, I–VI., 1–275, 76 plates (223 figs.), Ithaca, N Y., 1900. *Idem,* 2d edition, I–VI., 1–322, 86 plates (250 figs.), Ithaca, N. Y. 1901. *Idem,* New York City, 1903.

[9] Bresadola, G., "Funghi Mangerecci e Velenosi," 1899.

[10] Atkinson, Geo. F., "The Development of *Agaricus arvensis* and *A. comtulus,*" *Am. Jour. Bot.,* 1, 3–22, pls. 1, 2, 1914. "Homology of the Universal Veil in *Agaricus,*" *Myc. Centralb.,* 5, 13–19, pls. 1–3, 1914.

tain individuals. A protoblem[11] is very likely present, but it is difficult to distinguish in primordia havng a subterranean origin because of the ease with which the delicate protoblem is removed while removing the soil, and especially in the forms and species of *Agaricus* with a white pileus. In those with a brown pileus, like *Agaricus campestris* var. *bohemia* of the commercial spawn growers, the delicate, white protoblem is very distinct.

Differentiation of an Internal Annular Hymenophore Primordium.—The first evidence of internal differentiation is the appearance of an internal annular zone of new growth in the region of the smaller end of the oval fruit body. This can be studied with advantage by means of serial, longitudinal sections. A median longitudinal section is shown in Fig. 3, while a "tangential" section, *i. e.*, parallel with the axis of the basidiocarp, but through one side of the annular zone of new growth is shown in Fig. 4. Diagrams 1 and 2 (in the text) show how the sections were made. Fig. 3 is from the region marked by the line 2, while Fig. 4 is from that marked by the lines 1 and 3. The darker staining areas in Figs. 3 and 4 mark the position of the zone of new growth. In the median

[11] The delicate, floccose, primary universal veil, or protoblem was observed by Fries on *Agaricus campestris* and a few other species, and called by him a subuniversal veil. Vittadini (in Fung. Mang., 147, pl. 18, fig. 2, 1835) describes and figures it in connection with his study of the development of his *Agaricus exquisitus*. But in this species he seems to confuse this delicate universal veil (protoblem) with what he terms the volva in several species of *Agaricus*. He also applies the term volva to the lower limb of the annulus in *Agaricus exquisitus* and in *Agaricus edulis*. He says (*l. c.*, 148) this delicate universal veil in *A. exquisitus* is perfectly similar to that which constitutes the veil of the "Tignose," *i. e.*, the scaly Amanitas like *A. muscaria*. etc. Vittadini also states (*l. c.* 147) that Trattinnick observed this delicate universal veil (protoblem) on *Agaricus edulis* (the species which Trattinnick describes as *A. edulis* is different from *A. campestris edulis* Vitt. or *A. rodmani* Pk.), but it appears that Vittadini misinterpreted Trattinnick's statment. The latter says, in order to prevent confusion one should avoid (*l. c.*, p. 73) taking for the edible one a mushroom (74), which may have also only the slightest trace of a membrane which in youth envelopes the entire mushroom, including pileus and stem, down to the roots. "Um Verwechslungen. zu vermeiden, hüte man sich statt der Gugemuke einen Schwamm zu nehmen" (73), "(*d*) der auch nur die geringste Spur von einer Wulsthaut haben sollte, die in der Jugend den ganzen Schwamm mit sammt den Strunk und Hut bis auf die Wurzel verhüllet" (74 Die essbare Schwämme, 1830).

longitudinal section two such areas are seen, symmetrically situated on either side of the long axis and some distance from the surface of the fruit body. The annular zone is of quite limited extent as the

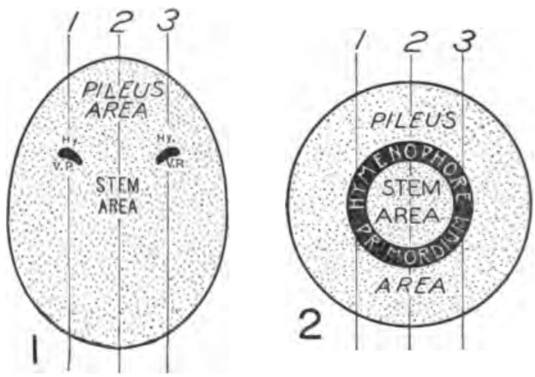


DIAGRAM 1 Lateral view through young basidiocarp representing early stage of differentiation into the primordia of the four principal parts; pileus area, stem area, hymenophore fundament (*Hy*) and veil primordium (*V. P.*).

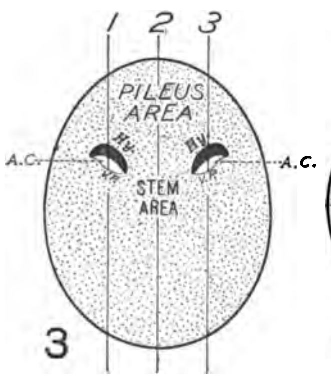
DIAGRAM 2. Zenith view in young basidiocarp at same stage of fundaments, and annular hymenophore primordium. See text for details.

small area presented by its transection in Fig. 3 shows. The outline of this area in transection is somewhat elongated and rises at an oblique angle from the stem area, well shown in Fig. 3 and indicated in diagram 1. The area of the primordial hymenophore seen in the tangential section is much more extensive as shown in Fig. 4. The difference in the extent of these areas shown in median (Fig. 3) and tangential (Fig. 4) sections is clearly appreciated by reference to diagram 2.

Structure of the Young Hymenophore Primordium.—This internal annular zone of new growth arises by the origin of numerous. slender hyphal branches, rich in protoplasm, which are directed downward, or obliquely downward and outward. They have a more direct course than the hyphae of the basidiocarp primordium, the latter irregularly sinuous and interwoven, while the hyphae of the young hymenophore primordium are nearly or quite straight. Because of their small diameter and their slender, gradually tapering ends, they easily crowd their way through the rather open weft of hyphæ forming the ground tissue or fundamental plectenchyma. Fig. 9 is a highly magnified view of the hymenophore primordium

shown in the section represented in Fig. 3, from the right-hand area. The dark area in Fig. 9 represents the mass of deeply stained hyphæ of the new growth zone. Because of the compactness of the tissue, very little detail is shown. But along the middle portion of the figure between the lighter, open mesh of the ground tissue below and to the right, and the dark area of the hymenophore primordium above and to the left, a number of hyphæ in advance of the others are shown extending into the loose mesh of the ground tissue. These are nearly parallel and their extremities are more or less distant, because they are in advance of the greater number of new branches present in the more deeply staining area. No annular gill cavity is present at this time.

Growth and Increase of the Hymenophore Primordium.—The growth and further organization of the hymenophore primordium is readily studied by the aid of similar serial sections of successively older stages of the basidiocarps. Sections of such stages are repre-



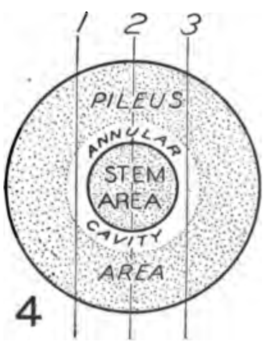


Diagram 3. Lateral view through young basidiocarp at a slightly later stage of development than in diagram 1. *Hy* = hymenophore; *A. C.* = annular cavity; *V. P.* = veil primordium.

Diagram 4. Zenith view in young basidiocarp at same stage of development. See text for details.

sented in Figs. 5–8 and 10–16. Diagrams 3 and 4 indicate how the sections were made. From the condition show in Figs. 3, 4 and 9, there is a rapid increase in the number of hyphæ in the zone of new growth, extending in the same direction, *i. e.*, downward and obliquely outward. During the increase in number the hyphæ become more crowded, are straighter and lie more nearly parallel. The

upper outer portion of this new zone of growth, *i. e.*, the hymenophore primordium, represents the early stage of the organization of the pileus margin: in other words, the annular internal zone of new growth is to be interpreted as the young primordium of hymenophore and pileus margin, the latter including the area from which the new hyphal branches arise as well as the basal area of these branches. Not only is there interstitial growth in the increase of these hyphal branches, the new ones crowding in between the older ones forming a more compact zone, but there is also a centrifugal increase in the periphery of the annular zone. The centrifugal growth of the pileus margin and hymenophore primordium is very characteristic.

The position and direction of the hyphæ of the young hymenophore primordium, as well as the increasing density of this area, is well shown in Figs. 10–16. The stem axis of all the figures is parallel with the long axis of the Plate. Several of these figures are highly magnified views of the hymenophore primordium shown in Figs. 5–7; Figs. 10 and 15 being highly magnified views of the hymenophore of Figs. 5 and 6, while Figs. 12 and 16 are highly magnified views of that in Figs. 7 and 8. Figs. 10 to 14 are from median longitudinal sections of the basidiocarps. Fig. 10 is from the right-hand side of the stem axis, *i. e.*, the stem axis is at the left. Figs. 11–14 are from the left-hand side of the stem axis, the stem axis therefore being on the right-hand of the figures. The increasing density of the elements of the young hymenophore is progressively shown in Figs. 10 to 13. With the increasing density the ends of the hyphæ reach more and more to the same level and thus tend to form an even surface which forms the transition to the palisade layer.

Origin of the General Annular Gill Cavity.—A striking feature in all these radial transections of the hymenophore zone and pileus margin is the curved outline of the zone as seen in transection. This is remarkably strong in Figs. 11 and 12 because the young hymenophore primordium extends for a considerable distance down around the apex of the stem fundament. This arched form of the young annular hymenophore zone is the result of epinastic growth of the pileus margin, which is very marked even in this very early stage in the organization. The rapid increase in the number of

the hyphæ in the young hymenophore, crowding in between the older ones, as well as their increase in diameter, produces a great pressure in this region. As a result of this increasing pressure within the arch a strong tension is exerted on the ground tissue below and adjacent to the arch. The ground tissue at this point is thus torn apart, forming a distinct opening, or cavity, beneath the young hymenophore, which is known as the annular gill cavity. The continuity as a general, annular, internal cavity can easily be determined by serial longitudinal sections through the young fruit body, the sections being made as indicated in diagrams 3 and 4, the knife travelling through the basidiocarp in the direction indicated by the lines 1, 2, 3. As the knife passes the region marked by the line 1, the sections will show a single cavity elongated transversely as shown in Figs. 6 and 8, 15 and 16. As the knife passes into the stem area the sections will show two cavities situated symmetrically as in Figs. 5 and 7 (or as in diagrams 3 and 4). Then as the knife passes out of the stem area, into the region indicated by the line 3, the sections will again show a single cavity elongated transversely.

The annular gill cavity[12] varies in strength in different individuals and at different stages of development. Sometimes it is very weak, at other times it is quite strong. The tearing apart of the ground tissue often leaves it with quite an open mesh, and the surface next the gill cavity is more or less frazzled. The gill cavity is stronger next the stem where the hymenophore is older, and is weaker toward the margin. Where the cavity is weak, isolated threads or irregular strands of the ground tissue are not completely torn away from the hymenophore, and the cavity is thus often traversed by lagging elements of the ground tissue. At a later stage, after the origin of the lamellæ, the annular cavity in some indi-

[12] In a recent paper, after describing the gills in *Coprinus micaceus*, Levine ("The Origin and Development of the Lamellæ in *Coprinus micaceus*," *Am. Jour. Bot.*, 1, 343–356, pls. 39, 40, 1914), makes the statement (p. 352) that "There is no general gill cavity as described by Hoffmann, deBary, Atkinson, and others." Since deBary ("Morphologie und Physiologie der Pilze, Flechten und Myxomyceten," 69, 1866) is the only person hitherto who has announced the presence of a general annular gill cavity in *Coprinus micaceus*, this statement by Levine can only be interpreted as a general denial of the presence of a general annular gill cavity in the species in which it has thus far been described, a rather rash statement which will be referred to again in the discussion of the origin of the lamellæ.

viduals may become nearly or quite closed by the increase in the elements of this ground tissue, which forms a portion of the marginal veil, but chiefly by the epinastic growth of the pileus margin which crowds this ground tissue up against the margin of the lamellæ, as shown in Figs. 32–38.

Organization of the Palisade Layer.—The level palisade layer of the hymenophore follows the primordial stage, immediately after the latter stage has become dense and compact by the increase in number and thickness of the parallel hyphal elements. The growing compactness of the primordial hymenophore zone is accompanied by the evening up of the hyphal ends into a plane surface. As the ends of the hyphæ broaden the free surface of the hymenophore becomes compact and smooth, or even. This is the level palisade stage of the hymenophore. It is a gradual, not abrupt, transition from the primordial stage. It begins next the stem, or in many cases on the outer surface of the upper part of the stem fundament as shown in Fig. 12. Here the palisade area, in radial section, rises upward at a strong oblique angle from the axis of the stem, and then grades into the primordial area toward the left. The palisade area progresses, like the primordial area and the pileus margin, in a centrifugal direction, the older portion lying next to, or on the upper part of the stem fundament.

The level palisade layer of the hymenophore, preceding the origin of the lamellæ, was first described by Hoffmann[13] in 1856, 1860, and 1861, in about a dozen species (see the later paragraph on the origin of the lamellæ for a list of species). DeBary[14] (1859, p. 386, 394) described the palisade layer of the young hymenophore in *Nyctalis asterophora* and *parasitica,* as having radial folds from its

[13] Hoffmann, H., "Die Pollinarien und Spermatien von Agaricus," *Bot. Zeit.*, 14: 137–148; 153–163, pl. 5, 1856. Beiträge zur Entwickelungsgeschichte und Anatomie der Agaricinen," *Bot. Zeit.*, 18: 389–395; 397–404, pls. 13, 14, 1860. Icones Analyticae Fungorum; Abbildungen und Beschreibungen von Pilzen mit besonderer Rucksicht auf Anatomie und Entwickelungsgeschichte," 1–105, pls. 1–24, 1861.

[14] DeBary, A., "Zur Kenntnis einer Agaricinen," *Bot. Zeit.*, 17: 385–388; 393–398; 401–404, pl. 13, 1859.

[15] DeBary; A., "Morphologie und Physiologie der Pilze, Flechten und Myxomyceten," Leipzig, 1866. "Vergleichende Morphologie und Biologie der Pilze, Mycetozoen und Bacterien," 1884. "Comparative Morphology and Biology of the Fungi, Mycetozoa and Bacteria," Oxford, 1887.

earliest appearance. But as this interpretation was shown by Hoffman (1860, p. 402) to be wrong, deBary[15] (1866, p. 63; 1884, p. 58, 312; 1887, p. 55, 289) studied a number of other forms and agreed with Hoffman that the earliest stage of the young palisade hymenophore was level, or smooth.

III. The Differentiation of Parts in the Primordial Ground Tissue.

There are four principal parts of the fruit body which are differentiated in the ground tissue of the basidiocarp primordium, the *hymenophore, pileus, stem* and *veil.* The primary differentiation in the ground tissue of *Agaricus rodmani* is the origin of the hymenophore primordium. As described above this arises as an internal annular zone of new growth, a little above the middle of the small oval primordial basidiocarp. It consists of numerous hyphal branches which extend downward and obliquely outward. These new hyphæ are nearly or quite parallel, are at first slender and taper very gradually to the free end. This form assists them in making their way through the mesh of the ground tissue. They are rich in protoplasm, become compacted by increase in number and diameter, and thus in sections, take on a deep color when stains are applied (see Figs. 3–16). The origin of this internal hymenophore zone differentiates at once the stem and pileus areas, or fundaments, but the organization of the stem and pileus occurs later.

In the early origin of the primordial hymenophore zone, *Agaricus rodmani* agrees with *Agaricus campestris*[16] as presented in a study of the commercial varieties, *alaska* and *bohemia.* In that paper I pointed out that we should not necessarily expect the first evidence of differentiation to be the appearance of the hymenophore primordium in plants not yet studied though it is probable that at least some of the other species of *Agaricus* (*Psalliota*) may show the same peculiarity. This suggestion is justified by the situation in *Agaricus rodmani.* The same situation exists in *Armillaria mellea.*[17]

[16] Atkinson, Geo. F., "The Development of *Agaricus compestris,*" *Bot.*
Universal Veil' in *Agaricus,*" *Myc. Centralb.*, 2, 13–19 pls. 1–3, 1914.
Gaz., 42: 241–264, pls. 7–12, 1906.

[17] Atkinson, Geo. F., "The Development of *Armillaria mellea,*" *Myc. Centralb.*, 4: 113–121, pls. 1, 2, 1914.

In the specimens of *Agaricus arvensis*[18] studied, the lagging behind of the ground tissue below the zone where the hymenophore primordium arises occurs before any differentiation of this zone is distinguishable, for a light area with a looser mesh occurs in an annular zone which marks the distinction between the stem and pileus areas. Or the lagging behind of the ground tissue may occur simultaneously with the appearance of the primordial hymenophore zone and the outline of the pileus area. In a number of forms studied by Fayod[19] the primordium of the pileus is organized, in the apex of the young homogeneous basidiocarp, as a new zone of growth, in the form of an inverted bowl, shown by the darker staining of the hyphæ rich in protoplasm, forming a pileus producing layer ("couche piléogène"). This method of differentiation he accepts as a general law for the Agariceæ, the only exception admitted by him being the coriaceous forms of *Lentinus*. *Agaricus rodmani*, the commercial varieties of *Agaricus campestris* (*columbia* and *alaska*) and *Stropharia ambigua* (Peck) Zeller,[20] also form exceptions to this rule. The primordium of the pileus in these forms may be regarded as diffuse within the upper part of the young basidiocarp, the differentiation and organization of the pileus margin beginning in conjunction with the organization of the primordial hymenophore zone, though in *Stropharia ambigua* the inverted bowl-shaped zone of new growth in the upper part of the pileus area is soon organized.[20] Other forms recently investigated which conform to the general law laid down by Fayod, are certain species of *Hypholoma* (Allen),[21] *Hypholoma fascicularis* and *Clitocybe laccata* by Beer,[22] *Lepiota*[23] *clypeolaria* and *Amanitopsis vaginata*.[24]

18 Atkinson, Geo. F., "The Development of *Agaricus arvensis* and *A. comtulus*," *Am. Jour. Bot*, 1, 3–22, pls. 1, 2, 1914. "Homology of the 'Universal Veil' in *Agaricus*," *Myc. Centralbl.*, 2, 13–19 pls. 1–3, 1914.

19 Fayod, V., "Prodrome d'une histoire naturelle des Agaricinées," *Ann. Sci. Nat. Bot.*, VII., 9, 181–411, pls. 6, 7, 1889.

20 Zeller, S. M., "The Development of *Stropharia ambigua*," *Mycologia*, 6, 139–145: pls. 124, 125, 1914.

21 Allen, Caroline L., "The Development of some Species of *Hypholoma*," *Ann. Myc.*, 4, 387–394, pls. 5–7, 1906.

22 Beer, R., "Notes on the Development of the Carpophore in Some Agarincaceæ," *Ann. Bot.*, 25^2: 683–689, pl. 52, 1911.

23 Atkinson, Geo. F., "The Development of *Lepiota clypeolaria*," *Ann. Myc.*, 12, 346–356, pls. 13–16, 1914.

Organization of the Pileus.—The organization of the pileus begins in connection with the primordial hymenophore zone. The upper part of this zone is very probably to be regarded as the primordium of the pileus margin which then increases by centrifugal growth. It is marked from an early period by strong epinastic growth, so the margin becomes strikingly involute, a feature also characteristic of *Agaricus campestris*,[25] *A. arvensis*,[26] *A. comtulus*, etc., as I have earlier described. The general relation of the hyphæ in the primordium of the pileus margin is a parallel one, and they become more and more strongly incurved as a result of epinasty. As the pileus primordium increases in width by marginal growth, it also increases in thickness, more perceptibly so farther back from the margin where the new growth is older. In this way the organization of the pileus advances more and more into the outer zone of the ground tissue, the blematogen, and becomes consolidated with it.[27]

Organization of the Stem.—The stem area is delimited at the same time as the pileus area by the origin of the young hymenophore zone, but its organization and differentiation from the ground tissue seems to lag behind the early stages of the organization of the pileus margin. While a general and more or less diffuse growth and expansion occurs for some time in the stem area, the first evidence of a differentiation from the ground tissue is seen in the organization of the stem surface. The outline of the stem may be compared to that of a broad, flat cone, since the stem at first is very short and

[24] Atkinson, Geo. F., "The Development of *Amanitopsis vaginata*," *Ann. Myc.*, 12, 369–392, pls. 17–19, 1914.

[25] Atkinson, Geo. F., "The Development of *Agaricus campestris*," *Bot. Gaz.*, 42: 241–264, pls. 7–12, 1906 (see figures 11 and 12).

[26] Atkinson, Geo. F., "The Development of *Agaricus arvensis* and *A. comtulus*." *Am. Jour. Bot.*, 1: 3–22, pls. 1, 2, 1914.

[27] In *Agaricus campestris* var. *edulis*, Vittadini ("Fun. Mang.," 44, pl. 6, fig. 1, 1835) in a young oval fruit body, figures and describes the outline of the pileus within a stout volva, and states that, during the course of development, the volva is ruptured circularly, and the margin of the pileus as it emerges is held for a time against the stem by the lower limb of the annulus. His account of the release of the volva (blematogen) from the pileus does not seem clear, and his figures do not show the transition stage from *a* to *b* in figure 1 of his Plate VI In *Agaricus rodmani* nor in any other species of *Agaricus* (*Psalliota*) have I ever seen any indication of the clear cut outline of the pileus surface as distinct from the blematogen, such as Vittadini shows at *a*, fig. 1.

broad, and the surface slopes outward at a strong angle. The surface outline of the stem is quite clearly differentiated from the loose ground tissue forming the marginal veil, because of the deeper staining property of the stem shown in longitudinal sections (Fig. 32). Its differentiation and organization agrees entirely with that described for *Agaricus campestris*,[28] *Agaricus arvensis* and *A. comtulus*.[29]

Organization of the Marginal Veil.—The organization and limits of the marginal veil, or partial veil, as it is sometimes called, in *Agaricus arvensis, A. comtulus* and *A. campestris,* has been very fully discussed in previous papers[29] (13–15, 1914), briefly in another[30] (17, 1914). Its organization and composition in *Agaricus rodmani* is in the main similar, its different features being due to its more robust character, the stouter pileus and shorter stem. The fundament of the marginal veil is ground tissue in the angle between the primordial hymenophore zone and the stem fundament, including on its outer surface a narrow section of the blematogen layer. The ground tissue in this angle is indicated in *VP* (veil primordium) in diagram 3, and the corresponding areas in Figs. 3, 5, 7, 9–14 can readily be understood. There is considerable increase in this ground tissue by growth of the portion clothing the stem fundament. It is also added to by growth of the hyphæ at the margin of the pileus. The mass of the loose inner surface is often crowded up against the edges of the gills by the involute margin of the pileus pushing it upward, due to epinastic growth.

In such robust specimens usually presented by *Agaricus rodmani* the blematogen layer is comparatively thick but still forms a comparatively small portion of the marginal veil, and lies on the outer under surface of the lower limb of the annulus. By the incurving of the thick margin of the pileus its edge is crowded into the thick veil, and presses against the stem, thus separating the veil, which later becomes the annulus, into an upper and lower limb. As stated above, the fact that the short stem elongates but little in comparison

[28] Atkinson, Geo. F., "The Development of *Agaricus campestris*," *Bot. Gaz.*, 42: 241–264, pls. 7–12, 1906.

[29] Atkinson, Geo. F., "The Development of *Agaricus arvensis* and *A. comtulus*," *Am. Jour. Bot.*, 1. 3–22 pls. 1, 2, 1914.

[30] Atkinson, Geo. F., "Homology of the Universal Veil in *Agaricus*, *Myc. Cantralb.*, 5, 13–19, pls. 1–13, 1914.

with that of *Agaricus campestris, arvensis,* and a number of other species, the veil is usually not ripped up from the lower part of the stem as it is in the other species. A thin layer on the stem below the annulus is often cracked into distinct areas or patches, the margins of the areas sometimes being partially exfoliated. The partial exfoliation of the under part of the lower limb of the annulus frequently occurs, and then the lower limb itself has a double edge as described above, and as shown in several of the figures of Plate I. In *Agaricus campestris, arvensis, augustus, subrufescens, placomyces,* and others, the freeing of the lower part of the annulus from the stem is very extensive, since as the stem elongates the veil is ripped off for a considerable distance. In *Agaricus rodmani,* as the pileus expands, the lower limb of the veil clings to the stem, splitting off from the outer surface of the pileus margin as the latter is withdrawn. The inner or upper limb of the veil remains attached to the edge of the pileus margin for a longer time, but is eventually separated.

IV. Origin and Development of the Lamellæ.

Origin of the Gill Salients.—The development of the hymenophore is progressive and centrifugal. As described in the previous section, the primordial hymenophore zone originates in conjunction with the primordium of the pileus margin and lies in the angle separating the stem and pileus areas. The organization of the level palisade zone of the hymenophore from the primordial stage, begins in the older region, *i. e.,* next the stem. The margin of the pileus, primordial hymenophore and palisade zone all progress by growth in a centrifugal direction, the younger, later stages succeeding the earlier. The lamellæ succeed the level palisade zone and arise as downward growing salients of the same. These salients begin next the stem (or in some cases on it). They are regularly spaced and progress in a radial, centrifugal direction. The origin of the salients from the level palisade stage is well shown in Figs. 17–21.

In Figs. 18 and 20, different stages in the origin of the salients are shown. Three gill salients are seen in Fig. 20. At the left side of Fig. 20 is the level palisade. Next it to the right is a very low salient. Continuing to read toward the right, the second and third salients are successively stronger. While the hyphal struc-

ture is not very distinctly shown in in this figure, due to the difficulty of illumination which will produce on the photographic plate the same degree of resolution which can be detected by the eye, still the palisade character is evident. A similar situation is seen in Fig. 18, but the progression in the origin and growth of the salients is to be read from right to left. A somewhat later stage is shown in Fig. 19. Here the hyphal structure is well shown. The palisade character of the exposed surface of the hymenophore is very clearly shown. This figure gives us some suggestion of the factors operating in the formation of the gill salients. The elements of the palisade layer increase by interstitial growth, *i. e.*, by new branches which crowd in between the older ones. At the same time the elongate cells composing the palisade layer increase in diameter. In the primordial stage they passed from the terete tapering condition to the cylindrical form. Now they pass from the cylindrical to the clavate form, as well as increasing somewhat in diameter throughout. This produces a great pressure on the level palisade zone, which if continued, must result in throwing the level palisade layer into folds.

Another factor now comes into play which prevents the palisade layer from being thrown into a series of irregular folds. This is the downward growth, by elongation, of the subadjacent tramal hyphæ, along regularly spaced radial areas, beginning next the stem and proceeding in a centrifugal direction toward the margin of the pileus. These radial areas of subadjacent tramal hyphæ, elongating downwards, push the palisade area downward into corresponding radial salients. These salients are the first evidence of folds or ridges which appear in the young hymenophore. They are the gill salients, and by continued growth form the lamellæ themselves.

Fig. 19 presents another very interesting situation. This is the flaring, or fantailing, of the gill salients very soon after their emergence below the level of the general palisade surface. This is very clearly one of the first results of the release from the pressure to which the elongate cells were subject in the level palisade condition. Another still more interesting feature at this stage is the pressure to which the neutral portion of the level palisade is subjected as a result of this fantailing of the gill origins. The flanks of the young

gill salients thus crowd against the intervening neutral palisade cells, more strongly against their free ends. This presses these intervening, neutral, radiating areas of the original level palisade into the form of ridges which thus alternate with the radiating gill salients. These intervening ridges between the young gill salients are very conspicuous in a corresponding stage of gill development in *Coprinus micaceus* as I have shown in another paper. This situation is a comparatively old stage in the development of the lamellæ and is one of the peculiar features presented by a number of the Agaricaceæ, which led Levine[81] to mistake these intervening ridges between comparatively old gill salients for the first ridges to appear in the hymenophore primordium of *Coprinus micaceus*. These ridges he thought were the first evidence of the gills. The gills were described as arising from the splitting of these first ridges and the union of approximate halves of adjacent ridges to form the gills between them. This matter will be referred to below when another peculiar situation is described which also assisted in leading this author astray.

Relation of the Different Phases of Hymenophore Development in the Young Basidiocarp.—Figs. 17–23 represent different phases of the organization and development of the hymenophore in a single basidiocarp, during an intermediate stage of its development. The relation of these different phases is determined by a study of longitudinal serial sections passing from near the stem to the margin of the pileus. With the exception of Fig. 20, Figs. 17–23 are all from the same plant, selected to represent the relation of different phases of the young hymenophore. The sections from which the photographs were taken were parallel with the axis of the stem, and thus were nearly or quite perpendicular to the hymenophore, or under surface of the pileus. The general plane of the hymenophore, or under surface of the pileus, is slightly arched, but for all practical purposes of this study, the plane is perpendicular to the stem axis, so that the sections are perpendicular to the general hymenophore surface, or plane. Fig. 17 is from a section near the stem, corresponding to line 4 in diagram 6 (diagram 6 is intended to illustrate the situation presented by the figures in Plate 5, but serves to illus-

[81] Levine, M., "The Origin and Development of the Lamellæ in *Coprinus micaceus*," *Am. Jour. Bot.*, 1, 343–356, pls. 39, 40, 1914.

trate also the relations now under consideration). An examination of the relation of line 4, in diagram 6, to the gill salients, the palisade and primordial areas, will assist in making the relation of the phases of the hymenophore presented in Fig. 17 very clear.

In the middle of the figure, or section, the gill salients are cut transversely. On either side of the middle they are cut obliquely, the more so the nearer the palisade area the salients are cut. But when the gill is so young, the structure of an oblique section at this angle is practically the same as in a transection. Since the hymenophore is older next the stem, and progressively younger toward the margin of the pileus, the gill salients are older next the stem, and younger next the palisade area, where they are very low and grade off insensibly into the level palisade zone. Toward the left and right from the middle of such a section as is represented by Fig. 17, the salients become less and less prominent until they grade insensibly into the level palisade zone on either side. In like manner the palisade zone grades to the left and right into the primordial zone, and this into the margin of the pileus, showing practically the same relation, so far as the palisade and primordial zones are concerned, as in a radial section.

Fig. 21 is from a section made near the outer ends of the middle salients, about in the region represented by line 7 in diagram 6. Only a few salients are shown, these are very low, and on either side soon grade insensibly into the palisade zone. Fig. 22 is from a section made in the region indicated by line 8 in diagram 6. Here there are no gill salients (nor any evidence of ridges in the hymenophore), a broad area in the middle is the palisade area, and this grades on either side insensibly into the primoridal area. Fig. 23 is from a section made in the region indicated by line 9 in diagram 6. It is entirely within the primordial zone, near the margin of the pileus. Knowing this relation of the different phases of the hymenophore, one can observe the transition of the primordial phase into the level palisade phase, and this into the phase of the salients. In other words, one can study the method of origin of the lamellæ by a study of the different phases of the gill salients in the area of transition from the palisade zone into the zone of the young gills.

Relation of the Hymenophore to the Stem.—One of the taxonomic characters employed for the genus *Agaricus* (*Psalliota*) is the free condition of the gills from the stem. In *Agaricus campestris*, while the gills are usually free, they are close to the stem, and in some cases are even adnexed to the stem. The same is true of *Agaricus rodmani*. Peck[32] says of the lamellæ,—" free, reaching nearly or quite to the stem. It is possible that in some examples the gills may be broadly attached to the stem fundament at the time of their origin, but become free at maturity by changes in the relation and tensions of the parts during expansion of the plant. That the young lamellæ are sometimes broadly attached around the upper end of the stem fundament has been observed in a number of examples during this study of development. In some examples the attachment of the stem is very broad, in others slight, and in still others the lamellæ are free from the time of their origin.

Deceptive Appearance of Sections near the Stem when the Young Lamellæ are Attached.—In studying the origin of the lamellæ in plants where the hymenophore, from its earliest appearance, is entirely free from the stem, little difficulty is experienced in the interpretation of the situation presented, in case there is a fairly well formed annular cavity prior to the origin of the gill salients. Longitudinal sections next the stem then present the simple situation shown in Fig. 17. But in those cases where the hymenophore primordium extends downward on the outer surface of the stem apex, as shown in Figs. 11 and 12, sections passing from the stem through this portion of the hymenophore, after the origin of the gill salients, present a complicated structure, which may be very confusing unless all the features of the situation are taken into consideration. As stated above the stem axis of the sections from which Figs. 11 and 12 were made is parallel with the longitudinal direction of the plate. In very young basidiocarps, as already described, the stem surface slopes outward at a very strong angle as shown in Fig. 32.

Now, when the gill salients begin to form by downward, or outward, extension of the level palisade, in those cases where the hymenophore primordium extends down on the surface of the stem,

[32] Peck, C. H., N. Y. State Mus. Nat. Hist Rept., 36, 45, 1885.

the salients first appear over this portion of the hymenophore, because it is the older. The older portion of the salients, therefore, extend outward perpendicular to the stem surface. Since their progression is centrifugal, the salients gradually extend over the angle between stem and pileus where their growth is downward. Since the growth in width of the salients is perpendicular to the surface of the level hymenophore at any point, there are formed, in the cases

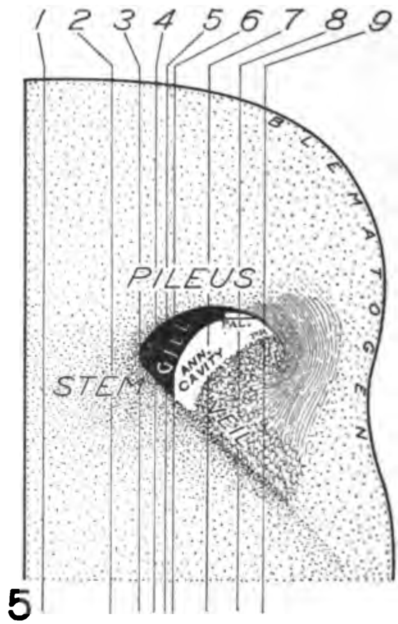


DIAGRAM 5. Lateral view through one half of a basidiocarp in an intermediate stage of development, showing (1) the strongly sloping surface of the stem; (2) the partly organized pileus margin which is becoming involute because of eipnastic growth; (3) the hymenophore presenting three stages of development, (*a*) the oldest portion, the gill area extending on the under side of the pileus and far down on the surface of the stem (adnate at this stage), (*b*) the palisade area (*PAL*) distal to the gill area on under side of pileus, and (*c*) the primordial area (*PR*) near margin of pileus; (4) annular cavity; (5) the loose ground tissue of the marginal veil; and (6) the blematogen layer. See text.

under consideration, a series of little stalls, or pigeon holes, around the stem apex, between the young gills in the angle between the stem

and pileus. This situation is illustrated in Figs. 24–31, from selected serial sections of the same basidiocarp. The sections were parallel with the long axis of the stem. Diagrams 5 and 6 illustrate the situation in this basidiocarp and show exactly how the scetions were made.

Fig. 24 is from a nearly median longitudinal section, made in the region indicated by line 1 of diagrams 5 and 6, which presents a situation practically the same as a median section. The outline of the narrow young gill salient is well shown in Fig. 24, with the distinct annular cavity. The gill salients are strongly curved and in the form of crescents, the lower limb of the crescent extending far down on the outwardly sloping stem surface; the upper limb reaching out on the under surface of the pileus, where it grades into the level palisade zone, and the latter into the primordial zone. The relation of parts is clearly represented by diagram 5. It is quite easy to form a mental picture of the series of little stalls, or pigeon holes, around the upper part of the stem between these crescentic salients.

Fig. 25 is from a section in the region indicated by line 2 of diagrams 5 and 6. The line 2 in diagram 6 shows how the section passes through the side of the stem and obliquely across a few of the young gills, then on either side passing through the level palisade and primordial zones. These features are clearly seen in Fig. 25. Fig. 26 is from the region indicated by line 3: Fig. 27 that of line 4; Fig. 28 that of line 5; Fig. 29 that of line 6; and Fig. 30 that of line 7, of diagrams 5 and 6 (figures of sections in the region indicated by lines 8 and 9 are not shown from this basidiocarp, but there is nothing essentially different in them from figures 22 and 23 from another plant). Fig. 31 is a more highly magnified view of the middle portion of Fig. 27.

Figs. 26–29 and 31 present a very interesting situation. They show transections of the stalls, or pigeon holes, mentioned above. Unless caution is observed this situation would be very misleading. The gill salients are attached above to the under side of the pileus and below to the surface of the stem, and this attachment above and below existed from the time of the origin of the salients. However, the attachment below is not that of the margin of the gills, but of their origin from the stem, since the salients grew outward from the

level palisade organized in this region over the upper surface of the stem.

Similar sections of *Coprinus micaceus*[33] through the region of the attached gills was one of the features contributing to the incorrect interpretation, by Levine, of the origin of the lamellæ in this plant, as shown by his Figs. 13 and 14. The palisade cells on the sides and in the upper angle of these pigeon holes could easily give the impression that the gills had their origin from isolated radial areas of new growth of palisade cells, these areas, or "ridges" of

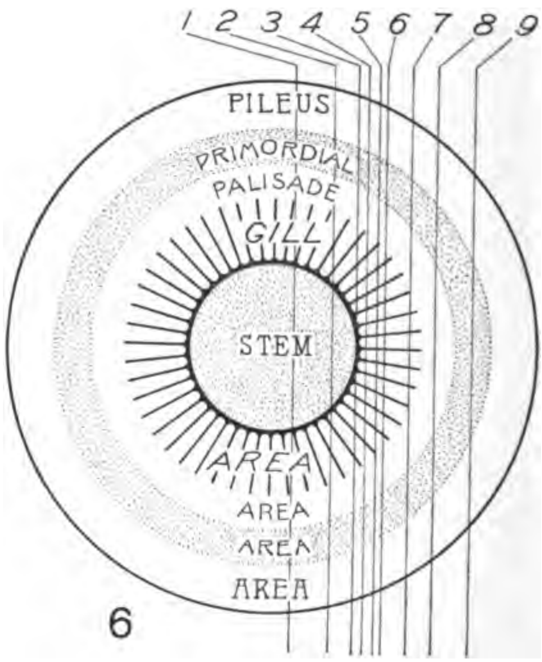


DIAGRAM 6. Zenith view in a basidiocarp of the same age as that represented in diagram 5. See text for details not marked in the diagram.

palisade cells parting as they increase, forming a lining over the ground tissue or partitions of these little stalls, and thus enclosing "the notch between the gills."

Relation of the Gills to the Involute Margin of the Pileus.—There are other peculiar situations presented in the development of

[33] Levine, M., "The Origin and Development of *Coprinus micaceus*," *Am. Jour. Bot.*, 1, 343–356, pls. 124, 125, 1914.

Agaricus rodmani (and other species) which may lead to serious misinterpretation unless great caution is observed. This is the relation of the gills to the involute margin of the pileus and to the marginal veil, shown in a series of longitudinal, "tangential" sections of basidiocarps at an age when the gill salients, by centrifugal progression, have nearly or quite reached the margin of the pileus. The various features of this situation are presented in Figs. 32–42. The figures are photographs of selected serial sections from a single basidiocarp. Diagrams 7 and 8 illustrate the situation in this basidiocarp and the lines show the regions in which the sections were made.

In Fig. 32, from a nearly median longitudinal section (in the region of line 1), the involute margin of the pileus is shown. An indefinite portion of the outer, lighter stained area is the blematogen. The margin of the pileus is so strongly involute that the edge is curved upward toward the gills and has crowded the mass of the ground tissue constituting the inner portion of the veil up against the middle zone of the lamellæ. The attachment of this ground tissue to the margin of the gills is not very firm, though there is some adherence of the hyphæ. The attachment has occurred after the ground tissue was crowded against the margins of the gills by the strongly upturned, involute pileus margin. The strongly involute margin of the pileus is well shown also in several of the figures in Plate VII. The position of the upturned edge of the involute pileus margin is such that the loose ground tissue of the inner portion of the veil is lifted up against the middle area of the lamellæ, while the edges of the gills near the stem and also near the margin of the pileus are free. This is very clearly shown in Fig. 33, from a section in the region of line 2 in diagrams 7 and 8.

Figs. 34 and 35 are from sections in the region of lines 3 and 4 just passing through the surface of the stem in the angle at the junction of the pileus and stem. The hymenophore extends a short distance down on the upper surface of the stem, but the gills are only "adnexed," not extending so far down on the stem fundament as in the basidiocarp represented on Plate XII. and in diagrams 5 and 6. In the middle area of Fig. 35, the nearly solid block of tissue in the same level with the gills on either side, is hymenophore tissue from

the surface of the stem, and a portion of the same area in Fig. 34 also belongs to the hymenophore. The hymenophore, as interpreted here, and in all of my recent papers, includes not only all parts of the lamellæ and the palisade cells between adjacent lamellæ, but also a thin, often indefinite zone of the subadjacent tissue corresponding to the subhymenial tissue of the palisade between the gill origins. As figure 35 shows, the "stalls," or "pigeon holes," in the angle of pileus and stem are quite small because the gill origins extend but

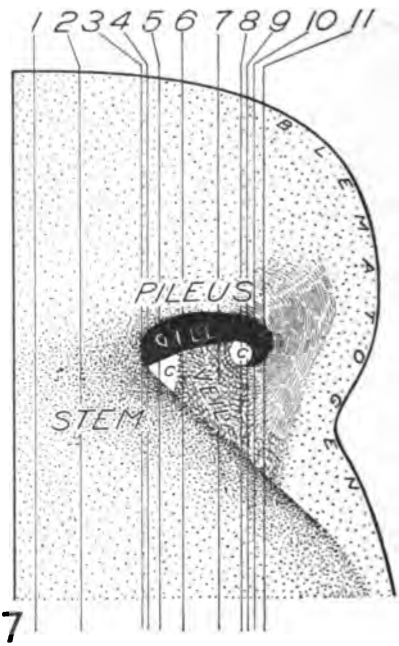


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DIAGRAM 7. Lateral view through one half of a basidiocarp in an older stage than that represented in diagrams 5 and 6. The hymenophore has all passed over into the gill stage. The gill area does not extend so far down on the stem as in diagram 5. The margin of the pileus is more strongly involute and the veil tissue has been crowded up against the middle portion of the gills. C = the portion of the annular cavity not filled. See text for other details not marked here.

a short distance down on the upper surface of the stem. The abrupt ending of this hymenophore tissue below is even with the margins of the gills on either side, and the lower edge is free from the ground

tissue clothing the stem fundament, as shown by the clear line between the two. This indicates that the portion of the hymenophore on the upper surface of the stem projected by growth slightly above the level of the stem surface, or above that of the ground tissue. In Fig. 34 the distinct boundary line of the more compact tissue shows, but it is in contact with the ground issue below since this section did not pass outside of the junction of stem and pileus fundaments. In Fig. 35 a few of the gills on either side of the middle are free from the ground tissue below. Outside of this on either side (the middle zone between stem and pileus margin) a number of the gills are attached to the ground tissue pressed up against them by the involute pileus margin. On either side of these areas, *i. e.*, near the margin of the pileus, the gills are free.

Fig. 36 is from a section in the region indicated by line 5 in diagram 7. The middle of the section, according to line 5, would pass through the space of the annular cavity near the stem which has not been filled by the upward crowding of the ground tissue. The margin of the gills here should therefore be free from the ground tissue below. This is shown to be the case in Fig. 36, for the gills over the middle portion of the figure (which are near the stem). On either side of this area, however, the section passes through the zone where the ground tissue is crowded up against the gills, while toward the margin of the pileus the gills are again free from the ground tissue.

Figs. 37 and 38 are from sections in the region of lines 6 and 7 respectively, of diagram 7. Both sections are thus "tangents" through the region where the ground tissue in contact with the middle zone of the gills would be continuous and of considerable extent, but the area in the region of line 6 would be of greater extent than that in the region of line 7. This corresponds with the situation shown in Figs. 37 and 38, while toward the margin of the pileus on either side the gills are free. Figs. 39 and 40 are from the region of lines 8 and 9. These pass through the portion of the annular cavity between the margin of the pileus and the ground tissue crowded up against the middle region of the hymenophore. The gills therefore would not be in contact with the ground tissue below. In Figs. 39 and 40, however, it is clear that on either side the gills

are attached below as well as above. The attachment below is not the margin of these gills, but their point of origin from the inner surface of the involute pileus margin. This will be clearly understood from a study of Figs. 41 and 42.

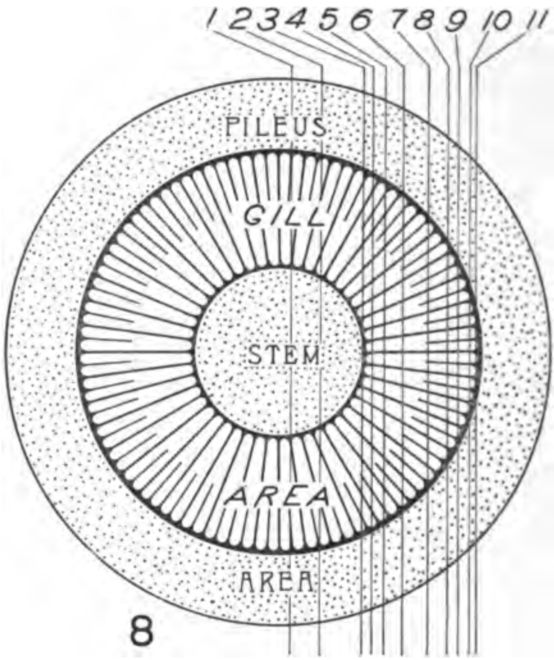


DIAGRAM 8. Zenith view into a basidiocarp of the same age. See text for details not marked.

Figs. 41 and 42 are from sections in the region of lines 10 and 11 in diagrams 7 and 8. The gills are attached above and below. But it is very clear here that the attachment below, as well as above, is to the pileus. Since the gills are downward growths of the level palisade, formed on the under surface of the pileus (*i. e.*, perpendicular to the level palisade), the attachment below in these figures, as well as that above, is at the point of origin of the gills, and must not be interpeted as an attachment of the gill margin to the stem.

The First Ridges, or Salients, of the Hymenophore are the Fundaments of the Lamellæ Themselves.—The question of the origin

of the lamellæ is of renewed interest since it has recently been stated that one of the problems yet to be worked out in the Agaricaceæ is the origin of the lamellæ.[34] The evidence presented in support of this sweeping, and rather surprising statement, is made, so far as we can judge, on the basis of an investigation of *Coprinus micaceus*. It carries with it the implied charge that all of the observations and statements in regard to the origin of the gills, covering a period of more than half a century, are incorrect. In the case of my own work on *Agaricus campestris*,[35] *Armillaria mellea*,[36] *Lepiota clypeolaria*,[37] *Agaricus arvensis*[38] and *A. comtulus* it can be most positively reaffirmed that the lamellæ originate as described, as downward, radial growths of the level palisade portion of the hymenophore. The evidence was so clear in these examples that at the time of the study it did not seem desirable to present full series of "tangential" sections of the different stages in the origin of the gills, particularly as the method of origin agreed in all respects with that described in more than a dozen different species in earlier works. The present study of *Agaricus rodmani* was undertaken, not only for the purpose of examining into the significance of the double annulus, but also for the purpose of examining the different stages in the organization of the hymenophore primordium, the level palisade stage, and the origin of the gills, in a species closely related to *Agaricus campestris*. It is very clear that the present study has fully confirmed the earlier statements with reference to the origin of the lamellæ. Material has also been grown, and the young stages obtained for sectioning in the following commercial forms of *Agaricus: A. campestris* varieties *bohemia* and *alaska*, and *A.* "*villaticus*."

[34] Levine, M., "The Origin and Development of the Lamellæ in *Coprinus micaceus*," *Am. Jour. Bot.*, 1, 343–356, pls. 39, 40, 1914.

[35] Atkinson, Geo. F., The Development of *Agaricus compestris*," *Bot. Gaz.*, 42, 241–264, pls. 7–12, 1906.

[36] Atkinson, Geo. F., "The Development of *Armillaria mellea*," *Myc. Centralb.*, 4, 113–121, pls. 1, 2, 1914.

[37] Atkinson, Geo. F., "The Development of *Lepiota clypeolaria*," *Ann. Myc.*, 12, 346–356, pls. 13–16, 1914.

[38] Atkinson, Geo. F., "The Development of *Agaricus arvensis* and *A. comtulus*," *Am. Jour. Bot.*, 1, 3–22, pls. 1, 2, 1914. "Homology of the Universal Veil in *Agaricus*," *Myc. Centralb.*, 5, 13–19, pls. 1–3, 1914.

The situation in certain species of *Coprinus*, where the margins of the gills are attached to the stem before maturity, and break away during the expansion of the plants, has for a long time interested me, and I have intended to investigate certain of the species for the purpose of comparing the situation in this genus with that described in *Amanita rubescens*[39] by deBary, *A. muscaria*[40] by Brefeld and in *Amanitopsis vaginata*[41] by myself, where there is no general prelamellar cavity, and the first evidence of the lamellæ is the differentiation of a series of radial trabeculæ in the hymenophore primordium, continuous with the stem and trama of the pileus. This investigation was delayed, however, until the autumn of 1914. Material of three species, *Coprinus comatus, atramentarius* and *micaceus,* was studied, and the results will be published in another paper. This much may he said here, that these three species do not belong to the *Amanita* type but to the *Agaricus* type. There is a strong, annular, prelamellar cavity in *Coprinus comatus,* a weak one in *C. atramentarius* and *micaceus,* but in all three the lamellæ originate as downward-growing salients of a level palisade zone, exactly as described here for *Agaricus rodmani,* the only difference being in those specific features relating to the structure of the lamellæ. Levine based his interpretation of the origin of the lamellæ in *Coprinus micaceus* on complicated and rather well advanced stages of their development. Had the origin of these complicated structures been sought it is probable that the origin of the lamellæ would have been found.

Of the plants thus far studied the following species may be mentioned as examples of the *Agaricus* type in which the origin of the lamellæ has been clearly and correctly described, those by Hoffmann more than half a century ago. *Agaricus carneotomentosus* (*Panus torulosus*) by Hoffmann[42] (1856, p. 145); *Cantharellus*

[39] De Bary, A., "Morphologie und Physiologie der Pilze, Flechten und Myxomyceten," Leipzig, 1866. "Vergleichende Morphologie and Biologie der Pilze, Mycetozoen und Bacterien," 1884. "Comparative Morphology and Biology of the Fungi, Mycetozoa and Bacteria," Oxford, 1887.

[40] Brefeld, O., "Botanische Untersuchungen über Schimmelpilze," 3, Basidiomyceten, I., I.-IV., 1-226; pls. 6-11, 1887.

[41] Atkinson, Geo. F., "The Development of *Amanitopsis vaginata*," *Ann. Myc.*, **12**, 369-392, pls. 17-19, 1914.

[42] Hoffmann, H., "Die Pollinarien und Spermatien von Agaricus," *Bot. Zeit.*, **14**: 137-148; 153-163, pls. 5, 1856.

tubaeformis, C. aurantiacus, Panus stipticus, Pleurotus tremulus, Omphalia umbellifera, O. pyxidata, Marasmius epiphyllus by Hoffmann[43] (1860); *Collybia velutipes, C. fusipes, Hygrophorus chlorophanus, Galera mycenopsis, Hebeloma mesophaeus, Coprinus fimitarius, Paxillus involutus, Entoloma sericeum,* and others by Hoffmann[44] (1861); *Mycena vulgaris, Collybia dryophila, Nyctalis parasitica, Clitocybe cyathiformis,* and *Cantharellus infundibuliformis* by deBary[45] (1866, 1884, 1887) the latter two in conjunction with Woronin; *Coprinus lagopus* by Brefeld[46] (1877, p. 127); *Agaricus campestris* by Atkinson[47] (1906); *Hypholoma* by Miss Allen[48] (1906) and by Beer[49] (1911); *Stropharia ambigua*[50] by Zeller (1914); *Agaricus arvensis* and *comtulus,*[51] and *Armillaria mellea*[52] by Atkinson (1914).

SUMMARY.

1. The lower limb of the double annulus of *Agaricus rodmani* is not a true volva like that of the Amanitas thus far studied. It is composed of a short segment of the blematogen plus some of the inner tissue of the marginal veil. The greater portion of the blematogen remains "concrete" with or consolidated with the surface of

[43] "Beiträge zur Entwickelungsgeschichte und Anatomie der Agaricinen," *Bot. Zeit.*, **18**: 389–395; 397–404, pls. 13, 14, 1860.

[44] Hoffmann, H., "Icones Analyticæ Fungorum; Abbildungen und Bescreibungen von Pilzen mit besonderer Rucksicht auf Anatomie und Entwickelungsgeschichte, 1–105, pls. 1–24, 1861.

[45] DeBary, A., "Morphologie und Physiologie der Pilze, Flechten und Mycetozoen," Leipzig, 1866. "Vergleichende Morphologie und Biologie der Pilze, Mycetozoen und Bacterien," 1884. "Comparative Morphology and Biology of the Fungi, Mycetezoa and Bacteria," Oxford, 1887.

[46] Brefeld, O., Botanische Untersuchungen über Schimmelpilze," 3, Basidiomyceten, I., I.–IV., 1–226; pls. 6–11, 1887.

[47] Atkinson, Geo. F., "The Development of *Agaricus campestris,*" *Bot. Gaz.*, **42**: 241–264, pls. 7–12, 1906.

[48] Allen, Caroline L., "The Development of Some Species of *Hypholoma,*" *Ann. Myc.*, **4**: 387–394, pls. 5–7, 1906.

[49] Beer, R., "Notes on the Development of the Carpophore in Some Agaricaceæ," *Ann. Bot.*, **25**²: 683–689, pl. 52, 1911.

[50] Zeller, S. M., "The Development of *Stropharia ambigua,*" *Mycologia*, **6**: 139–145, pls. 124, 125, 1914.

[51] Atkinson, Geo. F., "The Development of *Agaricus arvensis* and *A. comtulus,*" *Am. Jour. Bot.*, **1**: 3–22, pls. 1, 2, 1914.

[52] Atkinson, Geo. F., "The Development of *Armillaria mellea,*" *Myc. Centralb.*, **4**: 113–121, pls. 1, 2, 1914.

the pileus, while in *Amanita* the blematogen is finally delimited from the surface of the pileus by a cleavage layer. A double annulus homologous with that of *Agaricus rodmani* is often present in certain other species of *Agaricus.*

2. The primordium of the basidiocarp is oval in form, and homogeneous in structure, consisting of intricately interwoven hyphæ.

3. The four primary parts of the basidiocarp, pileus, stem, marginal veil and hymenophore, are first differentiated by the origin of the hymenophore fundament.

4. The hymenophore primordium arises as an internal, annular zone of new growth toward the upper part of the young basidiocarp. It consists of slender hyphæ rich in protoplasm, parallel, and directed obliquely downward. The lower outer surface is at first more or less open and uneven, presenting a frayed or fimbriate appearance. By continued growth and multiplication of these hyphæ the hymenophore primordium becomes more compact and the under surface becomes even, forming a level palisade zone. Growth of the hymenophore proceeds in a centrifugal direction, the older portions being next the stem fundament. By the epinastic growth of the pileus margin the hymenophore takes on the form of an annular arch.

5. The increase in number and diameter of the elements of the hymenophore fundament produce a tension upon the ground tissue beneath, which lags behind in growth and is torn away from the under surface of the hymenophore, thus forming an annular, prelamellar cavity. This cavity may later be nearly filled by the ground tissue of the inner portion of the veil which increases in bulk, and is often crowded up against the young gills by the involute margin of the pileus.

6. The lamellæ originate as downward growing radial salients of the level palisade zone, beginning next, or on the stem, according as the hymenophore primordium is free from or extends down on the upper portion of the stem fundament. They progress in a centrifugal direction. In an intermediate stage of development of the basidiocarp, all three stages of the hymenophore may be present, the zone of gill salients next the stem, then the level palisade zone, and beyond this the primordial zone.

7. The first ridges, or salients, which appear in connection with the hymenophore are the fundaments of the lamellæ themselves, and the palisade layer is continuous over their edges as well as in the notch between adjacent salients.

DESCRIPTION OF PLATES VII.-XIII.

PLATE VII.

Mature and nearly mature plants of *Agaricus rodmani* showing the double nature of the annulus with its edge grooved; forming an upper and lower limb; the short stem, involute margin of the pileus, etc. $\times$ 2/3 diameter. For details see text.

PLATE VIII.

Mature and very robust plants from parking between sidewalk and street. Real size. See text.

PLATES IX.-XIII.

The magnifications of the photomicrographs are as follows: Figs. 3-8; $\times$ 9 diameters. Fig. 33; $\times$ 10 diameters. Figs. 1, 2; $\times$ 12 diameters. Fig. 32; $\times$ 13 diameters. Figs. 34-36; $\times$ 20 diameters. Fig. 17; $\times$ 23 diameters. Figs. 15, 16; $\times$ 28 diameters. Figs. 21-30, 37-42; $\times$ 30 diameters. Fig. 31; $\times$ 100 diameters. Fig. 12; $\times$ 110 diameters. Fig. 13; $\times$ 155 diameters. Figs. 10, 11; $\times$ 160 diameters Fig. 18; $\times$ 170 diameters. Figs. 9-14; $\times$ 225 diameters. Figs. 19, 20; $\times$ 250 diameters.

PLATE IX.

FIG. 1. (No. 18.) Young stage of basidiocarp primordium.

FIG. 2. (No. 20.) Somewhat older stage of basidiocarp primordium, but still in the undifferentiated stage.

FIG. 3. (No. $^{27}/_{1}$.) Earliest stage of differentiation in the young basidiocarp, median longitunial section showing a transection of the internal annular hymenophore fundament, the general prelamellar cavity not yet formed. Pileus fundament is above, stem fundament below, and veil fundament underneath the hymenophore primordium (see Fig. 9).

FIG. 4. (No. $^{27}/_{4}$.) Longitudinal section of the same basidiocarp, "tangential" to the hymenophore primordium, which is shown as a transverse deeply staining area.

FIG. 5. (No. $^{23}/_{1}$.) Median longitudinal section through a basidiocarp just after the formation of the general, annular, prelamellar cavity. The hymenophore is still in the primordial condition (see Fig. 10) but does not extend down on the surface of the upper part of the stem fundament.

FIG. 6. (No. $^{23}/_{3}$.) Longitudinal section of the same basidiocarp, "tangential" to the hymenophore and annular cavity (see Fig. 13).

FIG. 7. (No. $^{12}/_{2}$.) Median longitudinal section of a basidiocarp just after the formation of the general, annular, prelamellar cavity. The

hymenophore is still entirely in the primordial stage (see Fig. 11) and extends for a considerable distance down on the surface of the upper part of the stem fundament.

FIG. 8. (No. 12/1.) Longitudinal section of the same basidiocarp, "tangential" to the hymenophore and annular cavity (see Fig. 16).

PLATE X.

FIG. 9. (No. 27/1.) More highly magnified view of the transection of the hymenophore primordium shown in Fig. 3; stem axis at the left. In the darker area (hymenophore primordium) the hyphae extend downward and obliquely outward toward, and some projecting into, the veil fundament below, which consists of a loose mesh of interwoven hyphae.

FIG. 10. (No. 23/1.) More highly magnified view of the transection of the hymenophore primordium and annular cavity shown in Fig. 5 (axis of stem at the left).

FIG. 11. (No. 12/2.) More highly magnified view of the transection of the hymenophore primordium and annular cavity shown in Fig. 7 (stem axis at right). The hymenophore primordium extends down over the upper part of the stem outer surface. Veil fundament in the angle below, the ground tissue tearing apart and separting from the fimbriate under surface of the hymenophore

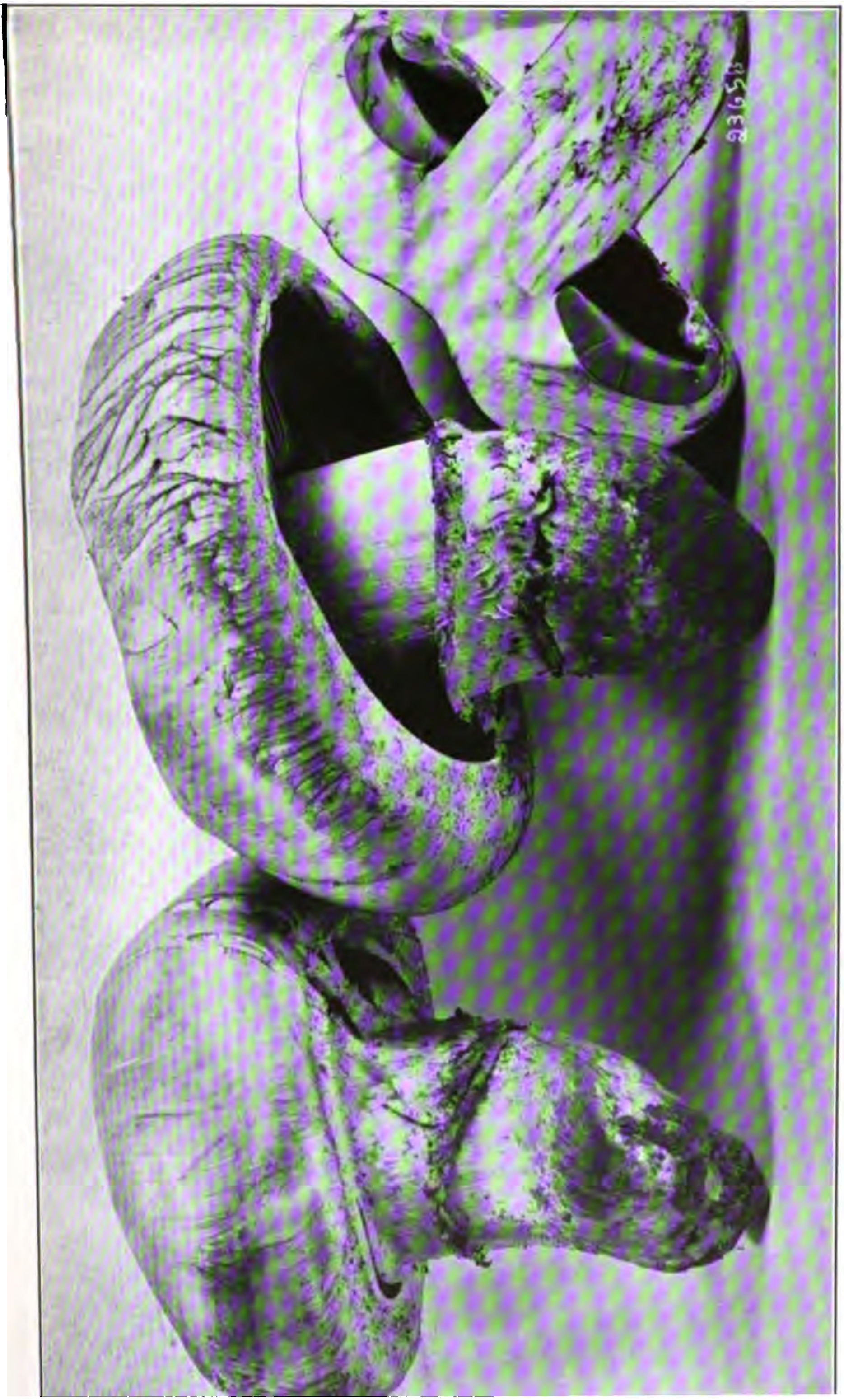
FIG. 12. (No. 3/8.) Transection of hymenophore and annular cavity, showing same view as Fig. 11 (stem axis at right) but in another basidiocarp and slightly older stage; the portion of the hymenophore primordium on the upper part of the stem fundament has become transformed into the level palisade stage.

FIGS. 13 and 14. (No. 2/2.) Section of another basidiocarp showing the hymenophore and annular cavity in same stage as in Fig. 10, at different magnifications (stem axis at right). Hymenophore primordium with fimbriate edge. Ground tissue below (veil fundament) breaking away from the fimbriate surface of the hymenophore as a result of the tension produced by the rapid increase in number and size of the elements of the hymenophore and the lagging behind of the ground tissue below, thus forming the annular cavity. These sections are radial and parallel with the direction of the later lamellæ. The elements of the hymenophore here are somewhat clustered, the slender ends of the hyphæ clinging in groups as the lower surface of the hymenophore is loosened by the tension of the increase above.

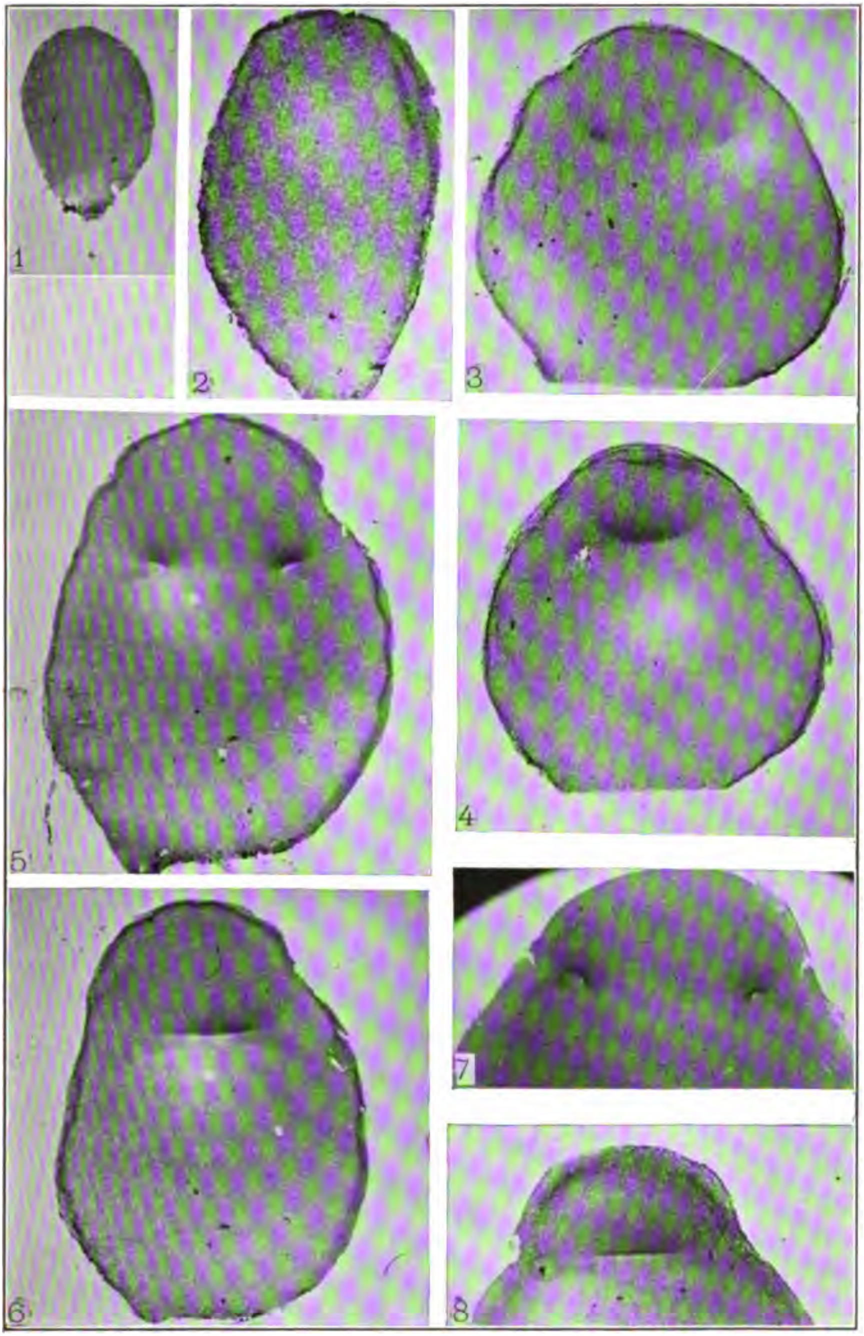
FIG. 15. (No. 23/3.) "Tangential" section of the hymenophore primordium, more highly magnified view of the hymenophore and general, annular, prelamellar cavity shown in Fig. 6. Note the fimbriate lower surface of the hymenophore primordium, and the loose ground tissue (primordium of veil) below separating from it and forming the annular cavity. The structure of the hymenophore primordium is homogeneous, there is not the slightest evidence of gill salients, or of ridges of any sort, which precede or have any relation to the lamellæ which are to arise later.

FIG. 16. (No. 12/1.) "Tangential" section of hymenophore primordium, annular cavity and veil fundament, a more highly magnified view of this part of the basidiocarp shown in Fig. 8. Details as in Fig. 15.

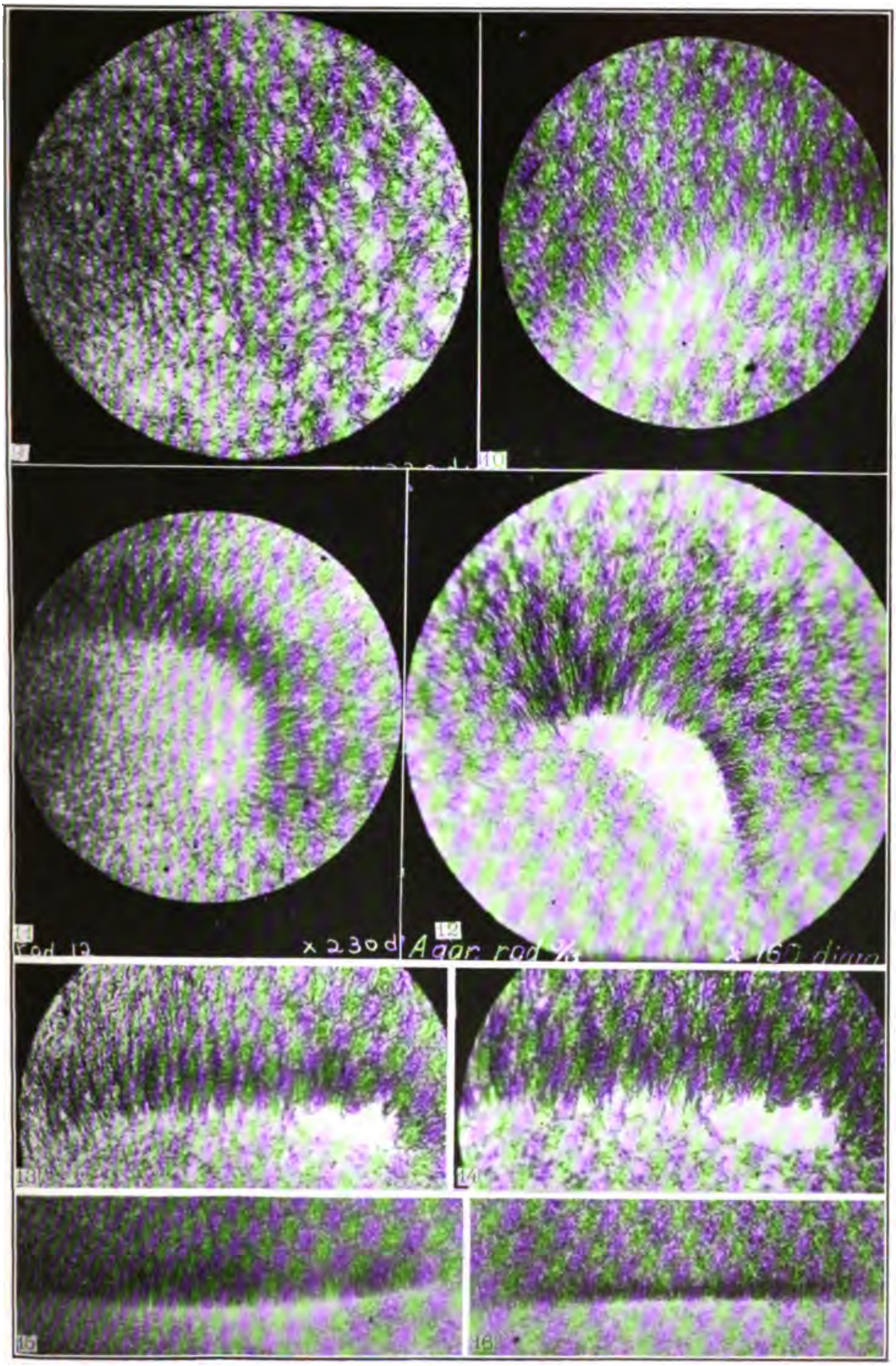
AGARICUS RODMANI



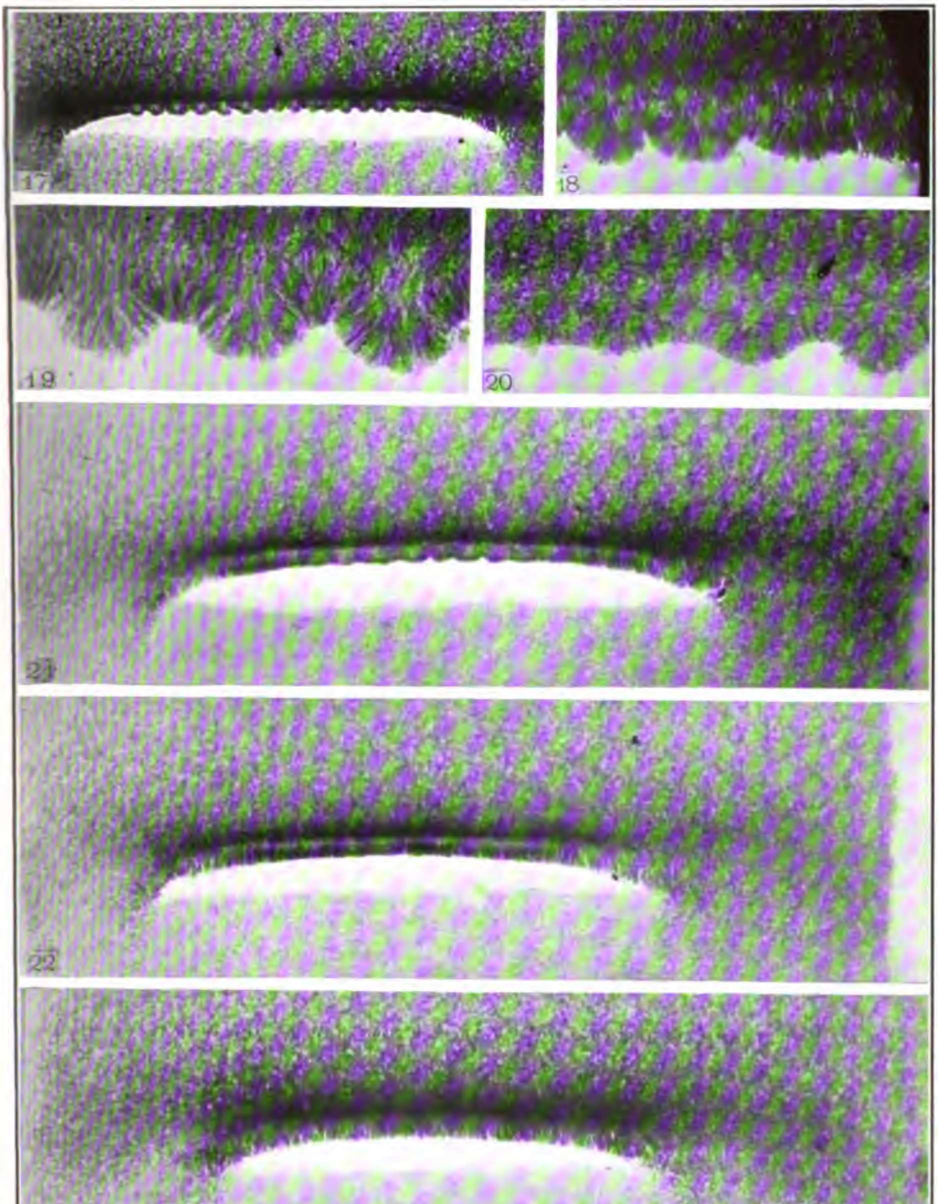
AGARICUS RODMANI



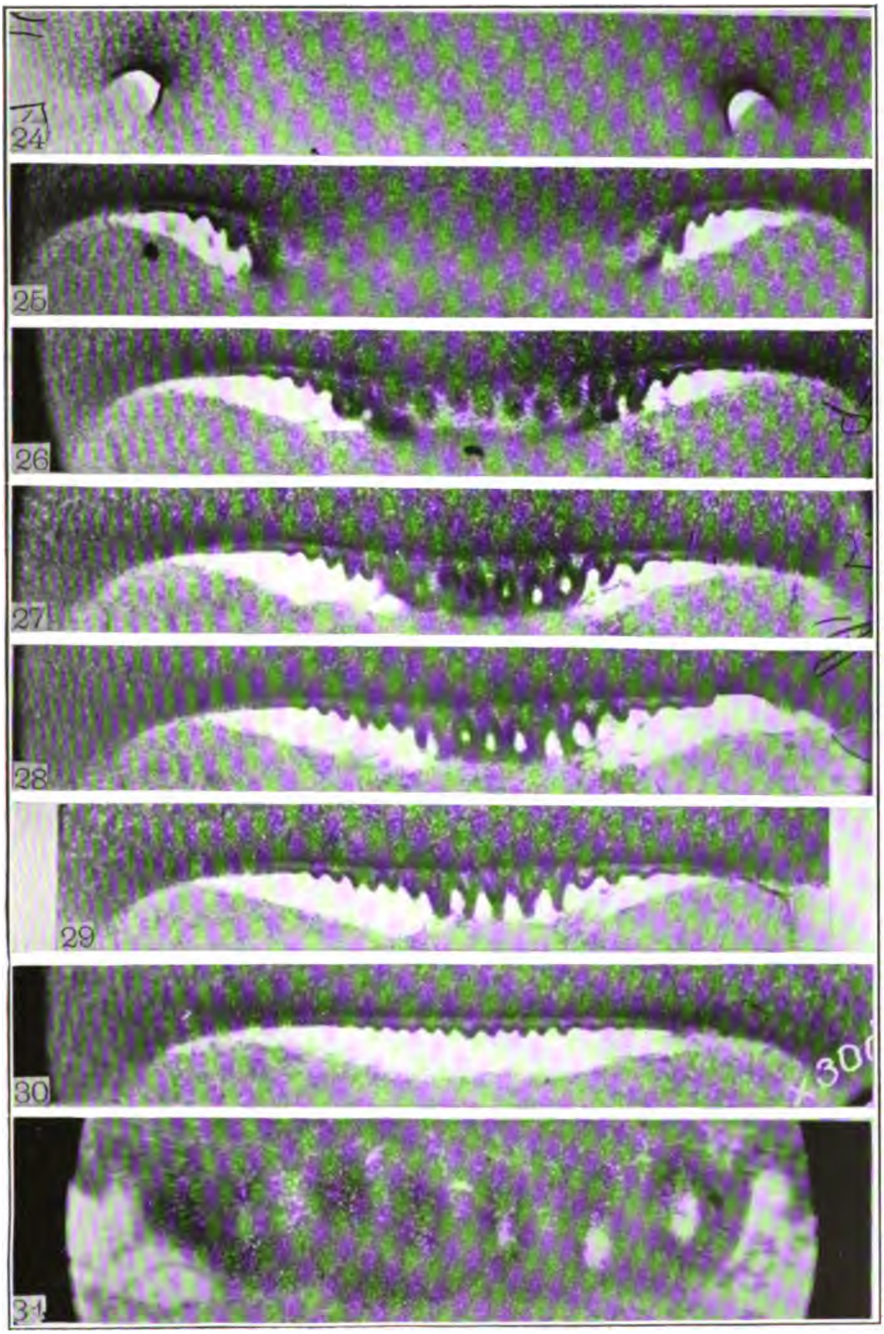
AGARICUS RODMANI



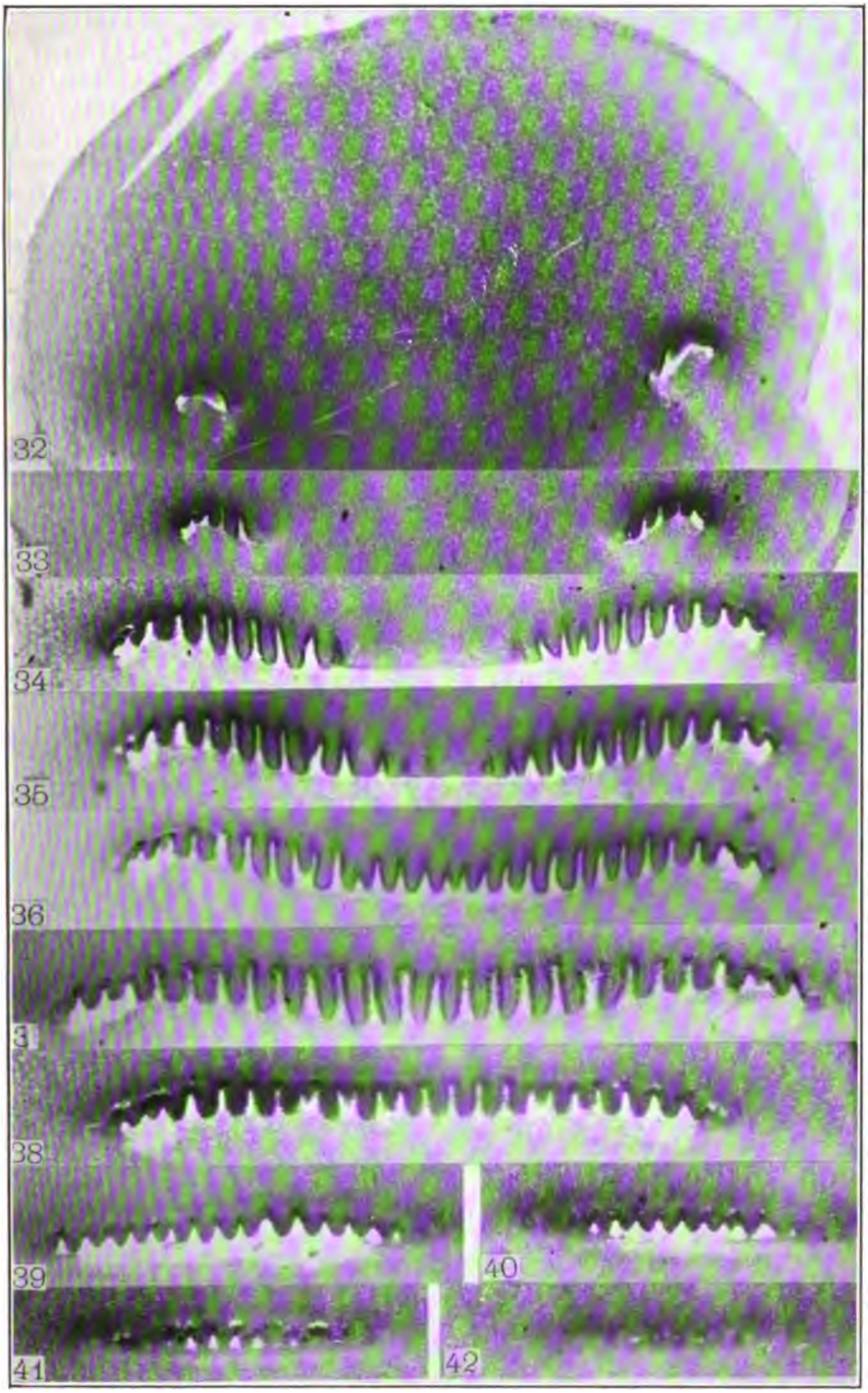
AGARICUS RODMANI



AGARICUS RODMANI



AGARICUS RODMANI



AGARICUS RODMANI

PLATE XI.

FIGS. 17–19 and 21–23, all from a single basidiocarp (No. 5/2), from selected serial sections parallel with the axis of the stem and "tangential" in the pileus. Fig. 17 is from near the stem, and shows the three stages of the developing hymenophore, primordial zone, level palisade zone, and the zone of gill salients (transected) with different stages in the origin of the latter from the level palisade condition (see text for details). The general annular cavity is well shown.

FIG. 18. More highly magnified view of portion of the same section in the region of the origin of the gill salients from the level palisade stage.

FIG. 19. More highly magnified view of the young gill salients, showing how they flare, or fantail, when released from the pressure to which the elements are subjected in the level palisade zone, also showing how this flaring of the young gill salients crowds the intervening palisade cells of the original level into "ridges," these ridges of palisade in the notch between two lamellæ being formed later than the gill salients, and as a result of the lateral pressure of the flaring salients. For details see the text.

FIG. 20. (No. 1/2.) Section from another basidiocarp showing transition from the level palisade stage to the gill salients.

FIG. 21. Section nearer the margin of the pileus than that shown in Fig. 17. In the middle area the gill salients are cut near their distal end where they are very low (see text for details). Transition to level palisade and primordial zone on either side.

FIG. 22. Section still nearer the margin of the pileus showing the level palisade zone in the center, and the primordial zone on either side.

FIG. 23. Section still nearer the margin of the pileus, entirely through the primordial zone.

PLATE XII.

FIGS. 24–31. Selected serial sections from a single basidiocarp (No. 1/3), parallel with the stem axis and from nearly median in the stem to midway from stem surface to the margin of the pileus. Here the hymenophore extends for some distance down on the outward sloping surface of the stem fundament, and there are little "stalls" or pigeon holes between them in the angle at junction of pileus and stem. See text for details.

PLATE XIII.

FIGS. 32–42. Selected serial sections from a single basidiocarp (No. 11), parallel with the axis of the stem and from median in the stem to "tangential" in the margin of the pileus. See text for details.

THE EULER-LAPLACE THEOREM ON THE DECREASE OF THE ECCENTRICITY OF THE ORBITS OF THE HEAVENLY BODIES UNDER THE SECULAR ACTION OF A RESISTING MEDIUM.

By T. J. J. SEE.

(*Read April 24, 1915.*)

In the "Mécanique Céleste," Liv. VII., Chap. VI., §§ 29–30, and Liv. X., Chap. VII., § 18, Laplace has developed the mathematical theory of the secular action of a resisting medium, and applied it to the motions of the moon and planets. The first discussion herein cited was published in Volume III. of the "Mécanique Céleste," 1802. It is on this discussion by Laplace that modern investigators chiefly base their treatment of the problems of a resisting medium. Laplace's development of the theory therefore has been of great service to science for more than a century.

Recently, while occupied with a careful review of the theories of magnetism and of gravitation since the time of Newton, I had occasion to examine Euler's "Dissertatio de Magnete," 1744, "Opuscula," 1746–51; and while looking into this work was surprised to find that Euler had preceded Laplace in his development of the chief effects of a resisting medium by more than half a century. Euler's work on the resisting medium will be found in the volume of "Opuscula," Berlin, 1746, in the paper "De Relaxatione Motus Planetarum," pp. 245–276.

Having shown that the aphelia are undisturbed by resistance, Euler considers in section XVII. the equations for the mean motion, and the return to perihelion, after changes in the mean motion by the increments representing a whole revolution:

$$nt + 2\pi, \quad nt + 4\pi, \quad nt + 6\pi, \quad nt + 8\pi, \text{ etc.}$$

Euler puts for the planetary orbit about the sun,

$h = a(1 - e) =$ perihelion distance,

$g = a(1 - e^2) = p =$ latus rectum of the orbit,

$y = r =$ radius vector of the planet,

$Z = e =$ the eccentricity of the orbit,

$t =$ true anomaly $= v$, in the notation now commonly used,

$s =$ arc of the orbit, reckoned from perihelion,

$c =$ sun's mean distance,

$= \mu\alpha$, where α is the earth's equatorial semi-diameter, and

μ a number which expresses the sun's mean distance in this unit. Euler uses a solar parallax of 13″, and takes $c = 15866\alpha$. With the values now adopted in astronomy we have about $c = 23445\alpha$. In some of his numerical work Euler uses $c = g = a(1 - e^2)$, which is admissible when we neglect the square of the eccentricity.

Euler also uses a small angle of deviation due to the angular effects of resistance, $z = \theta$, such that $\tan z = 2g/3c$; and then takes the equation for the Keplerian ellipse

$$r = \frac{a(1 - e^2)}{1 + e\cos v},$$

to have the form of an ellipse modified by resistance

$$\frac{1}{r} = \frac{1}{p}(1 + e\cos v) = \frac{1}{p} + \frac{e\cos v}{p} + P,$$

where P is function of the time, but modified by a very small quantity depending on the effects of the secular action of the resisting medium.

From the equations of the disturbed ellipse, in his notation,

$$\frac{1}{y} = \frac{1}{g} + \frac{\zeta}{g}\cos t + P,$$

$$P = \frac{1}{c}(t - \sin t - \tfrac{1}{2}\zeta\sin t + \tfrac{1}{2}\zeta t\cos t + \tfrac{3}{4}\zeta\zeta t - \tfrac{2}{3}\zeta\zeta\sin t - \tfrac{1}{24}\zeta\zeta\sin 2t),$$

Euler develops the following table:

If	there will be
$t = 0,$	$P = 0,$
$t = \pi - \theta,$	$P = \dfrac{1}{c}\left(\pi - \tfrac{1}{2}\zeta\pi - \dfrac{4g}{\zeta c}\right),$
$t = 2\pi,$	$P = \dfrac{1}{c}(2\pi + \zeta\pi),$
$t = 3\pi - \theta,$	$P = \dfrac{1}{c}\left(3\pi - \tfrac{3}{2}\zeta\pi - \dfrac{4g}{\zeta c}\right),$
$t = 4\pi,$	$P = \dfrac{1}{c}(4\pi + 2\zeta\pi),$
$t = 5\pi - \theta,$	$P = \dfrac{1}{c}\left(5\pi - \tfrac{5}{2}\zeta\pi - \dfrac{4g}{\zeta c}\right).$

He remarks that when therefore for perihelion we have

$$\frac{1}{y} = \frac{1 + \zeta}{g} + P,$$

and for aphelion

$$\frac{1}{y} = \frac{1 - \zeta}{g} + P,$$

if $t = 0,$	$\dfrac{1}{y} = \dfrac{1 + \zeta}{g} + 0,$
$t = \pi - \theta,$	$\dfrac{1}{y} = \dfrac{1 - \zeta}{g} + \dfrac{\pi}{c}(1 - \tfrac{1}{2}\zeta),$
$t = 2\pi,$	$\dfrac{1}{y} = \dfrac{1 + \zeta}{g} + \dfrac{2\pi}{c}(1 + \tfrac{1}{2}\zeta),$
$t = 3\pi - \theta,$	$\dfrac{1}{y} = \dfrac{1 - \zeta}{g} + \dfrac{3\pi}{c}(1 - \tfrac{1}{2}\zeta),$
$t = 4\pi,$	$\dfrac{1}{y} = \dfrac{1 + \zeta}{g} + \dfrac{4\pi}{c}(1 + \tfrac{1}{2}\zeta),$
$t = 5\pi - \theta,$	$\dfrac{1}{y} = \dfrac{1 - \zeta}{g} + \dfrac{5\pi}{c}(1 - \tfrac{1}{2}\zeta);$

it being understood that the final angle $4g/3c$ is neglected as very small.

Euler next considers the effect of i whole revolutions:

$$t = 2i\pi,$$

$$\frac{1}{y} = \frac{1+\zeta}{g} + \frac{2i\pi}{c}(1 + \tfrac{1}{2}\zeta);$$

and finds for the radius vector:

$$y = \frac{g}{1+\zeta} - \frac{2i\pi}{c}\frac{(1+\tfrac{1}{2}\zeta)gg}{(1+\zeta)^2}.$$

Putting for the following aphelion, $t = (2i+1)\pi - \theta$, there will result

$$\frac{1}{y} = \frac{1-\zeta}{g} + \frac{(2i+1)\pi}{c}(1 - \tfrac{1}{2}\zeta);$$

whence the radius vector becomes

$$y = \frac{g}{1-\zeta} - \frac{(2i+1)\pi(1-\tfrac{1}{2}\zeta)gg}{c(1-\zeta)^2}.$$

The successive distances of the planet from the sun are diminished in the following manner:

I. Perihelion	$\dfrac{g}{1+\zeta} = 0,$
Aphelion	$\dfrac{g}{1-\zeta} - \dfrac{\pi(1-\tfrac{1}{2}\zeta)gg}{c(1-\zeta)^2},$
II. Perihelion	$\dfrac{g}{1+\zeta} - \dfrac{2\pi(1+\tfrac{1}{2}\zeta)gg}{c(1+\zeta)^2},$
Aphelion	$\dfrac{g}{1-\zeta} - \dfrac{3\pi(1-\tfrac{1}{2}\zeta)gg}{c(1-\zeta)^2},$
III. Perihelion	$\dfrac{g}{1+\zeta} - \dfrac{4\pi(1+\tfrac{1}{2}\zeta)gg}{c(1+\zeta)^2},$
Aphelion	$\dfrac{g}{1-\zeta} - \dfrac{5\pi(1-\tfrac{1}{2}\zeta)gg}{c(1-\zeta)^2}$, etc.

In any revolution about the sun the perihelion advances by the interval

$$\frac{2\pi(1+\frac{1}{2}\zeta)gg}{c(1+\zeta)^2};$$

and the aphelion regresses by the interval

$$\frac{2\pi(1-\frac{1}{2}\zeta)gg}{c(1-\zeta)^2};$$

the mean distance therefore decreases in the interval about $2\pi gg/c$; and after i revolutions this decrease in the mean distance will be $2i\pi gg/c$.

Accordingly, after i planetary revolutions, the perihelion distance from the sun becomes:

$$\frac{g}{1+\zeta}-\frac{2i\pi(1+\frac{1}{2}\zeta)gg}{c(1+\zeta)^2};$$

and the following aphelion distance:

$$\frac{g}{1-\zeta}-\frac{(2i+1)\pi(1-\frac{1}{2}\zeta)gg}{c(1-\zeta)^2}.$$

The addition of these values, after i revolutions, effects the transverse axis of the orbit:

$$\frac{2g}{1-\zeta\zeta}-\frac{4i\pi gg}{c(1-\zeta\zeta)^2}-\frac{\pi(1-\frac{1}{2}\zeta)gg}{c(1-\zeta)^2}.$$

Here indeed, since the time is to be defined, the time from perihelion to aphelion may be omitted; and thus after i revolutions the transverse axis of the orbit is found to be:

$$\frac{2g}{1-\zeta\zeta}-\frac{4i\pi gg}{c(1-\zeta\zeta)^2};$$

wherefore also the initial transverse axis is assumed equal to $2g/(1-\zeta\zeta)$.

If, therefore, the distance from the perihelion to the sun after i revolutions, which is equal to

$$\frac{g}{1+\zeta}-\frac{2i\pi(1+\frac{1}{2}\zeta)gg}{c(1+\zeta)^2},$$

be subtracted from the distance from the aphelion to the sun, which would develop in the same time, and found to be equal to

$$\frac{g}{1-\zeta} - \frac{2i\pi(1-\frac{1}{2}\zeta)gg}{c(1-\zeta)^2},$$

it will give for the distance of the foci after i revolutions

$$\frac{2\zeta g}{1-\zeta\zeta} - \frac{2i\pi\zeta gg(3-\zeta\zeta)}{c(1-\zeta\zeta)^2}.$$

The initial transverse axis was $2g/(1-\zeta\zeta)$, and if we divide this into the last expression, we get for the eccentricity of the orbit at this time: $\zeta - 3i\pi\zeta g/c$, terms in ζ^3 being neglected as insensible.

In Euler's paper the factor 3 in the last term is inadvertently omitted. He remarks that the original eccentricity was ζ, whereas after i revolutions it is decreased by the negative term shown above, and thus is subject to a secular diminution, owing to the secular action of the resisting medium.

After this discussion Euler reaches the conclusion: "A resistentia ergo excentricitas continuo minuitur, orbitaeque planetarum propius ad figuram circularem reducuntur" (p. 271).

He therefore recognized clearly that the effect of a resisting medium is to decrease the eccentricity incessantly, and to render the orbit more and more circular; and had reached this important conclusion some fifty-six years (1746) before the corresponding theorem was established by Laplace in 1802.

Accordingly as Euler's reasoning is essentially rigorous, though not the same as that of Laplace, it is evident that he was the first discoverer of the theorem which is of such fundamental importance in the theories of cosmogony.

It is remarkable that although Laplace had this theorem clearly before his mind for a quarter of a century at the close of his life (1802–1827) he did not once suspect that the planets and satellites had originated in the distance and through the action of a resisting medium had neared the centers about which they now revolve, and thus acquired the wonderful circularity of their orbits.

It is well known that Laplace continually refers to these bodies as detached by rotation, in the form of zones of vapor, as first

outlined in his nebular hypothesis of 1796. He thus misled the scientific world for more than a century, till the capture theory, involving formation in the distance with subsequent approach to their central masses, under the secular action of a resisting medium, was developed by the present writer in 1908–10.

It is equally well known that Laplace always held the comets to be foreign to our system—another misleading doctrine in cosmogony, finally overthrown in 1910 by the independent researches of Strömgren of Copenhagen, and the present writer, who showed that the comets are surviving residues of the ancient nebula which formed our solar system.

In my "Researches," Vol. II., pp. 138–139, I have drawn attention to two letters from Euler to the Royal Society, pointing out, as early as 1749, that the earth was once beyond the present orbit of Saturn. He does not there discuss the secular decrease of the eccentricity of the planetary orbits; yet as he had grounds for holding to a secular approach to the central masses, he was the first writer to outline sound views in cosmogony.

Under the circumstances it appears appropriate that the theorem on the secular decrease of the eccentricities of the orbits of bodies moving in resisting media, should be known by the name of the Euler-Laplace theorem. This recognizes the correct historical development, as now made out; and probably will always hold a fundamental place in the science of celestial evolution.

MARE ISLAND, CALIFORNIA,
April 6, 1915.

MAGELLANIC PREMIUM

FOUNDED IN 1786 BY JOHN HYACINTH DE MAGELLAN, OF LONDON

1915

THE AMERICAN PHILOSOPHICAL SOCIETY

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IT WILL AWARD ITS

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TO THE AUTHOR OF THE BEST DISCOVERY, OR MOST USEFUL INVENTION, RELATING TO NAVIGATION, ASTRONOMY, OR NATURAL PHILOSOPHY (MERE NATURAL HISTORY ONLY EXCEPTED) UNDER THE FOLLOWING CONDITIONS :

1. The candidate shall, on or before November 1, 1915, deliver free of postage or other charges, his discovery, invention or improvement, addressed to the President of the American Philosophical Society, No. 104 South Fifth Street, Philadelphia, U. S. A., and shall distinguish his performance by some motto, device, or other signature. With his discovery, invention, or improvement, he shall also send a sealed letter containing the same motto, device, or other signature, and subscribed with the real name and place of residence of the author.

2. Persons of any nation, sect or denomination whatever, shall be admitted as candidates for this premium.

3. No discovery, invention or improvement shall be entitled to this premium which hath been already published, or for which the author hath been publicly rewarded elsewhere.

4. The candidate shall communicate his discovery, invention or improvement, either in the English, French, German, or Latin language.

5. A full account of the crowned subject shall be published by the Society, as soon as may be after the adjudication, either in a separate publication, or in the next succeeding volume of their Transactions, or in both.

6. The premium shall consist of an oval plate of solid standard gold of the value of ten guineas, suitably inscribed, with the seal of the Society annexed to the medal by a ribbon.

All correspondence in relation hereto should be addressed

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PROCEEDINGS

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VOL. LIV. OCTOBER–DECEMBER, 1915. No. 220.

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PHILADELPHIA

THE AMERICAN PHILOSOPHICAL SOCIETY

104 SOUTH FIFTH STREET

1915

American Philosophical Society

General Meeting—April 13-15, 1916

The General Meeting of 1916 will be held on April 13th to 15th, beginning at 2 p. m. on Thursday, April 13th.

Members desiring to present papers are requested to send to the Secretaries, at as early a date as practicable, and not later than March 1, 1916, the titles of these papers, so that they may be announced in the preliminary programme which will be issued immediately thereafter, and which will give in detail the arrangements for the meeting.

Owing to the embarrassment heretofore caused in a crowded programme by the receipt of titles at a very late date, the Committee of Arrangements announces, as a tentative plan, that additional papers can only be inserted in the *final* programme as there appears to be probable time for their presentation.

The Publication Committee, under the rules of the Society, will arrange for the immediate publication of the papers presented.

I. MINIS HAYS
ARTHUR W. GOODSPEED
AMOS P. BROWN
HARRY F. KELLER
Secretaries.

Members who have not as yet sent their photographs to the Society will confer a favor by so doing; cabinet size preferred.

It is requested that all correspondence be addressed

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VOL. LIV OCTOBER–DECEMBER, 1915 No. 220

SYMPOSIUM ON THE EARTH: ITS FIGURE, DIMENSIONS AND THE CONSTITUTION OF ITS INTERIOR.

(*Concluded from page 308.*)

IV.

VARIATIONS OF LATITUDE: THEIR BEARING UPON OUR KNOWLEDGE OF THE INTERIOR OF THE EARTH.

BY FRANK SCHLESINGER.

To review even hastily the contributions that astronomy has made to our knowledge of the figure and dimensions of the earth and the constitution of its interior, would consume more time than I can fairly claim as my share this afternoon. Let me therefore pass over those points that are on accepted ground and are matters of general agreement from the different points of view represented in this symposium; and let me dwell instead upon certain recent developments especially in need of consideration, concerning which the astronomer desires the criticism and help of the geologist, the seismologist, the physicist, and the meteorologist. These developments have come to us directly or indirectly through a study of latitude variations, so that most of what I shall have to say will deal with this subject.

Although variations of latitude are in a sense a very recent addi-

tion to our knowledge, yet on the theoretical side, at least, we find the beginning more than a century and a half ago. In 1755 Euler considered "the rotation of solid and rigid bodies" in a memoir that is now recognized as the foundation stone for our edifice. He showed that if such a body is projected into space it will exhibit two kinds of rotation; the first of these is the familiar one that corresponds to the day in the case of the earth; the other is more subtle and corresponds to the variation of latitude. By reason of this the axis of the diurnal rotation is continually changing within the body, progressing in a regular way and coming back after a time to its earlier positions. An ordinary top gives us a simple example of this kind of rotation. The spinner imparts to the top a motion of translation as well as a rotation, and if we wish to study the rotation we must arrest the translation in some way. This we can do by letting the top fall upon a hard surface in which the iron peg soon wears a minute hole for itself, and the effect is to stop the translation of the top without modifying seriously the rotation. Then we can see that while the top is turning very rapidly around an axis, this axis is itself rotating in a comparatively leisurely way. Just the same thing is occurring with the earth: the point (or pole) at which the axis of the daily rotation pierces the surface of the earth is continually in motion. If we could take to the neighborhood of the pole a modern instrument, and if we could observe there at leisure and in comfort, we should have no particular difficulty in finding the position of the pole within a meter. But if we should repeat these observations a few months later we should find that the pole had wandered away to some distance. To be sure, this distance would not be great and all the wanderings of the pole that have thus far been observed could be plotted to true scale on the floor of a room not much larger than the one we are in. Of course if the pole is moving, so too is the earth's equator; and thus the latitudes of all points on the earth are varying. Such wanderings as these need not disturb the peace of mind of those gentlemen who like to discover the arctic or the antarctic pole. Under the circumstances that the polar explorer must work and with the meager instruments he can transport, he is glad to determine his latitude within half a mile of the truth.

We must understand that it is only in our time and only after the lapse of many years since Euler published his memoir, that latitude variations have actually been observed. There was nothing in Euler's theory to indicate how large a variation to look for, since this is a matter that depends upon the whole complex of "initial conditions," of which our knowledge is the very vaguest. But this theory does tell us what the period of the variation should be, since this depends upon the shape of the earth and the distribution of the material within it, and precisely the information that is here needed is afforded by a study of precession. Applying this information Euler was able to say that the period of the latitude variation should be ten months. Bessel at Königsberg in 1842, later Peters at Pulkova, Nyren also at Pulkova, Downing at Greenwich, and Newcomb at Washington, all searched their observations for evidence of a latitude variation having a period of ten months, but all in vain. Astronomers concluded that if latitude variations existed at all, their extent was too small to be detected by instruments of the precision that had then been attained.

Toward the end of the nineteenth century vague whisperings that this conclusion might be incorrect seem to have been in the air. But the first clear word to this effect came in 1888 from the lips of Küstner at Berlin. He had invented and applied a method for determining the amount of the aberration of light; but he found that his observations gave well-nigh impossible results, agreeing neither among themselves nor with earlier reliable observations. By a nice chain of logic he was able to exclude one possible explanation after another until there was left only the supposition that the latitude of his station had changed while his observations were in progress. Next he examined nearly contemporaneous observations made at other places, and when he found that he could account for certain puzzling discrepancies, he no longer hesitated to announce that latitudes were variable after all.

This announcement awoke the liveliest interest and encountered no little scepticism. Special observations were at once set on foot at various observatories in Europe and America, as well as at a station near Honolulu in the Sandwich Islands. These islands are

about opposite in longitude to the European stations, and this was the reason for establishing a station there. For obviously if the pole is really changing its place then the changes in latitude for two opposite stations will be the reverse of each other. When in 1893 this was found actually to be the case, other possible explanations for the observed phenomena at once fell down, and latitude variations became for the first time a universally accepted fact.

Much time and effort have since been expended in attempting to formulate the "laws" of latitude variations and to give them a mechanical interpretation. But observation has shown that the variations are of unexpected complicity, and as a consequence we are still very far from having satisfactory knowledge of this subject. By the same token it is probable that an intensive study of these variations, particularly from points of view other than the astronomical, will teach us much concerning the interior of the earth as well as some of its surface phenomena.

It was the late Dr. Chandler, of Cambridge, Massachusetts, who took the lead in investigating the nature of latitude variations. By overhauling ancient observations (made of course without any reference to the present subject) he was able to trace the presence of the variations back to the time of Bradley in the middle of the eighteenth century. Thus it happens that at the very time that Euler was writing the first theoretical paper on the subject, Bradley had already begun making the observations from which the actual existence of latitude variations might have been proven at once. Chandler was able to gather similar evidence from other miscellaneous series of observations and thus to set down a tolerably continuous record of the variations during a century and a half. However interesting a fact this may be from an historical point of view, it does not help very much in a practical study of the subject. There are two reasons for this: first, it is only for European stations (and for the most part only for Greenwich) that we have any knowledge of these earlier variations; the other component of the wanderings of the pole, namely that in the meridian at right angles to the meridian of Greenwich, did not begin to be known until very recently. Again, these ancient observations were undertaken for

certain definite purposes that they served as well as could be expected for their time; but they were not intended and are not well suited for precise determinations of the latitude. Close acquaintance with the subject has taught us that exceedingly delicate observations are necessary to define the variations with adequate accuracy. If I held in my hands two plumb lines half a meter apart, they would not be quite parallel to each other, though both are exactly vertical; if they were prolonged, they would meet somewhere near the center of the earth, 4,000 miles below. The angle between them is a little less than $0''.02$ and represents approximately the accuracy that is demanded and that has recently been attained in latitude observations. This success is due chiefly to the International Geodetic Association which has organized an "international latitude service" of high efficiency, and to whose efforts and experience are due the improvements in instruments and methods that have made possible this extraordinary degree of precision. Since 1899, the Association has maintained six observing stations for this sole purpose, two of these being in our own country. One of the minor effects of the war that is now raging in Europe will be the discontinuance of some of these stations. One of the American stations has already been abandoned and the same fate will overtake the other in June, 1916, unless some independent means of maintaining it, at least temporarily, presents itself soon. An interruption of these observations would be a great pity, for this is one of the cases where a continuous record is highly desirable.

To return to Chandler and his work on these variations, perhaps the most important of his achievements was to show that the principal term in the variations, instead of having a period of ten months in accordance with Euler's theory, has in reality a period of fourteen months. This difference explains the failure of Bessel and all the others who preceded Küstner to find a latitude variation in their observations; for, relying upon Euler's results, they had all tested their observations for the ten-month variation and had sought for no other variation. For the same reason, Chandler's announcement of the longer period was received with incredulity in some quarters, and this feeling did not vanish until Newcomb

pointed out that Euler had made a certain assumption regarding the interior of the earth that had in the meantime been universally discarded; his period of ten months applies in fact only to a perfectly rigid and unyielding earth. Newcomb showed that if the earth yields to deformation to the same extent as though it were composed throughout of steel, then Euler's period would be lengthened to about fourteen months. Here we have the first dependable determination of the rigidity of the earth, a result that has since been confirmed in several ways, particularly by a measurement of "bodily tides" in the earth.

The fourteen-month term (or the modified Eulerian term as it is now called) has been under accurate observation for a quarter of a century. The period can probably (though not certainly) be regarded as constant. This is what we should expect, for a change in this period would call for a sensible alteration in the distribution of the material within the earth, or a change in the rigidity of the earth. The amplitude of this term presents a very puzzling problem. Its usual value is about 0".27, but twice in recent years it has jumped to about 0".40. Such a change could be accounted for by supposing that the earth had received a severe blow or a succession of milder blows tending in the same direction. We are reminded that both Milne and Helmert have suggested that there might be a direct connection between latitude variations and earthquakes. This suggestion was originally made by Milne very early in this century when the astronomical data necessary to test it were still very meager. It is to be hoped that the question will be taken up again in the light of the information that has been added during the past ten or twelve years.

Though the Eulerian term is the largest part of the latitude variation, it is by no means the only important one. We have next an annual term with a maximum amplitude of about 0".20. We may say with some confidence that this term is seasonal and meteorological in its origin, but at present no more definite statement would be warranted. It was early suggested that ocean currents might cause this variation. These currents would have to vary greatly with the season, either in the volume or the speed of the flow, or in

its direction; for an unvarying current would merely modify the Eulerian term once for all and would leave the latitude variations otherwise unchanged. A similar suggestion has been made with regard to air currents, and appeal has also been made to unequal deposits of snow and ice on two opposite hemispheres of the earth, to account for the annual term. It seems to me that these explanations have not been subjected to the critical numerical tests that are possible and desirable. The meteorological data are doubtless competent to enable us to compute at least the order of the effects in the latitude variations that we should expect from these various causes. Furthermore the annual term is probably variable in its amplitude, and it is important to ascertain how (if at all) these changes are related to the corresponding meteorological observations.

One other term must be mentioned in this brief summary. A few years ago Kimura of Japan made the important discovery (the most striking contribution to astronomy that has ever come out of Asia) that the latitudes of all stations are affected by a variation that does not depend upon the longitude but which is the same for all points in the same latitude. In other words there is present a variation that is not due to the wanderings of the pole. To ascertain more closely the nature of this term, the International Geodetic Association extended its latitude service temporarily to the southern hemisphere, with the result that the term was found to be of precisely the kind that would be caused by an annual wandering of the center of gravity of the earth to and fro along the axis of rotation. This must be regarded merely as an illustration and not as an explanation, for so great a change (about three meters) in the position of the center of gravity is excluded on other and very conclusive grounds. No plausible explanation for the Kimura term has as yet made its appearance, and as a consequence the reality of the term has been questioned from every possible point of view. Many explanations have been advanced, each of which sought to account for the term as merely an instrumental effect or the like, just as was the case twenty years earlier with the whole of the latitude variation itself. Against such attempts the Kimura term has

held up very well. It is not too much to say that at the present time all but one of the numerous explanations of this class have been disposed of; this exception deserves a brief mention, particularly as it calls loudly for the attention of the meteorologist. Let us suppose that the layers of equal density in the atmosphere above a station are not horizontal, but that they are sensibly inclined. If this occurs without our knowledge, as it would under ordinary circumstances, then we should apply refraction to our observations in a slightly erroneous way and we should derive a value for the latitude that is not quite correct. Let us suppose further that this effect were a world-wide one and that in any given month there would be a pronounced tendency for the inclination to be in the same sense in all latitudes, north and south, as well as in all longitudes. Then we should have a set of circumstances that would account for the Kimura term as an atmospheric effect, and therefore it would be excluded as a real variation of latitude. So far as the astronomer is able to testify, the evidence is against the occurrence of such tilts in the atmosphere. The inclination required to account quantitatively for the amplitude of the Kimura term is over two minutes of arc, or a slope of about one part in fifteen hundred. Presumably in a few years we shall be able to say something more definite as to the possibility of the existence of such conditions. My own opinion is that this explanation, like so many others of similar character that have been suggested for the Kimura term, will be found untenable. Further I venture to think that latitude variations as a whole will find their explanations less on the surface of the earth and more in its interior than seems now to be the generally accepted opinion.

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A PRACTICAL RATIONAL ALPHABET.

By BENJAMIN SMITH LYMAN.

(*Read October 1, 1915.*)

How to reform English orthography, and reduce it to simple regularity is an interesting problem. Repeated efforts have been persistently made in that direction. Among others, overhasty enthusiasts, in their disgust at the irregularities and phonetic inadequacies of the established English spelling, have insisted that a comparatively few of the most glaring irregularities should be "simplified" at once, hoping that later on another larger batch of "corrections" may be adopted. Of course, such alterations from the established usage can only come gradually into general, or established, use; not in less than fifty or seventy years, as may be seen in the few small changes urged by Noah Webster. Meanwhile, if the alterations meet with somewhat wide acceptance, there must be, on the whole, very greatly increased irregularity in English spelling, approaching, indeed, chaotic lawlessness. The repetition, and thereby prolongation of this painful unruly condition of our orthography in such an ill-considered effort at reform must remind one of the pretended humanity of cutting off a dog's tail by stages of an inch at a time. Would it not be far better to devise a practical and thoroughgoing system of orthography to be used alongside of the present established usage; and to become more and more used, until at last, it may become altogether adopted and universally used?

There are serious difficulties, however, in setting up a practical and thoroughgoing system of orthography. Any plan of reformed orthography should never fail to keep in mind the necessity of being thoroughly practical, if the least hope be entertained of its coming into universal, or even common, use. The great, widespread vogue of the Roman alphabet is doubtless due to its even rude simplicity; and in many hundred years it has been impossible to introduce into general use more than a very few extremely simple modifications of

the original forms of the letters: as for instance the carvilium to distinguish G from C and the distinction between J and I and between U and V, which appear to be still struggling for complete prevalence. It may, however, be borne in mind that notable additions to the Arabic alphabet have been made and accepted in order to express additional sounds in Persian or other languages: but it is noticeable that such added forms are strictly in keeping with the original character of the alphabet. The Russians have also strongly modified the Roman alphabet, and not always quite in keeping with the rude simplicity of its general character; yet have established its use throughout a great empire. In proposing new forms of letters for newly distinguished sounds, it is certainly advisable to maintain some restraint upon one's fancy, to adhere to the utmost simplicity, and to depart as little as possible from the general character of bare simplicity of the Roman alphabet, making use, so far as possible, of old devices, and putting forward as few novelties as possible, to be learned and made familiar. It seems highly desirable to avoid the use of altogether outlandish forms like the fully obsolete old Anglo-Saxon letters, wholly out of keeping with our modern alphabet; or to offend the eye by intermixing italic letters with Roman and by other tasteless similar devices, or by interspersing inverted letters, though to be sure of good Roman shape. Above all, however, let us avoid separate diacritical marks to distinguish sounds, marks that are a nuisance to write, an obscurity to read, and by their occasional forgetful omission a fruitful source of misleading. Especially the use of diacritical marks in a way opposed to their time-honored significance, is to be reprehended; as for example, the use of an accent to indicate merely the length of a vowel. Such practice has misled commonly into various errors of pronunciation of some oriental words. We shall see if there be any serious difficulty in getting handsomely along without any of those hastily, inconsiderately adopted, tempting, shallow, easy, but terrible, makeshifts. There are some restraints, or guides, which must cogently influence our choice of letters or symbols to be used in indicating the different sounds of the language. It is highly desirable, or absolutely necessary, that each sound should be indicated by only one letter, and that each letter should have but one sound; and it would

be absurd to acknowledge that principle, and then as in Volapuek and Esperanto, at the very outset give to *z* the sound of two letters, *ts*, merely because it happens to have those sounds in German. Another important principle is to give to letters or devices the force that they already have, and long have had, in the languages where they have been in use. In general, the customary practice of the majority should have sway, requiring the minimum of new learning. As English is far and away the most numerously spoken language throughout the world, the sounds to be attributed to the consonant letters should be as in English; though, owing to the extreme irregularity and variety of the English vowel letters, they must give place to letters that are more prevalent in the other European languages. The English consonant *y*, for example, should be used; not, as in Esperanto, the letter *j*, which has that sound among the comparatively small number who use German and Italian. In Volapuek, *j* is made to serve for the English *sh*, a most unheard-of use.

In English, the combinations *ch*, *sh*, *th* and *wh* each is used for a single sound, and it is desirable to substitute for it a single letter. Would it not be highly practical to write those sounds by means, in each case, of merely the first of the two letters with a subscript small appendage somewhat similar to the old device of the French cedilla, though a little different in form, to represent the letter *h*, and having a more or less distant resemblance to it in shape? In cursive writing, the resemblance to an *h* need not by any means be close, and may be really abbreviated, as there would be no danger of misunderstanding. We have, thereby, four new characters with but a single device to remember, and that not a new one, and the new forms are entirely in keeping with our old alphabet and with already customary methods. As to the sound of *ch* in *church*, it is sometimes maintained that it is in reality a sound compounded of *t* followed by *sh*. But that is clearly an error; for even the ear can distinguish a difference in the sounds, and the sound of *ch* is as distinctly different as is the sound of the opening or closing of a somewhat tightly swollen door, compared to the mild clapping to of a well-fitting closure. The peculiarity of the contact of the tongue and roof of the mouth, with the consequent vibrations of the roof of the mouth, occasions a peculiar sound different from *t* and from

sh. A corresponding difference occurs between the sound of a smack with the lips and *p* or *b*. The sound of *zh*, as in pleasure, would, of course, be indicated by *z* with a subscript *h*. If it be desired (unlike ordinary English) to distinguish the sound of *th* in *this* from that in *thin*, the logically analogous and simple mode of writing it would be with a *d* with a subscript *h*. The whispered, or surd, *y*, heard in the word *hue*, might also be indicated by a *y* with a subscript *h*. The guttural sounds indicated in oriental transliteration by *kh* and *gh*, would likewise be represented by *k* or *g* with a subscript *h*. Until types of these new forms are to be had, we may provisionally, instead of the subscript *h*, use a small *h* at the side: c_h, s_h, t_h, d_h, k_h, g_h, w_h, y_h. The simple sound written in English with *ng* should be indicated (as proposed so long ago as Benjamin Franklin) by a character similar to a *g* but with the upper part in the form of an *n*, for which there is already type.

Other consonant sounds, the so-called cerebral sounds, occurring, for example, in the Sanscrit and in the dialect of Peking, could be simply indicated in a similar manner, by giving to the upper part of the corresponding letter the shape of an *r*; since those sounds are made with the tongue rolled up, as for an *r*. In Sanscrit, such a modification of *sh* occurs and in the Peking dialect *y* is so pronounced, with the tongue rolled up, and may be indicated by a *y* with the upper right hand fork in the shape of an *r* (provisionally s_h^r, and y^r).

With these four or five simple characters, we have then a full supply of consonants without going outside of the ordinary English usage; *b, c, ch, d, dh, f, g* (always as in *give, get*), *j, k, l, m, n, p, r, s, sh, t, th, v, w, y, z, zh;* omitting *q*, and *x*, as superfluous; and using *c*, only with the subscript *h*. Indeed as the *c* is only so used, even if the subscript *h* should be omitted there would be no danger of confusion, and *c* would have before all vowels the same sound that it has in Italian before *e*, and *i*. *H* is sometimes reckoned as a consonant, but, of course, erroneously, as it is the whispered form of the vowel that follows it.

As already intimated, order out of the chaos of English vowels is only to be attained by adopting the more uniform practice of the European continental countries, with *a*, as in *arm*, *o* as in *note*, *u* as

in *rule, i* as in *pique, e* as in *they;* and, for the vowels, we must abandon the hope of indicating by a separate character every one of the infinite number of shades of sound, a few of which occur in such series of vowels as in: *hate, hale, hare, hairy, Harry, hal, hat.* The progress of enlightenment in thousands of years has led to far greater nicety of distinction in vowel sounds than was common formerly. But instead of five or six vowels that it was then found worth while to indicate by separate characters, it would now be hardly practical to have distinct letters for more than eighteen or twenty vowels and that number may be very practically arranged.

A difficulty in bringing into general use any such somewhat nicely adjusted system of indicating the sounds, especially the vowel sounds, of any language is that the pronunciation of words is different in different regions and even among different families and individuals of the same region; nay, even with the same individual according to varying emphasis in different connections, as *to* in "going to Boston," and "to and fro" and the pronunciation sometimes varies through slackness or slovenliness of articulation or enunciation, as in substituting a slight vowel sound for the consonants *y* and *w* in such words as *they* and *snow,* or in dropping *r* altogether after a vowel and before a consonant, as in *arm.* Hence strict regard to phonetics would give the same word several different forms according to the taste or habits of different writers, and stand seriously in the way of the uniformity of spelling that would be extremely desirable for at least a literary language to be used in common by a numerous people.

As regards the vowels Professor Samuel Porter over forty-eight years ago, in the *American Journal of Science,* September, 1866, excellently classified the readily distinguishable vowel sounds of English and other principal European languages, and arranged them according to their physiological mode of formation, with a simple illustration indicating nine different parts of the mouth where the tongue is placed to give the form of cavity, which with the issuing breath, will produce each vowel sound. So simple are the plan and the illustration that they have been perfectly successful in inducing very ignorant Orientals (in India and China) to indicate thoroughly and simply the mode of formation of some of their most

peculiar sounds, which to ordinary foreigners without Porter's help, and with merely the ear as a guide, are mysterious and even considered quite unattainable. He distinguishes nine points at which the tongue is placed, and at each of those points, four degrees of openness; making thereby thirty-six readily distinguishable vowels. But a number of them are not in ordinary use, and are therefore not to be considered in any orthographic scheme. A few additions are to be made on account of the effect of stiffening the lips, changing the sound. In order to accommodate ourselves to this classification of the vowels it is desirable to add to our letters æ (not a new combination) as ae in German *Maedchen,* for the sound of *a* in *care;* and oe (again not new), nearly like the oe in German *schoen* for certain closely allied sounds; and a new character, like the Swedish *a,* with an *o* over it; but contracted into a single form, for the sounds, like *a* in *war,* or *o* in *lord,* or *oa* in *broad.* Yet another new form may be added, *e* with a stroke like an accent just to its left, to correspond with the French acute-accented *é.* We have, then, nine characters for Porter's nine groups of four vowels each. He calls attention to the fact that in each group of four vowels, differing only in the degree of closeness of the tongue at the same place in the mouth, two of the four are long and two short. Let us therefore represent the long vowels by the ancient device of simply doubling (with slight contraction) the letter used for the short vowels, as the Greeks already set us the example with their omega. All the vowels can in like manner be doubled, and somewhat contracted, making at once eighteen easily written and easily read vowels conforming well to the already established character of our alphabet. Until appropriate type for the purpose are to be had, we might provisionally merely double the present letters; as: aa, ee, etc. In one or two cases the number can be increased by indicating a labial modification of the vowel by means of a small upright stroke, an abbreviated *l* (provisionally a small *l*), close to the right hand of the letter. In this way, we are easily provided with about twenty vowels, apparently an ample supply for the English language.

Let us now consider the vowels one by one, more particularly. In group I, the *a* of *last, ask, chant,* is short; while that of *father*

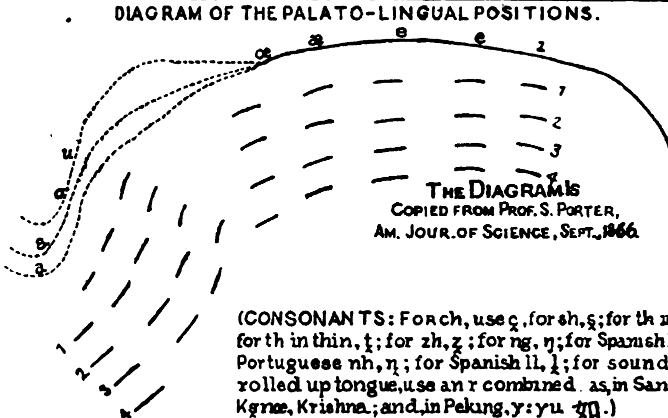
and *calm* is long; and that of *baa, ah, arm, charge* is still broader. The two last would therefore be written with a double letter (provisionally aa); and there would be no need to distinguish in writing between these two, because there is distinction enough in the following *r* or *h*.

In group II, the two closer vowels, as (long) in *war, lord, awe, pause,* or (shorter) *all, water, long, daughter,* are both labially modified, by stiffening the lips; and can be so indicated by means of a small upright stroke (an abbreviated *l,* provisionally a small *l*) just to the right of the letter. The longer vowel can be indicated by doubling, as already described. The shorter and not labially modified vowel of the second degree of openness is heard in the words *salt, although, cross, horror;* and the third degree of openness, also not labially modified, occurs in *sod, nor, off, what, knowledge;* and may be written with an *a* combined with an *o,* like the corresponding Swedish letter, but more contracted. These two closely similar vowel sounds, scarcely distinguishable by ordinary ears, it seems hardly worth while to provide with separate letters (though the distinction of the third degree might be marked by a small 3 just to the right of the letter). The fourth degree of openness does not occur in ordinary speech.

In group III, in like manner, the least open vowel, as in *note, toe, low, loaf, door, mourn,* being longer, may be written with a double letter (like the Greek omega), or, provisionally, by a repetition of the single letter, *oo*; and might be marked as labially modified, in the way already indicated. But this is hardly necessary, because, in English, it always has that modification, making it unnecessary to mark it. The next degree of openness is likewise always labially modified, and being short would be written with a single letter. It is also distinguished by being an unaccented vowel. The third degree of openness, as in *not, dot, folly, knock, proper, bite, eye* (*oy,* a short *o* followed by the consonant *y*) occurs only in accented syllables, and is thereby sufficiently distinguished.

In group IV, the long sound of the vowel in *rule, sure, fool, pool, moon, shoe, soup,* would be written with a double vowel (provisionally by *uu*), while the vowel of the second degree of openness, as in *full, pull, bosom, woman, should, good, foot, book,* would be

DIAGRAM OF THE PALATO-LINGUAL POSITIONS.



THE DIAGRAM IS COPIED FROM PROF. S. PORTER, AM. JOUR. OF SCIENCE, SEPT., 1866.

(CONSONANTS: For ch, use ç, for sh, ş; for th in this, đ; for th in thin, ţ; for zh, z̧; for ng, ŋ; for Spanish ñ, and Portuguese nh, ɳ; for Spanish ll, ļ; for sounds with rolled up tongue, use an r combined as, in Sanscrit, ʂ: Kşnœ, Krishna; and, in Peking, y: yu 𝔶𝔲.)

VOWELS.

I A, A, a, a, a, a, A, A
1. as in last, ask, chant: last, ask, çant.
2. as in father, calm: father, kaam.
3. as in baa, ah, arm, charge: baa, a, aarm, çaarj
4. (Not in good English.)

II. A. A, a, a, A, A, a, a.
1. as in awe, war, lord, pause: aw, waar, lard, paz, (labially modified—lips stiff.)
2. as in all, water, long, daughter: al, water, lang, dater, (labial.)
2. as in salt, although, cross, horror: salt, aldo, kras, har œr.
3. as in sod, nor, off, what, knowledge: sad, nar, af, what, nalej.
4. (Not in good English.)

III. O, O, o, o, O, O, o, o.
1. as in note, toe, low, loaf, door, mourn, beau. noot, too, loo, loof, door, moorn, boo.
2. as in opinion, agony, propose, mellow: opinyœn, ægone, propooz, melo (unaccented).
3. as in not, dot, folly, knock, proper, eye, bite: not, dot, fole, nok, propœr, oy, boyt.
4. (Not in good English.)

IV. U, U, u, u, U, U, u, u
1. as in rule, sure, fool, pool, moon, move, shoe: ruul, şuur, fuul, puul, muun, muuv, şuu.
2. as in full, pull, bosom, woman, should, good: ful, pul, buzœm, wumœn, şud, gud.
3. as in fulfil, willful: fulfil, wilful—(unaccented).
4. (Not in good English.)

V. Œ, Œ, œ, œ, Œ, Œ, œ, œ.
1. (Not in English—the German œ, French eu.)
2. as in girl, virtue, mercy, myrtle, earl: gœrl, vœrtyu, mœrse, mœrtl, œrl—(before r).
3. as in up, but, cousin, rough, dove, done: œp, bœt, kœzin, rœf, dœv, dœn.
4. as in burr, church, work: bœœr, çœœrç, wœœrk—(before r, accented).

VI. Æ, Æ, æ, æ, Æ, Æ, æ, æ.
1. as in their, fair, parent: đæær, fæær, pæærent.
2. as in care, there, prayer, hair, pair: kær, đær, prær, hær, pær—(before r).
3. as in cat, man, sad, hap: kæt, mæn, sæd, hæp—(accented, no r).
4. (Not in good English.)

VII. E, E, e, e, E, E, e, e.
1. as in they, grey, vein, great, name, fate: đey, grey, veyn, greyt, neym, feyt.
2. as in nitrate, climate: noytret, kloymet—(unaccented).
3. as in get, egg, red, mend: get, eg, red, mend—(accented).
4. (Not in English.)

VIII. E, e, E, e.
1. (Not in English—the French é.)
2. as in guinea, valley, carried, city: gine, væle, kæred, cite—(unaccent'd).
3. as in goodness, college: gudnes, kalej—(unaccented).
4. (Not in English.)

IX. I, I, i, i, I, I, i, i.
1. as in pique, machine, field, eat, eve, deep: piik, maşiin, fiild, iit, iiv, diip.
2. as in divine, vehicle, mandarin: divoyn, viihikl, mændarin—(unaccented).
3. as in pin, hit, sin, will: pin, hit, sin, will—(accented).
4. (Not in good English.)—(The French u would be ü.)

(Until proper type can be had, use double letters for long vowels.) B.S.L.

written with a single letter. Both these vowels are labially modified, and might be so marked, in the way already indicated, but it is unnecessary so to mark them, because there is no vowel in English with which they could be confounded. In the third degree of openness, the unaccented vowels in *fulfill,* and *willful,* occur; but (written with a single letter) are sufficiently distinguished by the absence of accent. The fourth degree of openness does not occur in good English.

In group V, the first and second degrees of openness, occur in the German oe, and the French eu (nearly, though not quite, the same); but not in English. The second degree of openness without labial modification occurs in English only before *r* as in *mercy, virtue, girl, myrtle, earl, pearl, earth;* and may be written with a single letter (œ). In the third degree of openness, likewise short, and to be written with a single letter, occurs the so-called natural vowel, accented, and without *r,* as in *up, but.* In the fourth degree (written with a double vowel), long, occurs before *r* the vowel sound of *burr, occur.*

In group VI, the long sound, with a double letter (provisionally, the single letter repeated, æ æ), is heard as the *a* in *parent, ei* in *their, ai* in *fair.* It is the German *ae* in *Maedchen,* and the French *è* in *après, scène, père.* The second degree of openness, with a single letter, is heard in *care, there, prayer, heir, pair;* in each case followed by the sound *r.* Without that sound of *r,* the third degree of openness gives us, with the same letter, the *a* in *at, cat, man, sad, hap.* The absence of the *r* makes it unnecessary for them to distinguish the two slightly different vowels.

In group VII, the first degree of openness with a double letter, or, provisionally, the single letter repeated, *ee,* gives us the *e* in *they, grey,* and the like sounds in *fate, name, great, vein, hail, pay;* the German *mehr, jeder, ledig, See.* The second degree of openness, with a single letter, gives us the *a* of unaccented syllables, as in *nitrate, climate.* The third degree of openness, with the same single letter, occurs in accented syllables, as in *get, egg, red, mend.* The fourth degree does not occur in English.

In group VIII, the first and fourth degree of openness do not occur in English. The first one, to be written with a double letter,

occurs in the French acute-accented *é* and *ai*. The second degree of openness (written with a single letter, provisionally, ^{1}e, an *e* with a small upright mark, or figure 1, above at its left) occurs in English in unaccented syllables only, as in *guinea, valley, carried, city*. The third degree of openness (likewise a single letter) differs so slightly from the second as hardly to need a separate character, though it might be marked with a small abbreviated 3 put to the right and upper part of the letter *e*. It occurs in the unaccented syllables *goodness, college*.

In group IX, the first degree of openness, to be marked with a double letter (provisionally, *ii*), is found in the *i* of *pique, machine*. When this is labially modified by stiffening the lips, it becomes the French *u*, as in *ruse*, and the German *ue*, as in *ueber*, to be marked with a small stroke, an abbreviated *l*, at the right of the letter. The second degree of openness, to be marked by a single letter, occurs in unaccented syllables as in *divine, vehicle, mitigate*. The fourth degree of openness does not occur in English.

We have, then, for the vowels nineteen letters; distinguishing all the readily distinguishable vowels used in English. In two or three cases the distinction is indicated by the accent as in certain unaccented syllables, as in *fulfill, goodness;* and in other cases by the subsequence of the sound *r*, as in *girl*. Even these slight differences could be indicated by a scrupulous writer with an abbreviated figure *3* alongside, to the right, and at the upper corner, of the letter.

Having thus made possible the writing of English with unmistakable letters, each letter for a single sound, and each readily distinguished sound by a single letter, a strong reason is advanced in favor of the general adoption of English as a universal language. Indeed, it is ardently to be hoped that eventually some one language may become universal, and known to the whole human race. Latin was formerly so widely known and extensively used among the more civilized nations as to give some color to its claim to become the universal language. But the gradually increased refinement of ideas in modern times has apparently made it impossible to be satisfied with so bald and rude a method of communication. The numerous artificial languages proposed for this purpose, even if not

liable to the same objection, or to greater crudity, are yet additional languages to be learned. English already known to a much larger number of men than any other language, seems to be, by all odds, the best adapted to become, perhaps with slight modifications, a universal language. The simplicity of its grammar, aside from orthography, makes it remarkably easy for foreigners to learn; and, for use in universal form, the comparatively few irregularities of grammar might considerately be eliminated, so that (in universal form) it might be allowed to say mouses, instead of mice, and digged instead of dug. English has already shown its capacity to express perfectly the finest distinctions of ideas and must in that respect far excel any artificial language, like Esperanto, or Volapuek, with their rude, bald, lack, for example, of the definite or indefinite articles. A rational, phonetic, practical spelling would, then, make English ideally perfect for a universal language. Clearly, for that purpose, the usage of speakers of some region, or of some degree of cultivation, with some degree of emphasis, must be selected as the norm to which the written language should conform, in order to make the writing and spelling in the main, though not always in every minute detail, phonetic. Well taught children should, then, everywhere learn to pronounce the words as they are spelled, and not be allowed to drop the sound of *r* in arm, or pervert the sound of the English long *u* (like *yu,* except after the sound of *ch, j, r, sh, zh,* or *y*). Normal schools should train teachers in these details so that the children may be properly drilled. In that way the language would be rightly conserved, and would tend to become fit for universal use.

One serious difficulty in the adoption of any such improvements of our alphabet is that there are so many men who excel more in persuasive eloquence, in "the gift of the gab," than in a thorough knowledge of phonetics and inclination to careful reflection. Cadmus could not have been a ready tongued, shallow utterer of rapidly up-bubbling superficial thoughts. A group, or committee, or society of such quick-witted individuals (perhaps some of them so densely ignorant as to suppose *h* to be a consonant, instead of the whispered, or surd, form of its following vowel, or to insist that the English *ch,* and *j* are compounded of sounds distinguishable even by the ear, and as much unlike the real ones as the bursting open, or banging shut of a tightly swollen door, is to the mild clapping to or open-

ing of a well-fitting closure), may make bold to put forward by their majority vote some alphabetic, or orthographic, system (as the Japanese Roman Letter Society did), and may really delay for a long time the adoption of an altogether rational and practical method. It would be much better for individuals to propose their own plans, and put them into use by themselves and by a portion of the public. Gradually, the best of such plans would take the lead, and come into more and more general use, without having to overcome at the outset the prestige of the dominant approval of a high-sounding society or committee. In any case, it would clearly take many years for such a rational new system fully to supplant the present established usage.

Meantime, it might be advisable to do something towards simplifying the learning of the present established spelling. To be sure, the difficulty of learning it has been much exaggerated, owing to the general extreme neglect of the study. It seems, however, possible that the six weeks or so that appears to be ample for a half-grown boy or girl to learn to spell well might be reduced to a couple of weeks, at most, with a properly arranged booklet; so that the present multitudinous army of typists might readily fit themselves to avoid tormenting their employers by ignorance of so simple an art as spelling.

But however advantageous a simple, purely phonetic spelling might be to a defectively educated typist, or to an adult foreigner, let it not by any means be imagined that the time spent by children in acquiring our more complicated established orthography is uselessly thrown away. On the contrary, it is a highly useful discipline, not only training the memory in a simple way, well adapted to young children, but giving most valuable habits of close and accurate minute observation (the precision that is the most efficient aid to the conservation of language), and enabling the easy understanding and remembering of the proper mode of writing a new word or name. Such habits may also be acquired by certain games of children, but in a way not a whit more interesting or "useful" than the old-fashioned spelling match. The comparatively recent way of teaching to read by the general appearance of the word, and with total neglect of syllabical spelling, is detestable, and produces results that are full of torture and disgust to those who have to listen to such reading.

THE CAMBRIAN MANGANESE DEPOSITS OF CONCEPTION AND TRINITY BAYS, NEWFOUNDLAND.

By NELSON C. DALE.

(*Read April 25, 1914*)

CONTENTS.

I. INTRODUCTION.

This paper is based upon data collected during the summers of 1912 and 1913. The former season, Mr. A. O. Hayes and Prof. van Ingen of Princeton University, while making a study of the general geology, stratigraphy, and palæontology of the shores of Conception Bay, Newfoundland, in connection with the investigation of the iron ores of Great Bell Island, came upon the manganiferous rocks of the Lower Cambrian exposed at Manuels, Topsail, Brigus, and other places. They were immediately struck by the unusual lithological and mineralogical characteristics and by the excellent state of preservation, particularly at Manuels, of what are undoubtedly primary

bedded deposits. Some collections and notes then taken of these interesting rocks were later placed at the disposal of the writer for further investigation. The following summer of 1913, the writer as a member of the Princeton Newfoundland Expedition undertook a more detailed study of these deposits at the various localities where the manganese had been found the preceding summer, and also of a deposit of the same age on the northern shore of Trinity Bay.

There are so few syngenetic manganese deposits which still retain their primary unaltered characters and are found to occur at the same horizon over such a wide area that a somewhat detailed investigation gave promise of yielding results of value. In this paper therefore there has been an attempt to present as comprehensive a study of the manganese of southeastern Newfoundland as our knowledge of this hitherto but little investigated region will allow.

The subject matter is primarily chemical in its nature and the analyses herewith presented are from samples taken from the principal manganese-bearing beds. Many more analyses however could have been made and in fact many more should be made if the deposits are to be seriously investigated for commercial purposes. The analyses of the manganese beds at Manuels, Topsail, and Smith Point, Newfoundland and those of the imported specimens from Elbingerode, Saxony were made by the writer in the chemical laboratory of the geological department of Princeton University.

Because of the impalpable fineness of grain of many of the manganese-bearing beds, the petrographical descriptions of certain of the thin sections can deal only with the larger features such as structure, mineral aggregations, and a few of the larger and observable minerals.

The writer feels particularly indebted to Prof. C. H. Smyth, Jr., for many helpful suggestions bearing upon the chemical side of the investigation, and to Prof. G. van Ingen for unpublished information regarding the stratigraphy and palæontology of this region, as well as for the loan of the locality maps and data for the columnar sections which are the results of careful surveys made during the summers of 1912 and 1913. All photographs and microphotographs were generously contributed by Prof. van Ingen to further the presentation of the results of this investigation.

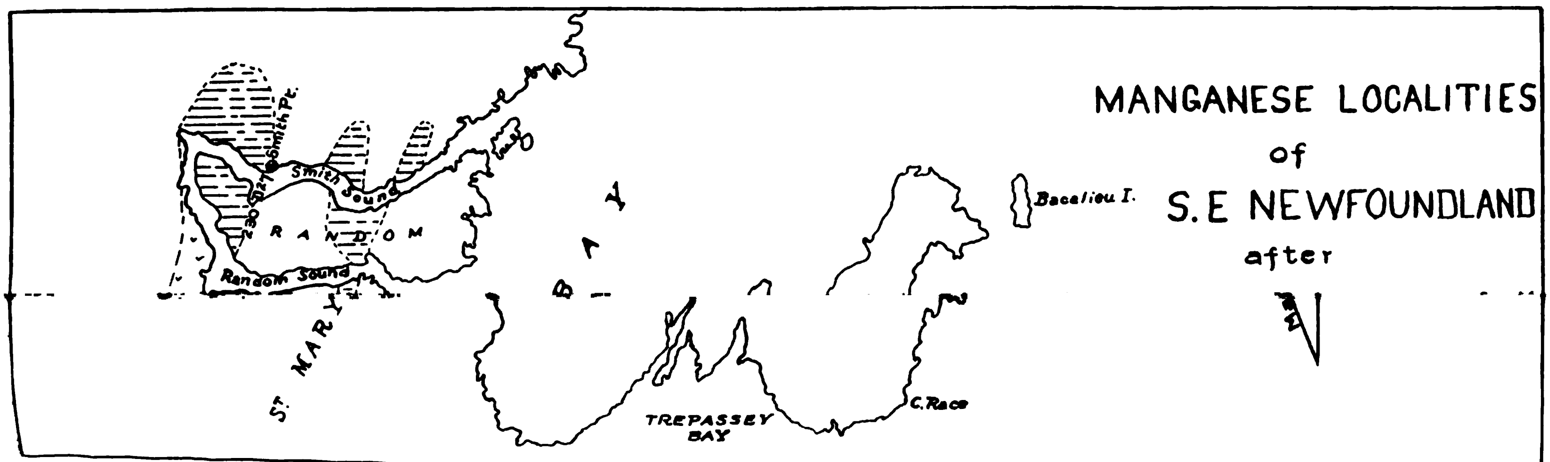


FIG. 1. Map showing manganese localities of southeastern Newfoundland, based on Geological Map of Newfoundland by [illegible] 1907.

II. GENERAL GEOGRAPHIC AND GEOLOGIC RELATIONS OF THE MANGANESE DEPOSITS OF SOUTHEASTERN NEWFOUNDLAND.

Geographic Relations.

The manganese deposits here considered are located in the southeastern part of Newfoundland in the vicinity of Topsail, Manuels, Long Pond, Chapel Cove, and Brigus on Conception Bay, and at Smith Point on Trinity Bay. Manganese is also said to occur near Ships Cove, Placentia Bay. The accompanying map, Fig. 1, shows the approximate location of these deposits.

General Geology.

The sedimentary rocks of this area are included in the Cambrian and Ordovician systems and may be seen on the map (Fig. 1) to occur as irregular patches, the Ordovician composing the larger islands of the bays and the Cambrian occurring as irregular and widely separated fringes resting on the pre-Cambrian of the mainland. The whole series consists almost wholly of shales and thin-bedded sandstones with some limestones, and in the base of the lower Cambrian an occasional conglomeratic bed.

The iron ores of Great Bell Island are Arenig while the manganese and their associated green and red shales are of late lower Cambrian.

Wherever the Cambrian strata have been found in contact with the pre-Cambrian an unconformable relationship exists. The pre-Cambrian rocks of this area as classified by Dr. Walcott (2:219) and by Messrs. Murray and Howley (18:141–154) respectively are as follows:

	Walcott	Murray and Howley
	Random	
Avalonian	Signal Hill	Avalonian
	Momable	
	Torbay	
	Conception	Huronian
	Laurentian	Archaean

The Avalonian and Huronian of Mr. Howley represent a thickness of 12,370 feet. A later unpublished estimate of 18,250 feet has

been made by Mr. A. F. Buddington, who is studying the pre-Cambrian rocks of this region. A brief description of these formations at this time will be necessary for a comprehensive view of the Newfoundland manganese deposits.

Laurentian: The rocks of this formation are in great part gneissic and granitoid, and are probably the oldest rocks of the area.

Huronian: This formation, which is equivalent to the "Conception" of Dr. Walcott, consists principally of the Conception slates which are of tufaceous marine origin. They are intruded by bosses and dikes of granite, diorite, monzonite, and gabbro, and contain basaltic and rhyolite flows. The Conception formation was estimated by Murray and Howley to have a thickness of 2,950 feet.

Torbay: This formation consists of about 3,300 feet of green and purple slates and argillites.

Momable: An estimated thickness of 2,000 feet of brown and black sandy shales overlies the previous formation.

Signal Hill: Red and green sandstones, conglomerates, shales, and arkoses largely of continental origin comprise this formation, the thickness of which is about 9,000 feet according to an unpublished estimate by Mr. A. F. Buddington.

Random: About 1,000 feet of green and red sandstones and white quartzites with occasional basalt flows comprise this series.

Murray and Howley in their report of 1868 for the Geological Survey of Newfoundland describe the general structural features of the Avalon Peninsula as follows:

"The region in question, in particular, and probably the whole island in general, seems to be arranged in an alternation of anticlinal and synclinal lines, independent of innumerable minor folds, which preserve throughout a remarkable degree of parallelism, pointing generally about N-NE and S-SW from the true meridian, corresponding with the strongly marked indentations of the coast as well as the topographical features of the interior. One such great anticlinal form occurs within the region examined this year, with a corresponding synclinal; the axis of the former was found to be more or less overlaid unconformably by rocks containing fossils of Lower Silurian age, none of which were of less remote antiquity than such as are attributed to the horizon of the upper Potsdam group."

"The axis of this anticlinal runs in a moderately straight line from Cape Pine on the south coast to that part of the Peninsula and coming up from below the Intermediate Series, occupies more or less of the surface from the vicinity of the Renew's Butterpots to the shores of Conception Bay be-

tween Holyrood and Manuels Brook. The newer or Great Intermediate Series which flanks this Laurentian Nucleus, was found on the Peninsula of St. Johns and Ferryland to show a general dip to the eastward although making many minor undulations; while on the Peninsula between Conception and Trinity bays the inclination is reversed, being nearly uniformly westerly, making many repetitions of the same strata however, as on the opposite side of the fold. Corresponding with this great anticlinal, the measure of the Intermediate rocks, as seen at parts of the eastern coast of Placentia Bay, appear, by the generally eastern dip which they present, to indicate the axis of a synclinal trough to run from Trinity Bay in the direction of St. Mary's Bay."

As structural work of a reconnaissance nature only has thus far been published in reference to Newfoundland it is hoped that this most interesting phase of geology of the island may be investigated in the near future. The following locality descriptions will take up briefly these smaller structural features which may serve as a clue to the more general structures of the entire manganese area.

III. GENERAL STRATIGRAPHY

There is very little published information regarding the general stratigraphy of the region under consideration but a few observations made while studying the individual manganese deposits and other information verbally communicated by Prof. van Ingen may be of interest at this point.

One of the most striking features of the manganese deposits is their occurrence at the same horizon in shales of late lower Cambrian age at widely separated points on Conception and Trinity Bays. At each deposit, the manganese zone was found to occur below the Paradoxides zone. At Manuels in the shales directly below the manganese nodular beds, heads of Protolenus harveyi (oral communication by G. van Ingen) were found so that in all probability the manganese beds may be included in the Protolenus zone of Matthews (16: 101–153).

By referring to the columnar sections (Figs. 2, 36, 42, and 44) it is readily seen that the sediments consist largely of shales and limestones and that there is a very decided increase in the total thickness of the beds from Manuels where there are 215 feet between the bottom of the Paradoxides zone and the top of the pre-Cambrian to Smith Point, Trinity Bay, where over 1,000 feet intervene between

the Paradoxides zone and the pre-Cambrian. From the bottom of the Paradoxides zone at Smith Point to the top of the Smith Point limestone according to a calculation based upon a careful stadia transit survey of the shore line (Fig. 43) there is a thickness of 546 feet. The total thickness in the number of limestone beds varies from a few feet at Manuels to 100 + feet at Smith Point. The thickness of the shales at Manuels below the Paradoxides zone is about 200 feet while the thickness of the shales at Smith Point within the corresponding limits is over 400 feet, on the assumption that the Smith Point limestone of Trinity Bay corresponds to that limestone of the Manuels section which is just above the basal conglomerate.

The increase in total thickness of the number of beds from the east shore of Conception Bay to the west shore within the corresponding limits would indicate a deeper portion of the Cambrian sea when the sediments were being deposited. The fact that sediments found below the Smith Point limestone on Trinity Bay are not represented at Manuels would indicate that sedimentation had been going on for a longer time in the western portion of the basin than in the eastern. Whether there actually was a greater amount of sedimentation in that portion of the basin remains to be investigated.

As very little information is at hand with regard to the area of the Cambrian rocks, it is quite out of the question for the writer to attempt to outline the area once occupied by the Cambrian Sea in southeastern Newfoundland. Moreover, it is likewise impossible for the writer to outline the original manganese area as it looked in early Cambrian times. If manganese occurs on the eastern shore of Placentia Bay, as all descriptions of that occurrence seem to indicate, it would seem that the original area of the manganese was approximately 200 or 300 square miles, assuming a more or less oblong shape for the deposit.

Although the basal conglomerate at Manuels is evidence of a definite shore line for the Cambrian sea at that part of the basin, there is also evidence at the other localities examined, where, however, the basal conglomerate is not found in any such large development. There are littoral pre-Cambrian contacts at Topsail, Chapel Cove, and Brigus; all with typical shore deposits.

IV. DETAILED DESCRIPTIONS OF LOCALITIES

MANUELS.—Manganese is found as thin jasper-like bands of green and brown color, as nodular beds, and as argillaceous and calcareous beds interbedded with green and red shales of late lower Cambrian age. This mode of occurrence is very well shown in Manuels brook close by the village of Manuels. The geographic, geologic, and stratigraphic relations are shown in Figs. 1–3. The Cambrian at Manuels consists in the main of shales with thin bedded sandstones with conglomerate and thin limestones at its base and the sediments show practically no metamorphism throughout the series. The strike of the beds is N 82 E (true meridian) and the dip is 10 N. One of the best unconformable contacts in the manganese area is that in Manuels brook at Manuels where the basal Cambrian conglomerate lies upon the Huronian. For a more intimate acquaintance with the manganese occurrence a somewhat detailed description of the stratigraphy, lithology, mineralogy and petrography of the manganese beds and their associated strata will be necessary and therefore the individual beds of the section (Fig. 2) will be described in stratigraphical order.

210 A 1, Basal Conglomerate. The base of the Cambrian at Manuels is made up of coarse conglomerate, eighteen feet in thickness, consisting in the main of boulders and pebbles of igneous character. These boulders at the bottom of the bed, where the base of the Cambrian lies unconformably upon the Huronian, measure in some instances twelve feet in diameter, but they diminish in size toward the top to an inch or less. The matrix, of an arenaceous nature toward the bottom, grades into a more calcareous one at the top where the overlying stratum is a limestone.

219 D 1, limestone. This bed is a bluish fine-grained to pebbly argillaceous limestone of about 3 feet in thickness. The pebbles averaging a fraction of an inch in diameter are angular to subangular in shape and appear to be of igneous rocks. Pteropod shells chiefly of the genus *Coleoloides* abound. Microscopic examination proves this rock to be a semi-crystalline, fine to locally coarse grained limestone. The texture is very suggestive of organic forms, being an aggregate of elliptical bodies, possibly algal concretions [or "copro-

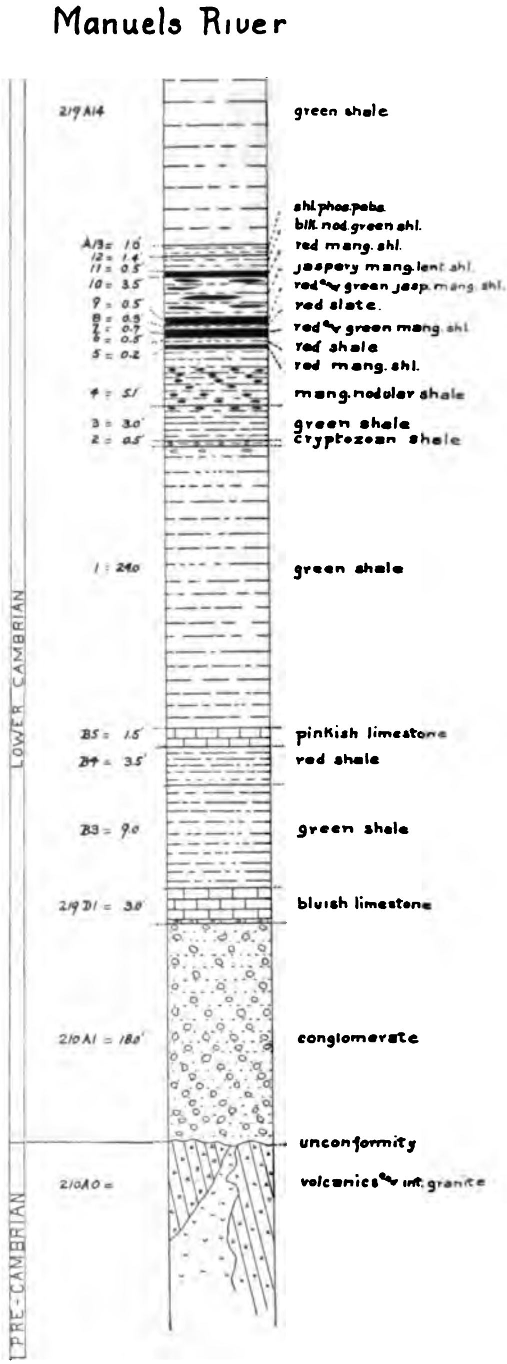


FIG. 2. Columnar section showing the details of the manganese zone in the Lower Cambrian of Manuels brook, 219 A and B.

FIG. 3. View of outcrop of manganese zone of late lower Cambrian age in left bank of Manuels brook.

lite ooze" similar to that described by Philippi from off the Congo mouths—G. van Ingen]. These bodies contain aggregations of carbonate material, probably calcite, which have no definite orientation. The section abounds with pteropod shell fragments, partially replaced with calcite. Calcite and carbonate material comprise the greater portion of the section but quartz occurs as infrequent local segregations and as irregular grains. Pyrite and hematite, as well as a few pink and brown stained areas which are possibly secondary products of manganese and iron, are sparingly present. No analysis was made of this rock but with the sodium carbonate and potassium nitrate bead test a manganese reaction was obtained. This bed is a bluish argillaceous manganiferous limestone.

219 B 3, overlying the limestone, is a brownish weathering olive green shale.

219 B 4 is a bed of red shale, the upper surface of which seems to be limey. The upper 2 inches of this bed has a wavy structure and is somewhat greenish in color. Microscopically the bed is found to be a hematitic shale with occasional grains of quartz and thin rectangular laths of feldspar. Magnetite and pyrite are found as irregular grains in sparing amounts.

219 B 5. With a sharp contact, the red shale is overlain by a 1.5 foot thick bed of nodular and pebbly reddish blue limestone. Because of marked lithological differences this bed has been divided into four smaller subdivisions which are lettered **a**, **b**, **c**, and **d**. Subdivision **a** consists of about 2 inches of green shale which is slightly calcareous. Subdivision **b** is a compact pinkish limestone containing pinkish or reddish mineral disseminations and occasional fragments of hyolithid and brachiopod shells. Microscopically this limestone is somewhat granular and crystalline, with calcite as the dominant anisotropic mineral. Quartz occurs occasionally. Hematite as an impalpable dust or pigment is abundant, bordering hyolithid fragments or as irregular accumulations. A fragment of probably organic substance with a cellular structure is a conspicuous feature of the slide. Sponge spicules replaced by calcite are noticeable.

Subdivision **c** differs not very much from the two members described but is nodular or pebbly and much more fossiliferous. Micro-

scopically this rock is a very fine grained semi-crystalline limestone. Calcite, frequently twinned, is the dominant mineral with quartz and chlorite in secondary importance. Barite occurs as occasional small and large irregular grains. Hematite is found bordering calcite grains and fossil fragments or replacing them, and as irregular accumulations. Pyrite is found occasionally. Certain nodular or pebbly forms, isotropic under crossed nicols, are, because of their fineness of grain, of an indeterminable nature.

A very noticeable feature of this section is the diversity of *Hyolithes* forms, some elliptical and concentric and others circular, either entirely or partially replaced by calcite or hematite. The circular forms measure .287 mm. in diameter (Fig. 4, Slide 250).

FIG. 4. Microphotograph of limestone, 219 B5c; slide 250; enlarged 22 diam. **a**, hyolithes with calcite and chlorite; **b**, calcite; **c**, quartz.

219 B 5d. The upper subdivision of this bed is of interest mainly on account of the mineral associations in the large nodules on its surface. Differential erosional effects between the limestone and nodule have resulted in a greater conspicuousness of the more resis-

tant nodule. The nodules, measuring as much as 6 inches in diameter, consist largely of argillaceous material, jaspery concentric bands, blades of barite, pyrite and some disseminated manganiferous and ferruginous carbonate minerals which are surrounded by dark areas. These latter are probably manganese oxide zones due to the alteration of a manganiferous carbonate.

Under magnification these nodular portions are roughly concentric and laminated in structure, with laminations red and green in color, and of fine and coarse grain. An oölitic structure, but with the spherules poorly formed, is found in combination with the banded structure. Calcite occurs as somewhat elongated crystals and is the dominant mineral. Wherever the calcite presents the peculiar elliptical and circular shapes mentioned on page —, an organic origin is immediately suggested (Fig. 5 and 6, Slide 254).

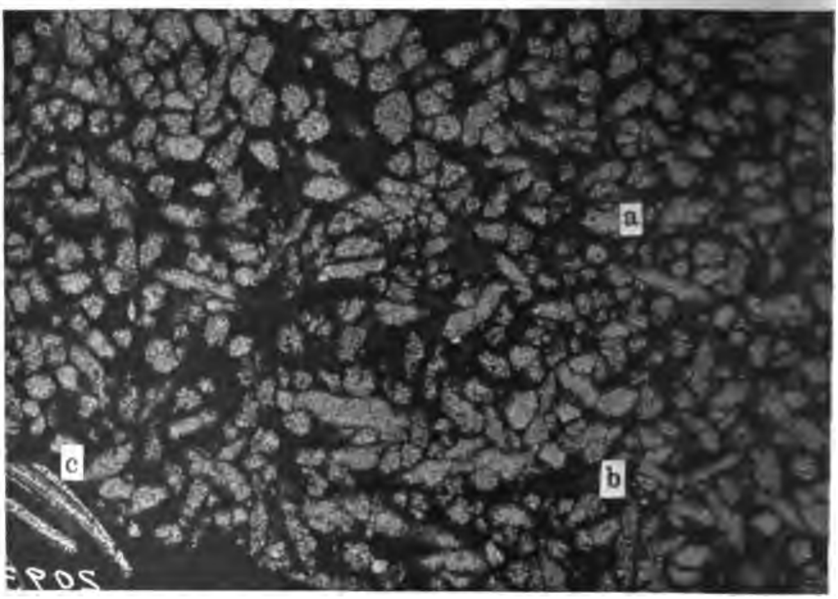


FIG. 5. Microphotograph of limestone, 219 B5d; slide 254; enlarged 22 diam. **a**, elliptical calcite aggregations; **b**, chlorite; **c**, hyolithes.

Quartz is found as irregular grains and aggregations. Barite occurs only sparingly. Among the opaque minerals, pyrite sometimes alter-

ing to limonite, is most conspicuous and occurs as large irregular grains and areas surrounding fossil fragments and associated with

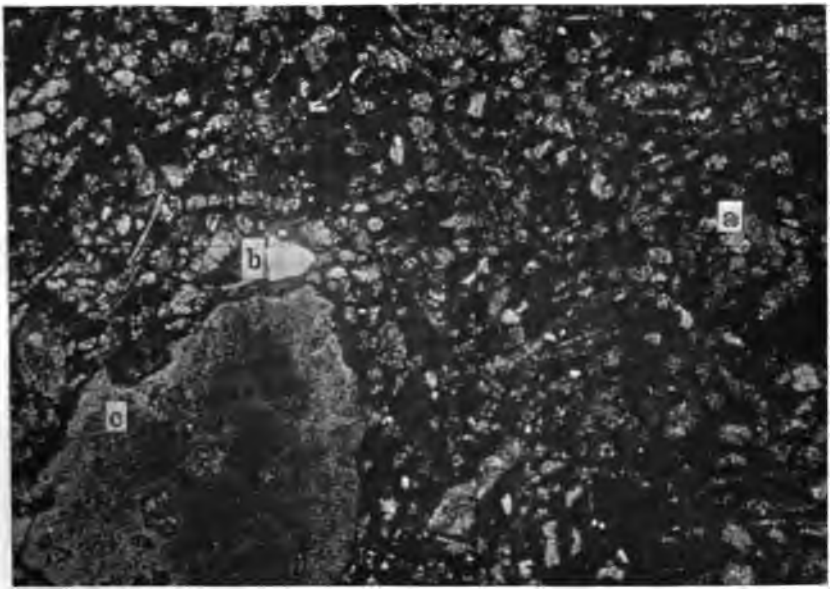


FIG. 6. Microphotograph of limestone, 219 B5d; slide 254; enlarged 22 diam. a, calcite; b, quartz; c, phosphatic? nodules.

more calcareous portions. Hematite (Fig. 7, Slide 257) is found in the more jaspery or laminated areas as irregular grains, aggregations, and spherules associated particularly with the green area which for the most part is of an indeterminable character. Veins of calcite are found cutting the nodule. As in the layer above, there are found in this one (Fig. 8, Slide 253), certain semi-isotropic nodular areas or pebbles which are partially chloritized. It is very possible that these nodular or pebbly areas are similar to the phosphatic nodules of 219 A 13 to be described later. These alter to carbonate locally. Among the organic remains are fragments of shells, hyolithes, trilobites, and sponge spicules, which in part show carbonate and chloritic replacement (Fig. 5, Slide 254, and Fig. 7, Slide 257).

219 A 1. Disconformably upon the above described nodular limestone there rests about 34 feet of a hard, fissile, green shale. About

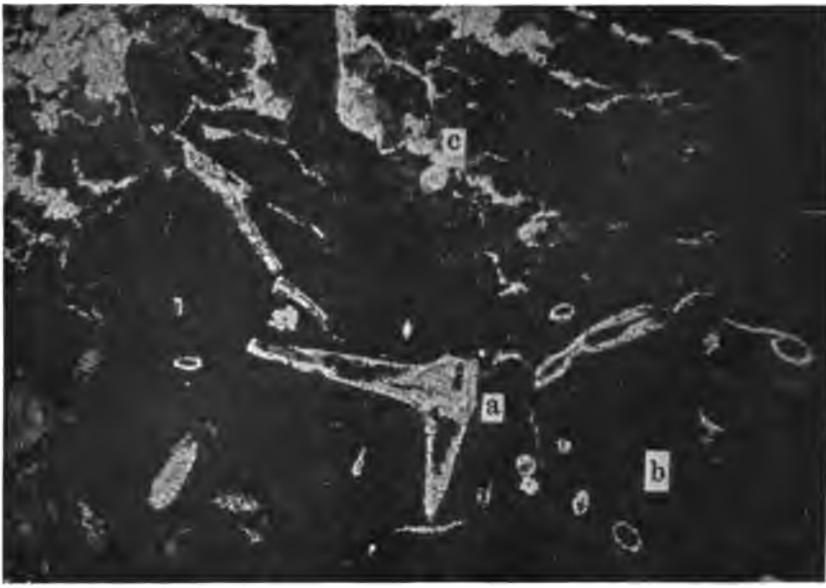


FIG. 7. Microphotograph of sponge spicules, 219 B6a; slide 257; enlarged 22 diam. a, sponge spicule; b, hematite; c, calcite.

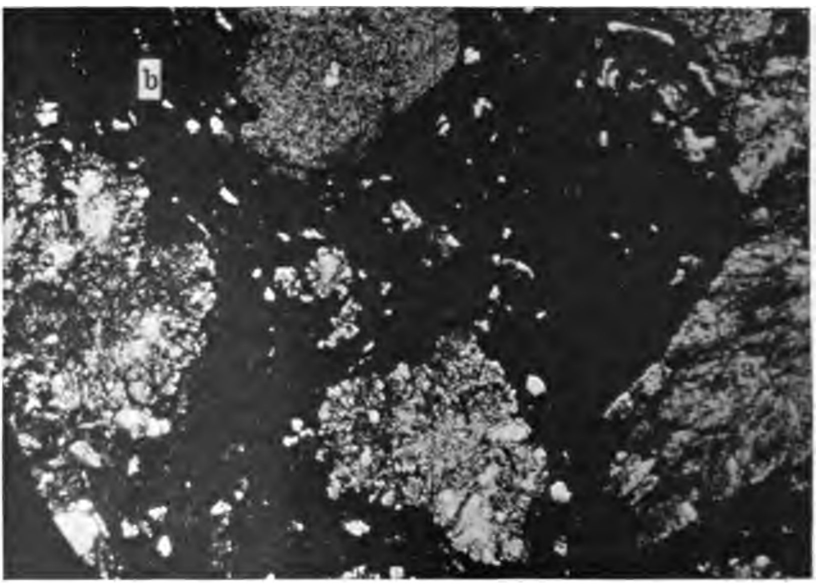


FIG. 8. Microphotograph of limestone, 219 B5d; slide 253; enlarged 22 diam. a, calcite; b, phosphatic? material.

5 feet above the limestone there are thin seams full of comminuted fragments of small *Lingulella,* and *Hyolithes* shells. The upper part of this shale is conspicuous because of the conchoidal fracture with which it breaks and the presence of local aggregations of small subspherical black nodules some of which show pinkish centers of some fine-grained minerals such as rhodochrosite or manganiferous calcite. MnO_2 occurs as small dots or as dendritic areas on the fracture planes. Microscopically, this is a chloritic micaceous shale containing sparingly, among the visible minerals, irregular grains of plagioclase, quartz, pyrite, magnetite and limonite in descending order of abundance.

219 A 2 is a nodular shale bed of .5 of a foot in thickness and forming the sloping surface over which the stream runs. This bed is noteworthy because of the Cryptozoon colonies showing on the surface (see Fig. 10).

219 A 2a, the lower portion of this bed, is a green shale containing frequent small subspherical nodules and disseminations of a pink carbonate which effervesces freely and is in all probability a manganiferous calcite similar to the pink nodules analyzed (see page 395).

219 A 2b is the Cryptozoon shale bed and contains roughly concentric or zonal structures measuring 1½ inches in diameter, irregular and sub-spherical nodules measuring 1 inch in diameter, and intercalated lenses of manganiferous calcite. These nodular and Cryptozoon structures weather brown. Scattered through the bed, particularly the shaly portions, are blades of barite.

Microscopic examination of this Cryptozoon bed brings out nothing which can be said to be of an organic structure. What structure there is may be characterized as broken veinous, concentric and laminated. The texture in great part is crystalline. The greater portion of one of the nodules consists of calcite and carbonate. Barite occurring as long blades is determined principally by the two cleavages, c and m, its birefringence greater than quartz and its biaxial + character. Chlorite either alone or in combination with carbonate is found replacing barite. Calcite or carbonate occur as irregular masses or as rudely formed or incipient spherules. Hematite occurs in the banded portions as more or less massive bands interlaminated with chlorite or as rudely formed spherules in the

FIG. 9. Details of lower portion of manganese zone in Manuels brook.

FIG. 10. Photograph of upper surface of Cryptozoon bed, 219 A2, in left bank Manuels brook.

FIG. 11. Microphotograph of section of Cryptozoon nodule, 219 A2; slide 292; enlarged 22 diam. a, ferruginous band; b, calcite.

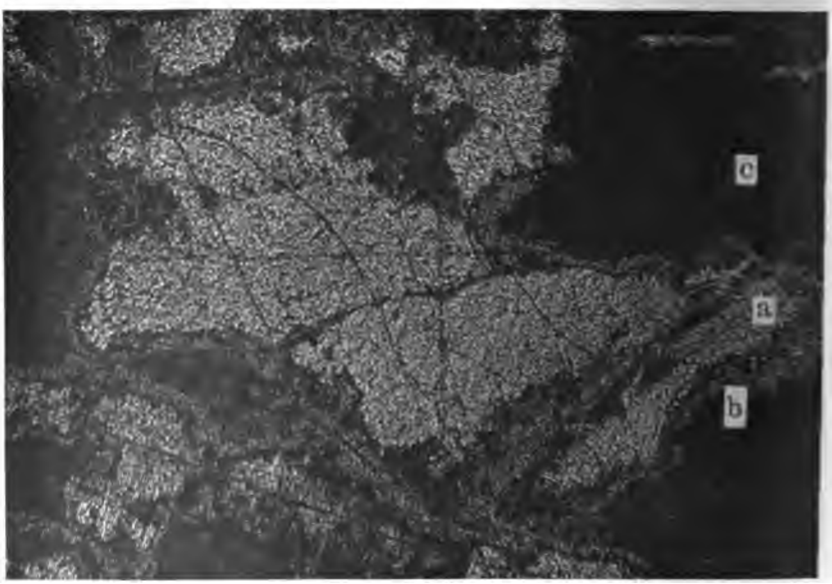


FIG. 12. Microphotograph of Cryptozoon nodules from 219 A2, showing barite being replaced by chlorite; slide 292; enlarged 22 diam. a, barite; b, chlorite; c, ferruginous and calcareous shale.

ground mass. These spherules measure as small as 9 microns in diameter but have an average diameter of between 30 and 40 microns (Fig. 11 and 12, Slide 292).

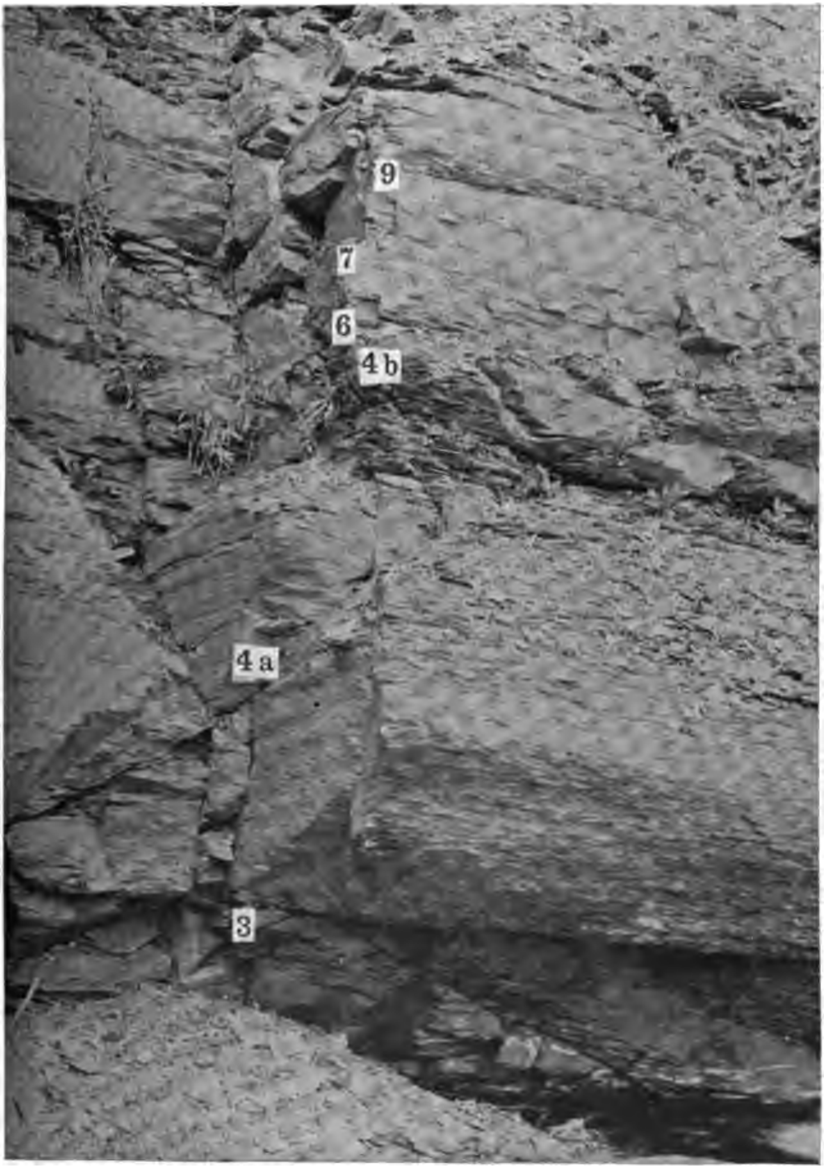


FIG. 13. Middle portion of manganese zone in Manuels brook. The numbers are those of the section.

The paragenesis of minerals within the nodules is as follows: Calcareous or carbonate material with probably synchronously formed hematite, barite veining, chloritization replacement, and finally calcite as vein or replacement material.

219 A 3 is a green shale bed, 3 feet in thickness, lying conformably above the Cryptozoon nodular bed. For the most part this bed consists of a hard fissile green shale which breaks with a conspicuous

FIG. 14. Photograph of manganese carbonate nodules extracted from shale 219 A4, natural size. Top, side and sectional views.

conchoidal fracture. 3 inches above the Cryptozoon bed is a layer containing fragments of trilobites which according to Prof. G. van Ingen are probably to be identified as *Protolenus harveyi*. Barren

green shale overlies this fossiliferous layer and this in turn is followed by nodular green shale containing manganiferous calcite nodules, a description of which is given in connection with the following bed.

FIG. 15. Photograph, natural size of ground and polished horizontal section of shale containing manganese carbonate nodules from 219 A4.

219 A 4 is a conspicuous rhodochrosite and manganiferous calcite nodular bed and may be considered the base of the manganese zone at Manuels (Fig. 13). Structurally this is a nodular and oölitic bed, the former structure conspicuously observable macroscopically, and the latter, though not so well defined a structure, observable microscopically. The entire bed measures 5.1 feet in thickness and is divisible into two members, **a** and **b**. The lower member, **219 A 4a** is a predominantly nodular reddish green shale while the upper division or **b** member is not so nodular.

The nodules of **219 A 4a** are discoidal in shape and vary in diameter from 1/6 inch to 1 1/2 inch, with an average of about 1 inch and a thickness ranging from 1/8 inch to 1/2 inch. The longer diameters of the nodules lie in the plane of the bed. Where the nodules are very numerous or crowded they are found intergrown with or overlapping each other. Specimens ground and polished often show a lemniscate formed by two nodules (Figs. 14 and 15). In color they are for the most part green, but may have greenish, white, or pink central cores. Cross sections of the nodules reveal a distinct zonal arrangement with spherical central cores surrounded by concentric

FIG. 16. Microphotograph of manganese carbonate nodule from 219 A4a; slide 288; enlarged 6 diam. **a**, carbonate of manganese, lime and magnesia; **b**, barite; **c**, barite replaced by chlorite; **d**, shale.

shells conforming to the shape of the nodule. The grain of the nodules is usually exceedingly fine, impalpable or crystalline. The pinkish cores are usually crystalline and respond to the HCl test quite readily, indicating some carbonate mineral. By analysis the green nodules are found to consist essentially of rhodochrosite (see Anal. *B*, page 395), while the pinkish crystalline mineral occupying

the centers of the nodules or occurring as intercalated lenses or nodules in the nodular bed is found to be essentially a maganiferous calcite (see Analysis *C*, page 395).

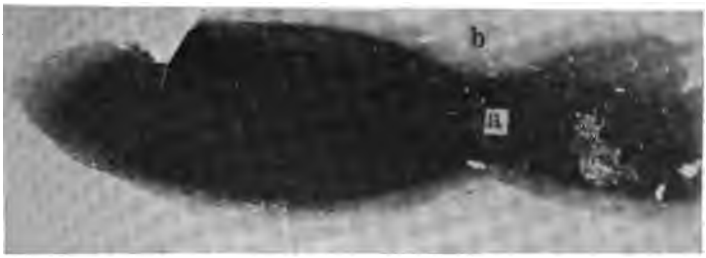


FIG. 17. Microphotograph of coalescing nodules from 219 A6c; slide 243; enlarged 4 diam. **a**, carbonate of manganese, etc.; **b**, oölitic shale.

Further macroscopical examination of the nodules shows the presence of barite blades within the central portions of the nodules or disseminated throughout the nodule or its shaly matrix. The characteristics which determined the barite are its c and m cleavage, its hardness of 2 and its diaphaneity. Its optical properties confirm it microscopically. Pyrite is found sometimes completely surrounding central cores as irregular and continuous grains. The surfaces of the nodules usually are covered with minute pink or reddish disseminated grains which upon microscopic examination are found to be hematitic spherules.

Thin sections of these nodules, on the whole, are not satisfactory for microscopical work because of the almost impalpable fineness of the grain. However some of the larger features may be of interest and importance. The structure is nodular and concentric and some of the concentric shells are oölitic. In all the thin sections of nodules the most conspicuous feature is the zonal arrangement of crystalline and indeterminable portions. The crystalline parts usually occupy the centers of the nodules while the impalpable or indeterminable areas are arranged around the centers (see Fig. 16, Slide 288). However some of the cores consist of indeterminable material. The zones are sometimes marked off from each other by more or less sharp contracts as brought out by a difference in shade of color or by an apparent difference in grain (Figs. 17, and 18).

The exterior zones merge imperceptibly into the shale, a fact which has some genetic significance.

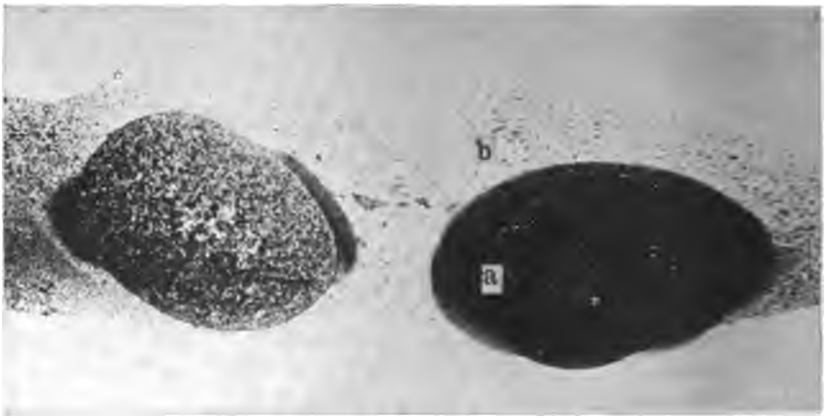


FIG. 18. Microphotograph of two manganese carbonate nodules from 219 A6c; slide 237; enlarged 8 diam.

An incipient oölitic structure with spherules of hematite is common to the outer zones of the nodule and shaly matrix. The spherules do not as a rule show any well-developed zonal structure nor are they of very regular form. They vary in diameter from 6 microns to 77 microns and have an average diameter of about 24 microns. Not infrequently the spherules consist of both carbonate and hematite, the former preserving a radiating structure and abounding in the more calcareous portions of the specimen, while the hematitic spherules are more common in the shaly parts.

Among the determinable minerals are calcite, which occurs as anhedral grains of variable dimensions in small crystalline areas, in veins, or as replacement material after organic remains such as sponge spicules, etc. Carbonate material for the most part specifically indeterminable makes up the greater part of the slide. Barite is found occupying the more central portions of the nodule in some sections. Quartz as irregular grains occurs only in sparing amounts. Pyrite is present as large and small irregular grains and masses.

The analyses of the green and pink nodules are as follows:

ANALYSIS *B.*

Green Nodules.

SiO_2	10.31
Fe_2O_3	7.35
Al_2O_3	3.68
MnO	31.76
CaO	10.46
MgO	1.80
$BaSO_4$	6.43
H_2O	2.85
CO_2	25.31
	99.96

ANALYSIS *B* 1.

Recalculation.

$MnCO_3$	39.56
MnO	7.30
$CaCO_3$	18.61
$MgCO_3$	3.79
SiO_2	5.94
$BaSO_4$	6.29
H_2O	1.51
Fe_2O_3	7.35
$2H_2O \cdot Al_2O_3 \cdot 2SiO_2$	9.17
	99.52

ANALYSIS *C.*

Pink Nodules.

SiO_2	5.14
Fe_2O_3	1.40
Al_2O_3	1.64
MnO	20.49
CaO	32.92
MgO	.01
H_2O	1.65
CO_2	36.77
	100.02

ANALYSIS *C* 1

Recalculation.

$CaCO_3$	58.05
$MnCO_3$	29.32
MnO	2.34
SiO_2	3.78
Fe_2O_3	1.40
H_2O	1.06
$2H_2O \cdot Al_2O_3 \cdot 2SiO_2$	4.07
	100.02

The pinkish crystalline mineral which exhibits a rhombohedral cleavage, has a hardness of about 3, effervesces freely with HCl acid, and, with the above composition, is essentially a manganiferous calcite. The excess MnO probably exists as a peroxide of manganese as indicated by the considerable amount of chlorine which was given off while the sample was being digested with HCl acid. As no thin sections were made of this specimen no petrographic confirmations can be made.

The upper subdivision of 219 A 4 (**219 A 4b**) is a greenish and reddish nodular shale bed measuring 2.9 feet in thickness and divisable into three roughly distinct portions. The lower part, **219 A 4b 1** is a greenish red shale overlaid by a reddish shale with occasional small nodules measuring about ¼ inch in diameter (Fig. 13, and 19).

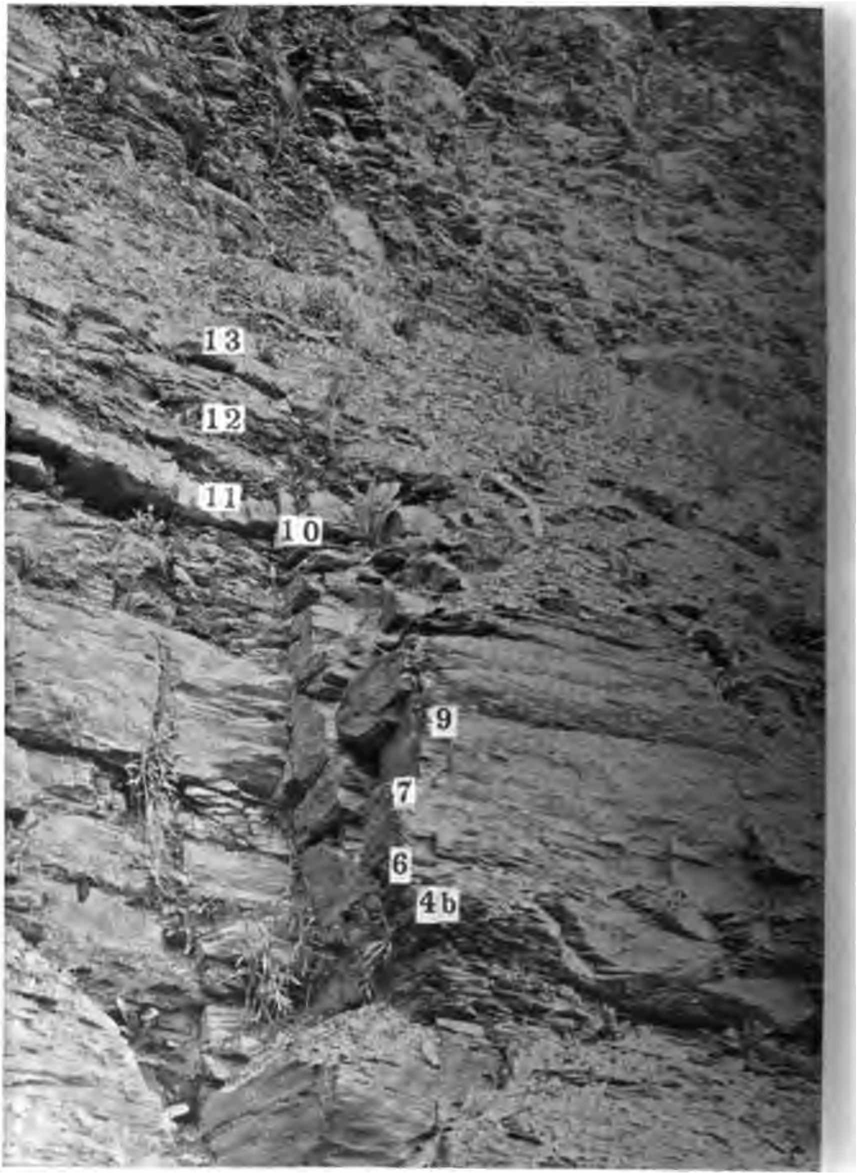


FIG. 19. Middle and upper portions of manganese zone in Manuels brook.

FIG. 20. Photograph of polished vertical section of banded manganese carbonate-oxide ore, 219 A7, slightly enlarged (1.8 diameters). **a**, green band; **b**, red band.

Under the microscope a thin section of this bed reveals hematite in the form of a pigment and as grains and ill-formed spherules, while local areas of carbonate are found. The upper member of this bed, **219 A 4b 3** is a red shale containing small subspherical and discoidal nodules quite similar to those described in detail above.

219 A 5 is a nodular ferruginous shale which is calcareous and manganiferous. The shaly structure and the manganese are brought out in a conspicuous way through weathering; the manganese by the black discoloration in evidence as one of the derived oxides. This bed has a thickness of .2 of a foot but thins and thickens, presenting a lenticular appearance. The nodules, or possibly pebbles, are subspherical in form, dark green in color, and of impalpable fineness of grain. They resemble those already described in connection with the

FIG. 21. Photograph of polished vertical section of banded manganese carbonate-oxide ore from 219 A7, natural size. a, green band; b, red band; c, barite.

219 B 5 limestone and those about to be described in beds 219 A 11 and 219 A 13, and are probably phosphatic pebbles in compostion. The minerals in evidence in this bed are hematite, calcite, and barite. This bed is undoubtedly a manganiferous bed as shown by the oxi-

Fig. 22. Photograph of vertical polished section of banded manganese carbonate-oxide ore from 219 A7, natural size. **a**, red band; **b**, brown band.

dized weathering products. The bed as a whole resembles 219 A 11 which to all appearances is suggestive of mineralized reworked material.

219 A 6 is somewhat fine-grained and gritty red shale, measuring 0.4 to 0.5 of a foot in thickness.

219 A 7 is the main manganese-bearing bed, measuring .7 of a foot in thickness. This bed is of more than usual interest in that the manganese occurs as primary carbonates and oxides in the form of thin jasper-like bands of green and light chocolate brown color, and as lenticles, and nodules. Interlaminated with the jaspery bands are reddish bands with manganese essentially in the form of an oxide and a carbonate (Fig. 20, 21, and 22). This bed has been divided into three layers, **a, b,** and **c.** The lowermost or **a** subdivision is the reddish band which is essentially a manganiferous shale. It is nodu-

lar with nodules, lenticles, and bands of the green jaspery carbonate and oxide of manganese. Wherever the jaspery minerals occur in the red band, whether as nodules, lenticles, continuous or non-continuous bands, they present or suggest concretionary characteristics. The red bands are locally pyritiferous and barytic. Red shale occupies the greater portion of the bed.

Microscopic examination of this red band brings out very little, other than that it is distinctly hematitic with the hematite occurring as a pigment or as irregular accumulations (Fig. 23, Slide 276).

FIG. 23. Microphotograph of banded manganese ore with barite, from 219 A7; slide 276; enlarged 9.5 diam. a, red band; b, green band; c, barite.

The chemical analysis of this band is as follows:

ANALYSIS *E*.

Red Bands.

SiO_2	27.61
Fe_2O_3	4.25
FeO	1.69
Al_2O_3	6.96
MnO	26.05
CaO	9.94
MgO	3.49
P_2O_5	4.71
H_2O	4.73
CO_2	10.57
	100.00

ANALYSIS *E* I.

Recalculation.

MnO	19.71
$MnCO_3$	10.23
$MgCO_3$	7.25
$CaCO_3$	7.50
$Ca_3(PO_4)_2$	10.31
SiO_2	19.44
H_2O	1.87
$2H_2O \cdot Al_2O_3 \cdot 2SiO_2 + FeO$	19.01
Fe_2O_3	4.25
	99.57

Of most interest in connection with this red shaly band are the jaspery bands of green and brown carbonate and oxide of manganese. Where in bands, they vary from $\frac{1}{8}$ inch to 1 inch + in thickness and may be continuous. The contact with the red band may be very even or very undulatory. This wavy character may be present whether the band thickens or thins or is of the same thickness throughout. The brown and green jaspery bands may contain thin laminæ or nodules of other colors.

The green material is characterized by its chalcedonic and somewhat waxy luster, its translucency on thin edges, its hardness of 5 to 6, its specific gravity of about 3.13 (that of the green nodule) and its slight response to HCl.

The chemical analysis of this material is as follows:

Analysis *A*. Green Band.		Analysis *A* I. Recalculation.	
SiO_2	7.24	$MnCO_3$	44.39
Fe_2O_3	3.36	MnO_2	8.08
FeO	3.21	$CaCO_3$	20.11
Al_2O_3	6.11	$MgCO_3$	4.21
MnO	35.53	FeO	3.36
CaO	11.30	H_2O	.86
MgO	2.30	$2H_2O \cdot Al_2O_3 \cdot 2SiO_2 + FeO$	18.24
H_2O	2.98		99.25
CO_2	28.06		
	100.09		

The green band so very similar chemically to the green nodule already described in connection with the nodular bed lower down in the series, is in great part a rhodochrosite in composition but has in combination, in descending order of abundance, considerable amounts of calcareous, argillaceous and ferruginous material. Manganese not combined with CO_2 probably exists as some oxide, probably a peroxide, as considerable chlorine was given off by the sample when first treated with concentrated HCl. Other features hardly need any explanation.

Thin sections of this band are very unsatisfactory in that, because of the impalpable fineness of the grain, little can be seen outside of structural features and certain opaque minerals, chiefly hematite.

The brown band differs in chemical composition, in color, and in specific gravity. The color is a light or dark chocolate brown. The specific gravity is 3.32. The chemical composition differs mainly in the higher percentage of manganese, as shown in the following analysis:

ANALYSIS *D.*

Brown Band.

SiO_2	10.23
Fe_2O_3	1.32
FeO	.89
Al_2O_3	4.84
MnO	49.25
CaO	8.11
MgO	3.02
H_2O	1.31
CO_2	21.83
	100.80

ANALYSIS *D* 1.

Recalculation.

MnO	28.93
$MnCO_3$	32.89
$CaCO_3$	14.01
$MgCo_3$	5.90
$2H_2O \cdot Al_2O_3 \cdot 2SiO_2$	11.08
SiO_2	5.40
Fe_2O_3	1.27
	99.48

Members **b** and **c** of bed **219 A 7** differ from the subdivision just described in the greater abundance of jaspery bands in comparison with the red shaly band and they show greater continuity on the whole.

Member **d** consists of green and brown jaspery bands all more or less nodular and interlaminated with the red manganiferous shale. Barite as segregations, disseminated blades, and veins occur infrequently. In the weathered portions of the section this bed is found altering on its more exposed structural planes to the secondary oxides of manganese such as psilomelane, etc.

219 A 8 is a purplish manganiferous nodular shale measuring 0.3 of a foot in thickness. It contains lenticles and discoidal nodules of the green jaspery manganese carbonate (Fig. 24, Slide 284). The noticeable microscopic features of a thin section of this rock are its nodular, oölitic and shaly structures. The spherules, though rudely formed, are of either hematite or a carbonate, the former more closely associated with the green jaspery structures, and the latter with the red shale.

219 A 9 is a manganiferous bed structurally, mineralogically, and, presumably, chemically, analogous to 219 A 7, and measuring .5 of a

foot in thickness. Green discoidal nodules of manganese carbonate and the green, brown and red manganiferous bands similar to those of 219 A 4 are a conspicuous feature of the bed. A thin section from one of the nodules of this bed collected during the summer of 1912 shows, aside from the nodular form, conspicuous zonal and

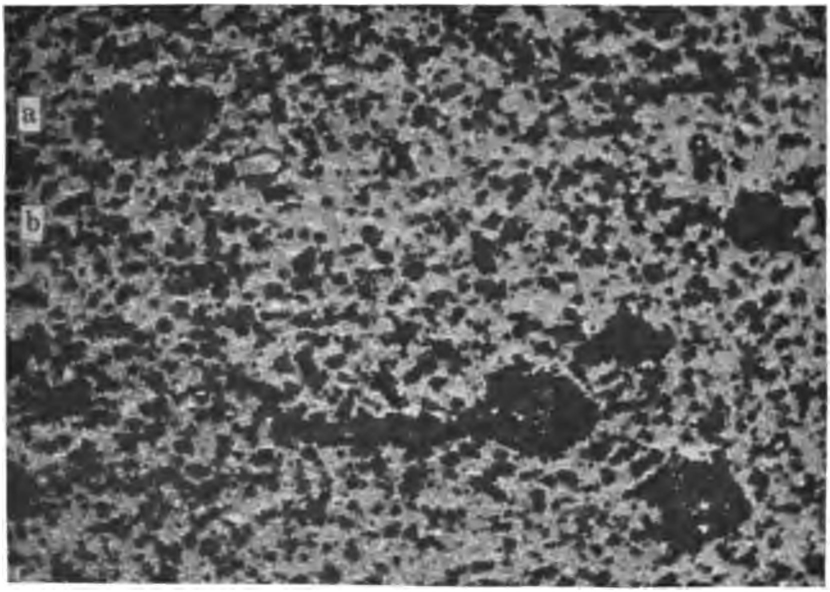


FIG. 24. Microphotograph of red shale from 219 A8; slide 284; enlarged 38 diam. **a**, hematite aggregation; **b**, spherules of hematite.

oölitic structures. For the most part the grain is impalpable, but that of the core is more or less crystalline. There are five pronounced parts consisting of a crystalline innermost core, No. 1, which in a great part is composed of carbonate, presumably that of calcium and manganese though nothing of a definite confirmatory nature could be observed, and 4 successive enveloping shells differentiated from each other by either the presence or absence of hematite, the shade or intensity of color or by fineness of grain. The oölitic character of zones 3 and 5 with spherules consisting in great part of hematite and measuring as small as 12 microns and as large as 90 is very noticeable. Layers 2 and 4 in a great degree consist of

indeterminate material (Fig. 25, Slide 244). Anisotropic minerals in this section are not common but those most noticeable are calcite.

FIG. 25. Microphotograph of nodule, from 210 A7; slide 244; enlarged 4.0 diam. **a**, outer zone of manganese carbonate; **b**, core of crystalline manganese carbonate.

barite, and chlorite, the latter being usually associated with the barite.

219 A 10 consists of 3.5 feet of alternate layers of purple and green shale which contain thin nodules and lenticles of jaspery manganese carbonate, some of which measure 1.3 feet in length and 0.1 feet in thickness. The lowermost subdivision of this bed, **210 A 10a**, is a dark reddish-green heavy nodular and oölitic shale with nodules very similar to those described above. Disseminated minute reddish mineral particles suggesting hematite spherules are found rimming the nodules in some cases. Barite occurs occasionally. Subdivision **b** of this bed is composed of 0.2 of a foot of green and red lenticular manganiferous seams with green jaspery nodules, similar to those in the lower beds, interlaminated with a hematic oölitic shale. Subdivision **c,** measuring 0.5 of a foot in thickness, is a dark gray oölitic and slightly nodular shale with green jaspery seams. Barite blades occur with nodular accumulations of manganiferous calcite. Microscopically this layer is essentially a hematitic oölitic shale with the individual spherules measuring from 15 to 23 microns in diameter while larger aggregations of spherules measure from 0.253 mm. to 0.387 mm. in diameter. The spherules consisting of hematite and car-

bonate are found in a groundmass the character and composition of which is for the most part indeterminable. Occasional pyrite grains are found (Fig. 26, Slide 280).

219 A 10d, the upper subdivision, consists of 0.3 of a foot of nodular and oölitic dark gray shale with thin jaspery manganese carbonate laminations.

Subdivision **e** is a dark green nodular and oölitic shale, 0.8 of a foot in thickness and not very different from the layer **d** just de-

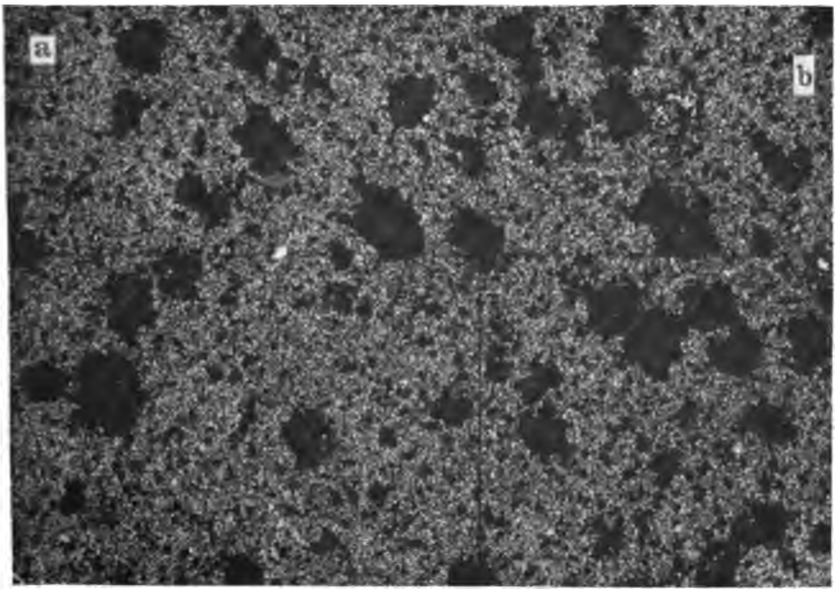


FIG. 26. Microphotograph of oölitic manganiferous shale from 219 A10c; slide 280; enlarged 22 diam. **a**, hematite spherules; **b**, shale with disseminated hematite.

scribed, and **f**—is a coarse nodular seam, 0.8 of a foot in thickness, in a dark green shale, comprising the uppermost portion of this bed.

219 A 11 is a heavy tough reddish band, 0.5 of a foot in thickness and lithologically very different from the immediately overlying and underlying beds. For the most part, the structure is both somewhat nodular and oölitic. The general fragmentary nature of the fossils and of certain nodular or pebbly forms leads one to think

that this layer consists in some degree of reworked material. The surface of this bed shows ripple marks. The predominant constituents which a macroscopic examination affords are calcite, barite, argillaceous material, limonite, manganese oxide, and pyrite. Outside of the nodular, oölitic, and fragmentary character of the layer,

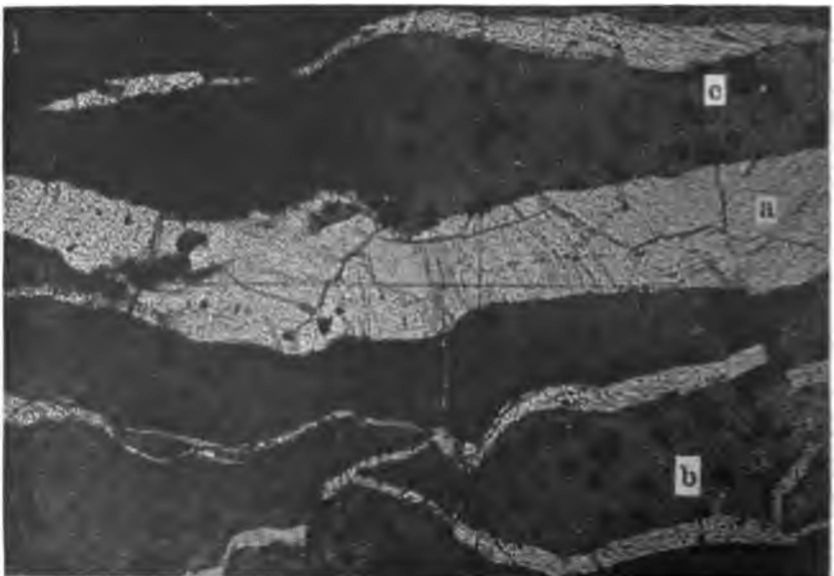


FIG. 27. Microphotograph of manganiferous red shale from 219 A11; slide 277; enlarged 22 diam. a, calcite vein; b, hematite spherule.

very little additional information concerning this peculiar rock could be gained microscopically (Fig. 27, Slide 277). The spherules are of two kinds, hematite and carbonate, and they average about 48 microns in diameter. Non-ferruginous portions of the slide show a groundmass of such fine-grained green material that very little could be made of it. *Hyolithes*, sponge spicules, and shell fragments partially or entirely replaced by calcite are a noticeable feature. Barite as scattered blades partially replaced by chlorite and pyrite, hematite as the chief constituent of the spherules, and carbonate are the most abundant of the determinable constituents of the slide (Fig. 28, Slide 278).

The chemical analysis of this rock is as follows:

Analysis *F*. 219 *A 11*.	
SiO_2	18.42
Fe_2O_3	6.33
Al_2O_3	7.95
MnO	21.44
CaO	14.46
MgO	5.01
P_2O_5	3.46
H_2O	2.58
CO_2	21.20
	100.85

Analysis *F* 1. *Recalculation*.	
MnO	9.36
$MnCO_3$	19.22
$CaCO_3$	18.61
$MgCO_3$	10.54
$Ca_3(PO_4)_2$	7.50
Fe_2O_3	6.22
SiO_2	9.18
$2H_2O \cdot Al_2O_3 \cdot 2SiO_2$	19.61
	100.24

This bed is essentially a manganiferous argillaceous dolomite with considerable percentages of barite, hematite, and phosphate. It would seem quite reasonable to suppose that the phosphate $Ca_3(PO_4)_2$ exists in the nodular portion as we have found to be the case in the nodules of 219 A 13 to be described later.

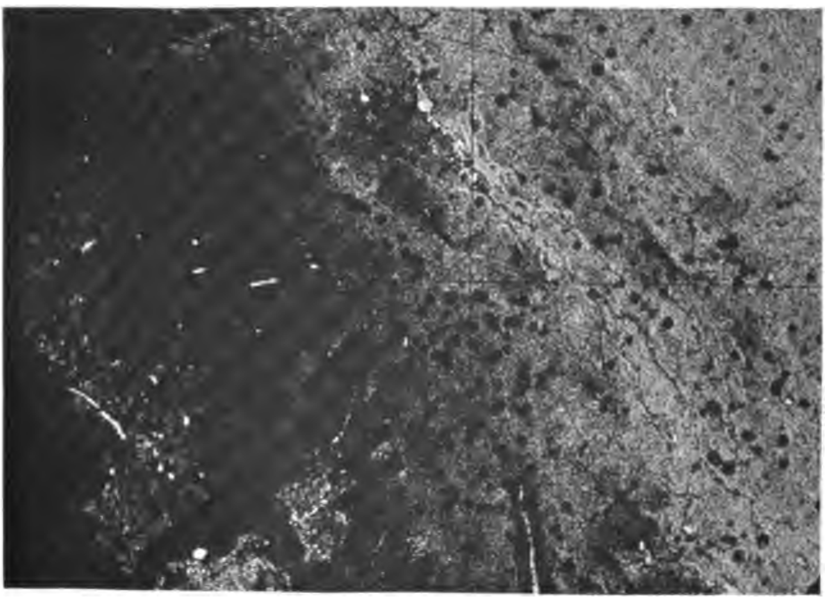


Fig. 28. Microphotograph of red manganiferous shale from 219 A11; slide 278; enlarged 22 diam.; showing hematite spherules in a groundmass of manganese carbonate.

219 A 12 is a fissile green shale measuring 1.4 feet in thickness with conspicuous black nodules which on weathering become white. Because of the similarity in form and color with those of 219 B 5, A 5, A 11, and those to be described from the bed immediately above this one, the suggestion is made here that these nodules also may be phosphatic.

219 A 13 is a phosphatic nodular manganiferous calcareous shale bed, 1 foot thick, with the nodules common in both bottom and top portions of the bed (Fig. 29). The nodules because of their white

FIG. 29. Photograph of a polished vertical section of a phosphatic nodular shale seam, 219 A13; natural size. **a**, phosphatic nodule; **b**, shale with trilobite fragments.

weathering and subspherical to elongated form resemble those of 219 A 12, A 11, A 5, and B 5. In chemical composition the nodules of this bed resemble those from the Cambrian of southern New Bruns-

wick described by W. D. Matthew (15). The chemical analyses of the Manuels brook and the New Brunswick phosphate nodules are as follows:

Analysis *H.*

Manuels Brook, N. F.		*Hanford Brook, N. B.*	
SiO_2	25.20	SiO_2	24.74
Al_2O_3	7.67	Al_2O_3	11.85
Fe_2O_3	10.13	Fe_2O_3	11.44
CaO	23.50	CaO	22.35
MgO	4.78	KO	0.59
P_2O_5	17.68	MgO	2.29
H_2O	2.71	NaO	1.41
CO_2	2.23	P_2O_5	14.99
	93.90	H_2O	3.43
			3.44
		CO_2	3.53
			100.06

The similarity between the percentages of SiO_2, Fe_2O_3, CaO and P_2O_5 of the two analyses is at once very noticeable and at the same time very suggestive. It is hoped that at some future time, work of a correlative nature may be taken up in connection with these interesting and genetically problematical nodules. Among the macroscopically observable minerals in the fresh and altered rock are pyrite, hematite, limonite, wad or psilomelane, and vivianite in an argillaceous dolomitic groundmass. *Hyolithes* fragments are in abundance.

As no apparent manganese was observable in the considerable thickness of overlying green shales, 219 A 13 was considered to be the top of the manganese zone at Manuels Brook. According to Prof. van Ingen the *Paradoxides* fauna begins in these shales which immediately overlie the manganese zone.

Topsail.—The manganese at Topsail some 4 miles east of Manuels (see Figs. 1 and 30) occurs interbedded in steep northerly dipping (50° to 78°) lower Cambrian strata consisting of shales, limestones and sandstones. The manganese is found in several beds of which only one measuring 1.4 feet in thickness seems to be of sufficient importance to have warranted prospecting, as shown by

some open cutting. This is a carbonate-oxide ore of manganese of brown color and vitreous luster.

Not only does the character and structure of the manganese at Topsail differ from that of Manuels but the section shows some

FIG. 30. Photograph of the open cut with the manganese prospect tunnel at Topsail; Loc. 219 E.

lithological variations. Moreover the rocks of the section are very much disturbed with the rapid changes in the dip of the beds. The structural changes in these beds are no doubt due to the great fault, the plane of which passes about 300 feet east from the manganese zone with a strike of N. 13 E. and a vertical dip. The fault plane lies between the Huronian and the lower Cambrian, and the beds immediately adjacent are considerably disturbed and so to a lesser extent are those farther away.

That a better idea may be obtained as to the relationship of the manganese, the following general and local stratigraphic sections with descriptions are given. The generalized section as worked out by Prof. van Ingen and Mr. A. O. Hayes during the summer of 1912 is as follows:

Loc. Number.		Ft.
210 E 10	Brown shales with manganese at base	Open cut
9	Brown shales with limestone at base.	
8	Heavy limestone.	
7	Shaly limestone	6.0
6	Brown sandstone with limestone nodules	3.0
5	Fine and coarse sandstone with small limestone nodules	6.0
4	Much sheared brown shale with limestone nodules and manganese at base	4.0
3	Mouth of tunnel and rotten zone	15.0
2	Coarse sandstone	6.0
1	Shear zone	0.3 to 0.5
0	Pre-Cambrian	25.0

It is quite apparent from a study of the above section that the lower Cambrian at Topsail is in many respects similar to that of Manuels. The absence of a basal conglomerate and the presence of sandstone are the most striking features of the associated beds. During the summer of 1913 a more detailed study of the manganese zone of 210 E 10 of the generalized section was made and the following subdivisions were made:

Loc. Number.		Ft.
219 E 7	Green shale, badly broken.	
6	Banded, concentric and nodular shale	1.0
5	Green shale, badly sheared	0.5
4	Manganese oxide-carbonate ore	1.4
3	Broken nodular green shale with manganese stain	0.7
2	Calcareous manganiferous shale	0.3
1	Hard nodular olive green shale, badly weathered and sheared, with manganese stain.	

Of this series two beds, 219 E 4 and 6, are worthy of more detailed description.

219 E 4 is an oxide-carbonate ore of manganese of 1.4 ft. in thickness. It is irregularly banded and nodular, of chocolate-brown color, somewhat vitreous in appearance and argillaceous, with a hardness of 5 to 6 and specific gravity of 3.26. Disseminated

through the ore are irregular small areas of a pink carbonate resembling rhodochrosite in physical characteristics, and barite. The ore is incrusted with psilomelane as an oxidation product. Microscopic examination brings out a coarsely banded and nodular structure with a groundmass of indeterminate material which is for the most part homogeneous to all appearances and of brown color.

FIG. 31. Microphotograph of barite sheaf in manganese oxide-carbonate ore from 219 E4; slide 269; enlarged 22 diam. **a**, barite sheaf; **b**, manganese oxide-carbonate ore.

The color of this ore is due to the brown and black oxides of manganese and iron. Conspicuous among the anisotropic minerals are barite which occurs as blades or bundles of blades generally replaced by chlorite, and calcite, all very much discolored by the manganiferous and ferruginous oxides. Minute veins of discolored calcite are present (see Fig. 31, Slide 269).

The chemical analysis of the ore and its recalculation are as follows:

ANALYSIS I.

219 E 4

SiO_2	18.04
Fe_2O_3	4.82
Al_2O_3	6.58
MnO	41.26
CaO	2.24
MgO	2.39
$BaSO_4$	5.40
CO_2	8.34
H_2O	7.98
	97.05

ANALYSIS I 1.

Recalculation.

MnO_2	34.25
$MnCO_3$	11.27
$CaCO_3$	4.00
$MgCO_3$	4.97
SiO_2	10.32
$BaSO_4$	5.40
Fe_2O_3	4.82
$2H_2O \cdot Al_2O_3 \cdot 2SiO_2$	16.30
H_2O	5.41
	96.74

This is essentially a hydrous oxide of manganese with considerable amounts of argillaceous material, rhodochrosite, silicious matter, dolomite, barite and hematite in descending order of abundance.

219 E 6, not a manganese ore bed, though manganiferous, is of interest mineralogically and petrographically. In structure it is concretionary and banded, nodular and microscopically oölitic. It is essentially a calcareous, ferruginous and manganiferous nodular and banded shale (see Fig. 32). Under the microscope the greater part of the groundmass, isotropic under crossed nicols, is of indeterminable composition simulating phosphatic material. Of the anisotropic minerals, calcite is most frequent and occurs with other carbonate material in bands which show an oölitic structure. The individual spherules, subspherical to elliptical in form show either concentric or radiated structure, the latter showing an interference cross with crossed nicols (Fig. 33, Slide 272). Calcite frequently has the curved twinning planes indicative of strain. Barite occurs in narrow veins or bands, as disseminated blades, or as sheath-like blades or aggregations, usually being replaced to a greater or less extent by chlorite and in a few instances by pyrite (Fig. 34, Slide 272). The spherules consist of hematitic pigment, carbonate and chlorite. Because of the frequent association of chlorite with barite one is led to suspect that possibly the chlorite spherules were originally of barite which has since been replaced by the chlorite. Other spherules made up in great part of hematite, sometimes show-

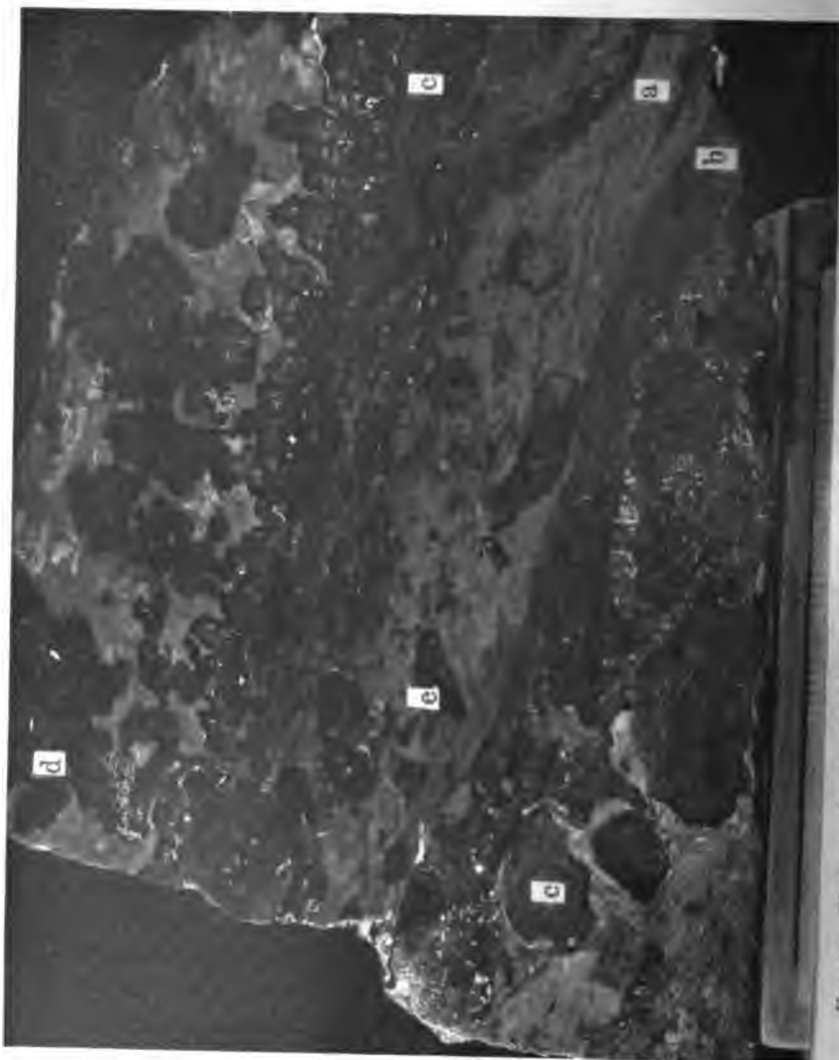


FIG. 32. Photograph of polished vertical section of banded and nodular shale from 210 Ro natural size. a, green shale; b, hematite nodule in hematite band; c, ferromanganese concretion; d, phosphatic nodule.

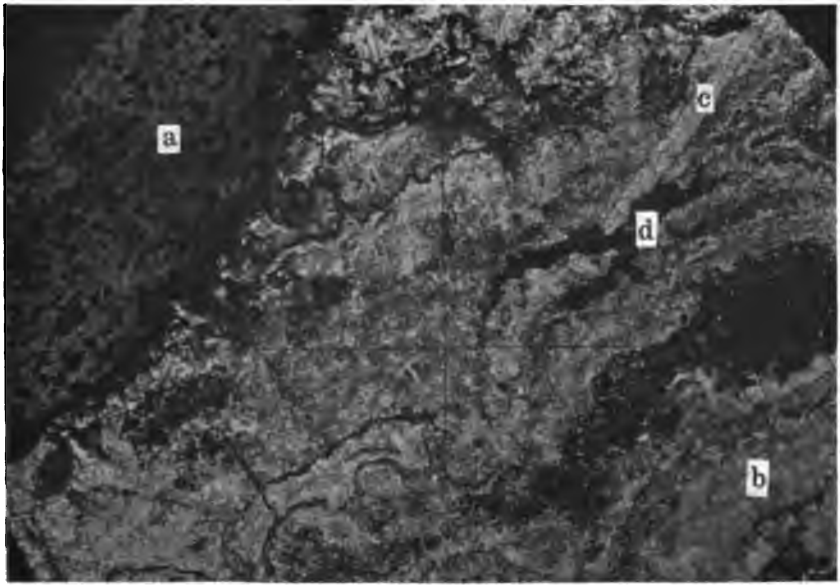


FIG. 33. Microphotograph of banded and nodular shale from 219 E6; slide 272; enlarged 38 diam. **a**, oölitic hematite shale; **b**, carbonate calcite band; **c**, barite; **d**, pyrite.

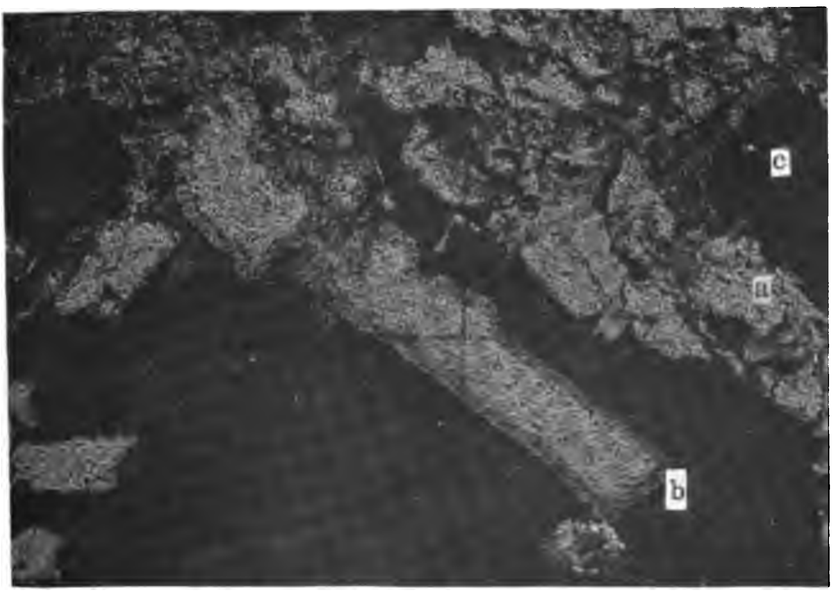


FIG. 34. Microphotograph of barite with chlorite replacement from 219 E6; slide 272; enlarged 22 diam. **a**, barite; **b**, chlorite replacing barite; **c**, phosphatic? material.

ing carbonate centers, are found most frequently in the jaspery bands. The spherules vary in size from 12 to 120 microns but have average diameter of about 94 microns.

The chemical analysis of this bed, with its recalculation, is as follows:

219 E 6.		*Recalculation.*	
SiO_2	18.24	$MnCO_3$	16.79
Fe_2O_3	10.01	$CaCO_3$	20.91
Al_2O_3	14.52	$MgCO_3$	10.57
MnO	10.42	$FeCO_3$	4.52
CaO	13.74	Fe_2O_3	5.49
MgO	4.94	$Ca_3(PO_4)_2$	3.75
P_2O_5	1.71	SiO_2	1.20
CO_2	24.01	$2H_2O \cdot Al_2O_3 \cdot 2SiO_2$	36.38
H_2O	2.07		99.61
	99.66		

From the above analysis, this rock is essentially a dolomitic manganiferous ferruginous shale with considerable amounts of $Ca_3(PO_4)_2$. Among the microscopically observable minerals in the above recalculation are calcite, hematite, quartz. The nodular portions, usually isotropic and of exceedingly fine grain, are probably

FIG. 35. Photograph of manganese prospect along the Kelligrews highway just south of Long Pond; Loc. 219 F.

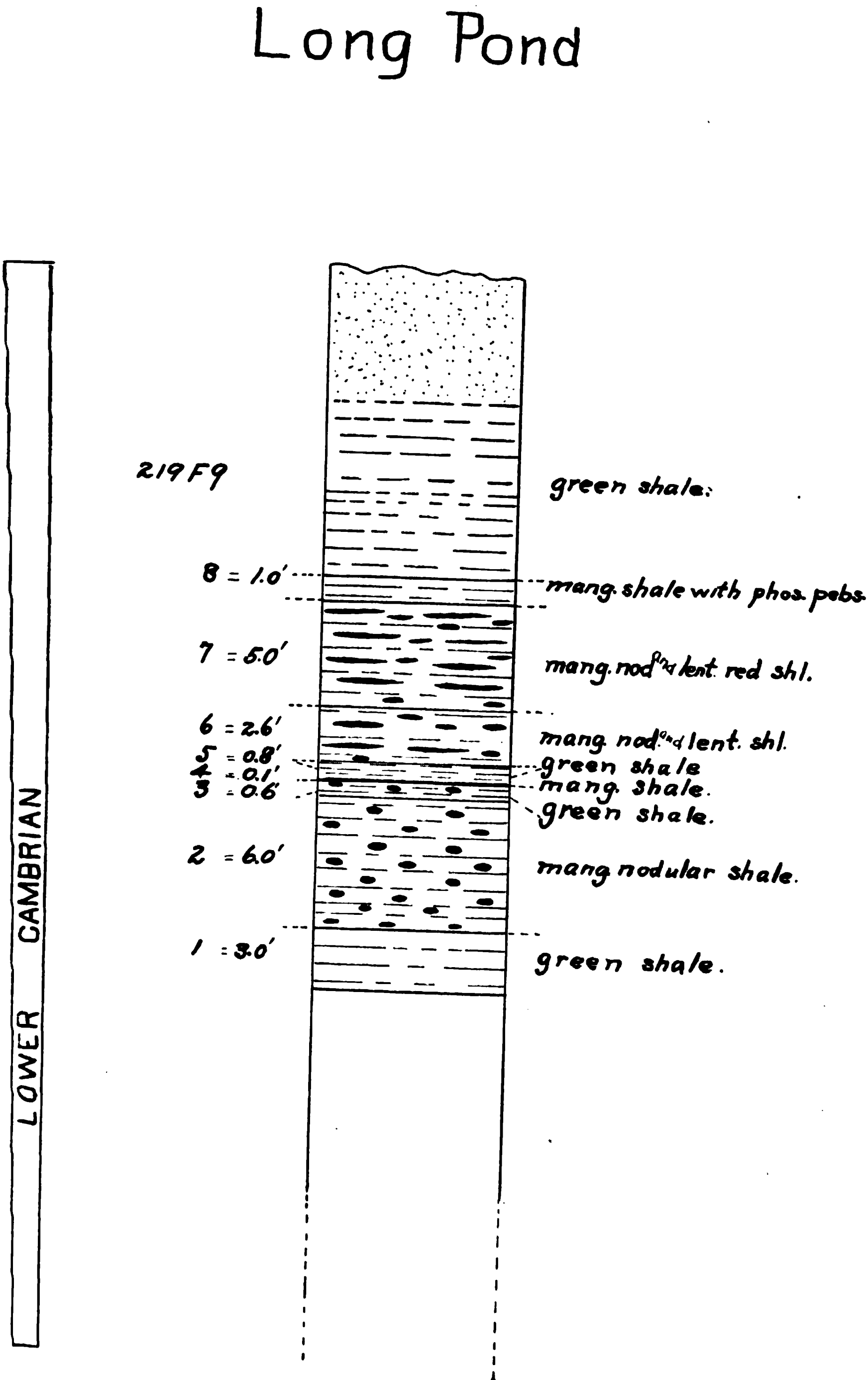


FIG. 36. Columnar section of the manganese zone at Long Pond; Loc. 219 F.

in great part shaly in composition. The nodules of this band suggest a very possible analogy to those of 219 A 5, 11, 12, 13, and B 5. In as much as this bed is somewhat phosphatic, the phosphate in all likelihood is associated with the nodules, as is the case with the nodules of 219 A 13 of Manuels. This bed is structurally and mineralogically quite similar to the phosphatic beds of Manuels.

Long Pond Section.—About 2½ miles southwest of Manuels and west of the railroad and wagon road (see Fig. 35), manganese occurs, in a low cliff, as nodular and banded layers interbedded with shales. Though the manganiferous beds at this locality are considerably more oxidized than those at Manuels, the occurrence as a whole is similar and it is necessary to present the section with only brief macroscopical descriptions of the important beds (Fig. 36).

Loc. Number.		Ft
219 F 10	Glacial mantle.	
9	Manganiferous green shale.	
8	Phosphatic nodular manganese shale	1.0
7	Manganiferous nodular and lenticular green shale	5.0
6	Banded nodular ore	2.6
5	Fissile green shale	0.8
4	Manganiferous banded ore	0.1
3	Massive nodular green shale	0.6
2	Nodular shale	6.0
1	Heavy green olive shale	3.0

219 F 2 of the above section corresponds quite closely to the lower nodular bed, 219 A 4a, of Manuels (see page 392), chiefly because of the presence of abundant discoidal-shaped nodules identical with those at Manuels. The nodules have altered for the most part to a wad and clay, some having secondary manganese or white clay centers and clay border zones and others with limonitic green clay centers with secondary manganiferous clay border zones. The weathered nodules are very abundant.

219 F 6 is a heavy manganiferous bed composed of several ½″ to 3″ red, brown and green manganiferous seams separated by thin nodular shale laminations that are now red. It is quite evident that this bed is a continuation of either 219 A 7 or 10 of Manuels. The interior of some of the weathered nodules is a red and green residual clay. The manganiferous seams weather reddish and greenish.

219 F 7 is probably a continuation of 219 A 8 and 9 of Manuels inasmuch as this bed is nodular and has many lenticular and continuous jaspery seams of 0.1 inch to 1 inch in thickness alternating with ½ inch to 1 inch seams of reddish manganiferous shale.

219 F 8 from its similarity to 219 A 13 of Manuels may be described as a phosphatic nodular manganiferous shale with the manganese in evidence as some hydrous oxide.

CHAPEL COVE SECTION.—The manganese at Chapel Cove, of inconsiderable amount, occurs in a very much faulted series of lower Cambrian limestones and shales as alteration products on many of the structural planes. If it were not for certain lithological analogies with the deposits just described it would hardly seem necessary to give any detailed description of this deposit because of the small quantity of manganese present (see Figs. 1 and 37).

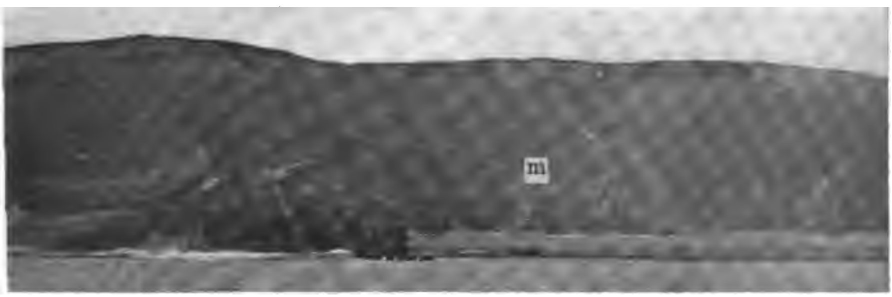


FIG. 37. Photograph of the section along the shore at Chapel Cove near Holyrood, Loc. 213 C; showing the managenese zone at (**m**).

The generalized section as worked out by Prof. G. van Ingen and Mr. A. O. Hayes during the summer of 1912 is as follows:

Loc. Number.		Ft.
213 C 4 c	Olive green shale.	
b	Alternate pink layers with small black pebbles, manganese layer	3.6
a	Olive green shale, sheared near fault.	
C 3	Nodular limestone and shale	24.0
7	Argillaceous red limestone and alternating shales	25.0
6	Red shaly limestone	18.0
5	Red shale	8.0
D 7	Heavy red limestone	10.0
6	Red shales with limestone.	
5	Red and green limestone.	
4	Green limestone.	

E 2 Gray limestone.
1 Conglomerate with pebbles of syenite, black chert and limestone.
0 Syenite.
C 2 Agglomerate.
1 Ribbon slates. Conception slates (Huronian).

The section represents the stratigraphic sequence and the locality numbers indicate the position of the layers. Quoting Prof. van Ingen in regard to this most interesting locality:

"It appears to me that we have here the remnant of a squeezed syncline, the northern margin of which has been shoved far northwardly onto the underlying agglomerate and ribbon slates."

213 C 4b was studied more in detail by the writer during the summer of 1913 in the hope that some more definite knowledge might be gained on the occurrence of the manganese at this point, but without very much satisfaction. The subdivided manganese bed is as follows:

Loc. Number.
213 L 4 c Finely banded nodular bed.
b Fractured and slickensided green shale.
213 L 4 a Black nodular calcareous green shale with manganese staining.
L 3 Nodular ferruginous calcareous green shale with manganese stains.
2 Fractured and fissile shale.
1 Manganiferous calcareous green shale with hematite and pyrite.

In as much as the manganese was not visible to any great extent in its primary form throughout this small series of 3 to 4 feet no analysis was thought necessary. Two of the above beds, 213 L 3 and 213 L 4 are worthy of macroscopical and microscopical descriptions because of marked lithological resemblance to certain of the rocks at Manuels.

213 L 3 is a nodular shale with conspicuous calcareous ferruginous and manganiferous aggregations and jet black pebbles or nodular forms. All structural and divisional planes of this bed are conspicuously stained with some secondary oxide of manganese, probably a hydrated oxide such as psilomelane. Microscopical examination of this shale brings out the fact that the structure is nodular and oölitic and that the rock is a ferruginous chloritic shale.

The groundmass consists of chlorite and, for the most part, of an indeterminable material. Calcite occurs as an alteration product or as a constituent of the hematitic spherules. Quartz is found composing infrequent aggregates and as vein-filling material. The opaque minerals other than hematite are manganese as psilomelane or some other secondary derivative, pyrite as disseminations, and

FIG. 38. Photograph of manganese prospect on Brigus South Head; Loc. 212 A12a. **a**, oxidized manganese beds; **b**, green shale.

limonite as a yellow staining. The spherules are for the most part hematite in composition but carbonate is a very common constituent. The diameters of the spherules range from 21 to 159 microns but average around 44 and 77 microns. The ferruginous centers of some of the spherules measure 0.8 of a micron.

Certain discoidal nodules in **213 L 4c** resemble those of 219 A 4 at Manuels though they are very much less abundant.

213 L 4a is nodular and the texture exceedingly fine-grained and locally crystalline. The greater portion of the thin section is probably composed of shale material and the remainder is taken up in great part by calcite and carbonate disseminations, as replacement material of hyolithes shells, or as mineral aggregates. Barite occurs

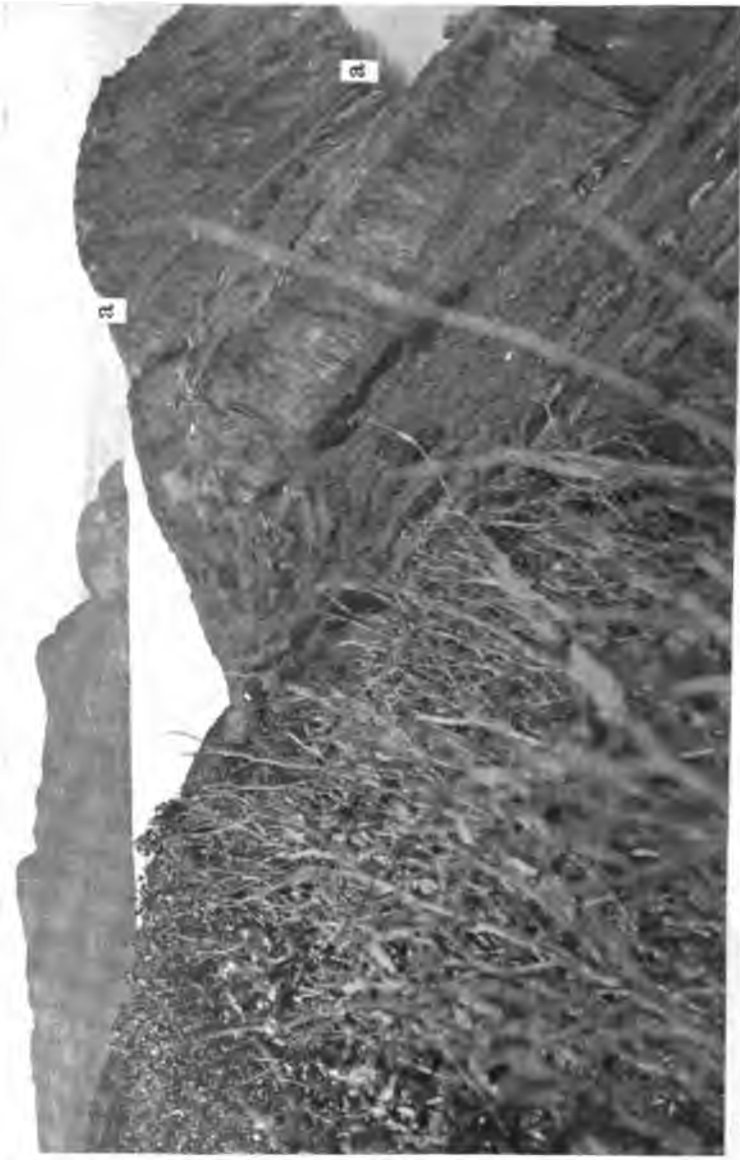


Fig. 39. View of Brigus South Head looking across the mouth of the harbor in a northerly direction and showing the position of the manganese zone (**a**–**a**).

as infrequent disseminations and individual platy crystals and probably once formed the *Hyolithes*-like rods now replaced by chlorite and a carbonate. Pyrite and hematite are found. The nodules of this bed, subspherical in shape, show under the microscope a compact

FIG. 40. View of the manganese beds (a) dipping into the sea on the east side of Brigus South Head.

structure and an almost impalpable fineness of grain. Under crossed nicols an occasional angular fragment of quartz is found but the groundmass as a whole appears to be isotropic. It is possible that these pebbles are analogous to the phosphate pebbles of Manuels, Topsail, and Long Pond.

Brigus Section.—At Brigus South Head on the west shore of Conception Bay (see Figs. 1, 38, 39, 40, and 41) manganese is found to a great extent in the oxidized state in several beds at the water's edge in the shales of lower Cambrian age which make up the sharp hog back ridge overlooking the "Needles." Because of the inaccessibility of that portion of the ridge where the manganese was best preserved, detailed measurement of the section was not possible. Prof. van Ingen and Mr. A. O. Hayes in 1912 found that the best manganese measured about 4.5 feet thick in a zone of 15 feet. Specimens collected from more accessible portions were all practically altered to psilomelane but there is one which shows the original jaspery carbonate quite similar to the types described in connection with the Manuels occurrence. Several old prospect pits on the more accessible parts of this ridge were examined by the writer, but the manganese was found to be in its secondary state and the interbedded shales in a very much disturbed condition. The strike of the strata of this locality is N. 10 E. and the dip 47 E.

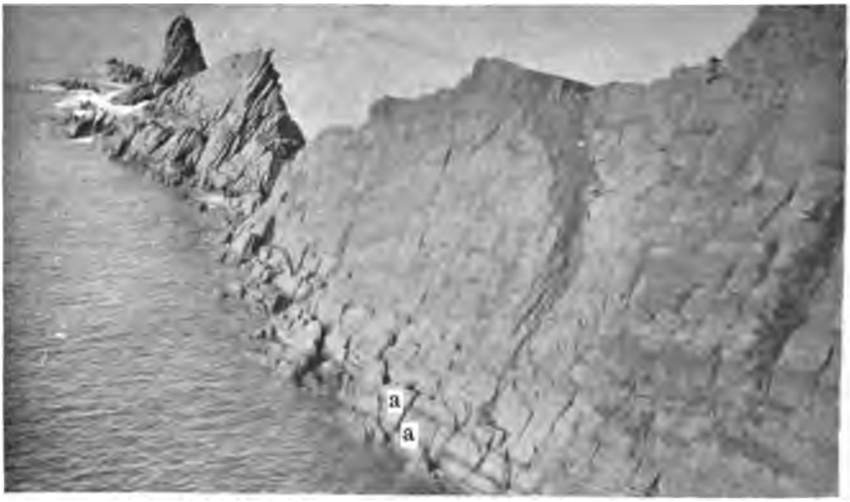


Fig. 41. View of the "Needles" at the extremity of Brigus South Head, showing the manganese zone (a–a).

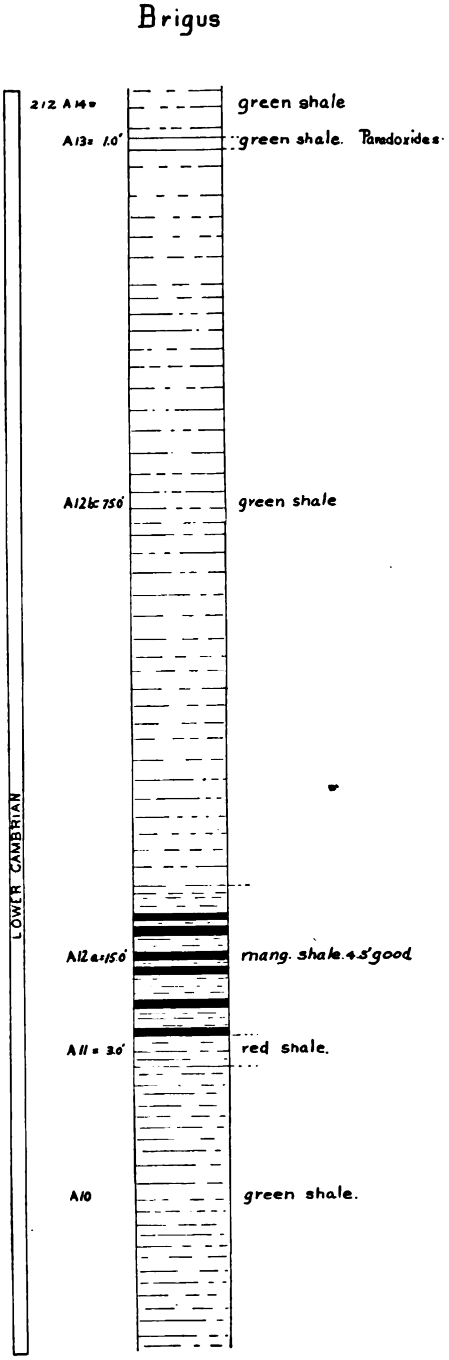


FIG. 42. Columnar section of a portion of the lower Cambrian at Brigus South Head, from measurements made by Gilbert van Ingen and A. O. Hayes, 1912.

The following section as prepared by Prof. van Ingen and Mr. A. O. Hayes from their study of the region in 1912 shows the stratigraphic relations of the manganese deposits at this point (Figs. 39 and 42).

Loc. Number.		Ft.
212 A 14	Green shales, end of needles	50.0
13	*Paradoxides* zone, green shales	1.0
212 A 12 b	Green shales	75.0
12 a	Manganese zone (4.5 ft. best)	15.0
11	Red shales, thin band	3.0
10	Green shale	60.0
9	Red shale	210.0
8	Red shaly limestone	11.6
7	Red shale	28.0
6	Limestone, heavy white at base, nodular and red above. *Holmia bröggeri* and other trilobites	30.0
5	Red shale	5.0
4	Limestone, very shaly	12.0
3	Red shale	32.0
2	Limestone with Cryptozoon	30.0
1	Red shale with local sandstone and conglomerate	50.0
	Unconformity.	
0	Pre-Cambrian shale and ash beds.	

The striking feature of this section is the position of the manganese zone in relation to the *Paradoxides* bed which is exactly the relation established at Manuels and undoubtedly at the other localities described.

SMITH SOUND SECTION, TRINITY BAY.—The manganese zone on Trinity bay occurs at Smith Point (Fig. 1) as two massive beds associated with red and green nodular shales and limestones of lower Cambrian age. The accompanying map (Fig. 43), prepared from a transit survey of the shore line by Prof. van. Ingen during the summer of 1913, shows the structural and stratigraphic relations of the two manganese beds, 230 D 20 and D 27. The general strike of these beds is north and the dip, 20 west.

230 D 27, the important manganese bed of this section (Figs. 44, 45), measures some 38 inches in thickness, and is faulted with a downthrow of 15 feet on the west side. It is the thicker of the two manganese beds, and has been found by analysis to be essentially a

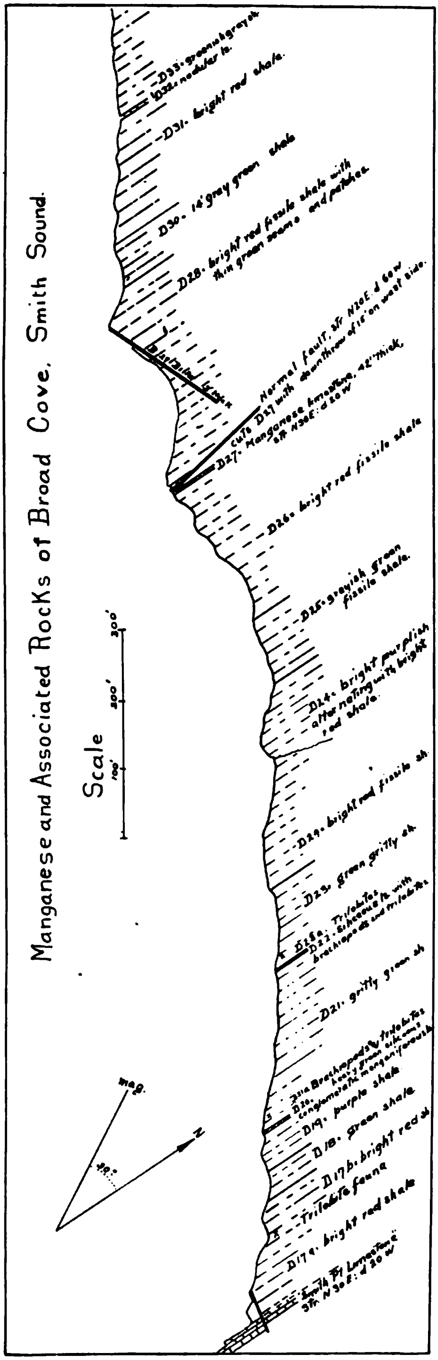


FIG. 43. Map of the outcrops of the protolenus and manganese zones exposed on the shore of Broad Cove, near Smith Point, Smith Sound, traced from field map based on stadia transit surveys by Gilbert van Ingen, 1913.

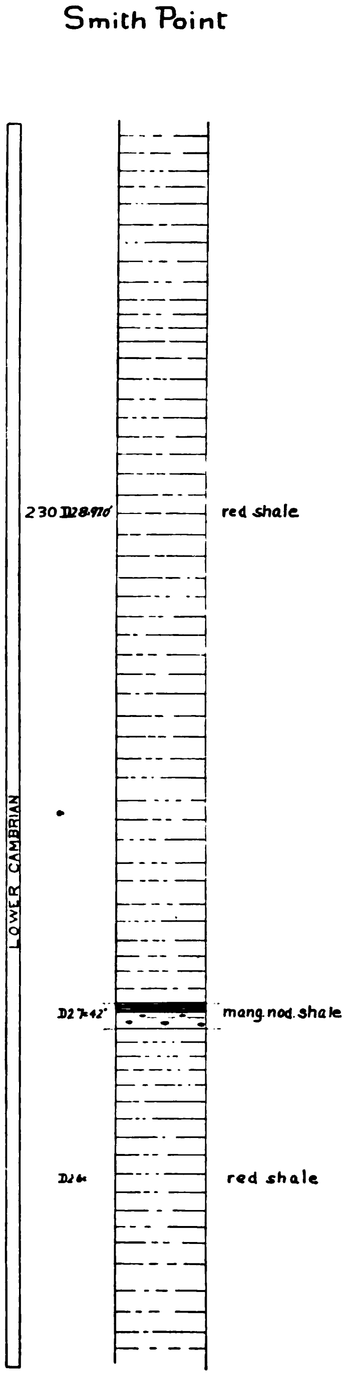


FIG. 44. Columnar section of a portion of the lower Cambrian at Broad Cove, near Smith Point, Smith Sound, Trinity Bay, Newfoundland, showing the manganese zone, 230 D26 to 28.

manganiferous dolomitic ferruginous shale. The bed is somewhat massive and nodular though the nodules are very irregular as compared with those at Manuels and other localities; irregular crystalline areas form the nodular portions while the matrix is made up of more argillaceous matter. Thin sections taken from the bottom and central portions of the bed were examined microscopically.

Fig. 45. Exposure of manganese ore, 230 D27, on the Broad Cove shore, near Smith Point, Smith Sound. This is a nodular ferro-manganese carbonate-oxide bed.

230 D 27aa is a reddish nodular and oölitic shale, with hematitic carbonate making up the greater portion of the determinable minerals; aggregations of a fine-grained dark material suggest phosphatic nodules so common in the Manuels occurrence. Irregular grains of quartz and aggregations of chlorite are found. Sections of trilobites and other organic forms containing carbonate material abound. Some hydrous manganic dioxide occurs (Fig. 47, Slide 299). Sections from the middle portions of the bed, **230 D 27e,** show a somewhat massive, nodular or oölitic reddish rock. Hematite is found as a pigment and to a lesser extent as lustrous opaque grains to which the color of the rock is due. A manganic oxide occurs as irregular and infrequent grains. Carbonate occurs as vein filling, as irregular areas, or as replacements of sponge spicules

Fig. 46. Nearer view of the ferro-manganese ore bed, 250 Day on the Broad Cove shore.

and other organic bodies. Barite is found infrequently, sometimes with chlorite fringing it. Chlorite may be found replacing trilobite fragments.

FIG. 47. Microphotograph of fossiliferous manganese ore, 230 D27; slide 299; from Broad Cove; enlarged 22 diam. a, manganese carbonate-oxide ore; b, fragment of trilobite test.

The following analysis and recalculation represent the chemical composition of an average sample of the bed and will corroborate some of the petrographic observations:

ANALYSIS *K*.

230 D 27.

SiO_2	15.14
Fe_2O_3	9.22
Al_2O_3	12.04
MnO	25.63
CaO	10.04
MgO	3.72
P_2O_5	1.26
H_2O	2.73
CO_2	21.05
	100.83

ANALYSIS *K* I.

Recalculation.

$MnCO_3$	26.91
MnO_2	9.00
$CaCO_3$	15.21
$MgCO_3$	7.75
Fe_2O_3	9.22
$Ca_3(PO_4)_2$	2.50
SiO_2	.84
$2H_2O \cdot Al_2O_3 \cdot 2SiO_2$	30.06
	101.49

The manganese according to the recalculation of the analysis is essentially in the form of a rhodochrosite and whatever manganese there is in excess probably exists as a peroxide. It is quite possible that $Ca_3(PO_4)_2$ exists in the irregular black fine-grained areas, though nothing definite can be said in confirmation of it at this time.

Section of the Lower Cambrian from the Base of the Lower Paradoxides Zone down to the Top of the Smith Point Limestone, Trinity Bay (Fig. 47).

Loc. Number.			Ft.
230 D 32	Thin seams of nodular limestone in red shale		4.0
31	Bright red shale		46.0
30	Gray green shale		14.0
29	Dike		3.0
28	Bright red fissile shale with thin green seams and patches		97.0
27	Manganese limestone (manganiferous dolomitic shale)		3.5
26	Bright red fissile shale		78.0
25	Grayish green fissile shale		28.0
24	Bright purplish shale alternating with bright red shale		97.0
23	Green gritty shale		33.0
22	Gray band of fine grain silicious limestone full of pyrites and same brachiopods and trilobites		0.5
21	Gritty green shale, brachiopods and trilobites		62.0
20	Heavy green silicious conglomeratic manganiferous limestone		2.5
19	Purple shale		10.0
18	Green shale		10.0
17	Red shale		47.0
	b—Contains trilobite fauna	13.0 ft.	
	a—Red shale	19.0	
	Interval covered	15.0	
16	Smith Point Limestone.		
	Total		535.5

V. OTHER MANGANESE DEPOSITS OF SOMEWHAT SIMILAR CHARACTER.

Sedimentary deposits of manganese are not of uncommon occurrence but it is rare that we find such deposits still in their unaltered condition as they were originally formed. There are however a few deposits elsewhere which in many respects resemble the Conception Bay and Smith Sound occurrences.

Newfoundland, Placentia Bay. In Placentia Bay, New-

foundland, manganese has been described by Murray and Howley as a massive carbonate bed interbedded with slates of "Silurian" age. Dr. T. Sterry Hunt (12: 204 and 205) described this mineral as

"compact and impalpable in texture, brittle, with a conchoidal fracture and a feeble waxy luster; slightly translucent on the thin edges; color fawn to pale chestnut-brown; streak white, hardness 4.0; density 3.25. The specimen shows faint lines which seem to be those of deposition and give to the mass the aspect of a sinter. It is encrusted and penetrated in parts with black crystalline oxide of manganese. The presence of oxide of manganese in this mineral is probably due to its partial decomposition." Analysis of this mineral by Dr. Hunt is as follows:

$MnCO_3$	84.6
SiO_2	14.40
Fe, CaO and MgO	traces

"This deposit is of interest on account of the existence of the metal in the form of a bedded carbonate. It probably represents the former condition of many of the oxide ores of manganese elsewhere in the stratified rocks, but they have since been converted to their more stable form."

It is quite evident from the above description of the Placentia Bay manganese that we have in all probability a deposit similar in mineralogic character and stratigraphic position to those in Conception Bay. No published stratigraphical or palæontological work has appeared on the Placentia Bay occurrences. In that portion of this paper relating to the stratigraphy of the manganese deposits it will be readily seen that the basin into which the manganiferous muds were deposited to form the present manganese beds of the lower Cambrian probably extended to or covered Placentia Bay or that portion of Placentia Bay where we now find Cambrian rocks. There is no doubt that the "Silurian rocks" referred to above by Howley and Murray are the lower Cambrian.

WALES.—Sedimentary manganese deposits have been described as occurring in the Cambrian rocks of Merionethshire, North Wales by Mr. Edward Halse (9: 156) in an article entitled, "The Occurrence of Manganese Ore in the Cambrian Rocks of Merionethshire." He says:

"in the Harlech mine, the bed of ore is a little over a foot thick, consisting of grit of medium grain, overlaid by a thin band of quartzite, probably meta-

morphosed grit. The roof proper consist of about 2 feet of very hard, schistose rocks, termed 'blue stone' by the miners. Specimens of ore taken from the mine are seen to be formed of uniform layers, having gray yellowish, white, greenish and chocolate-brown layers."

A reference by J. A. Phillips and Henry Louis (21 : 296) to the same occurrence is as follows:

"Beds of carbonate of manganese with some silicate, the outcrops of which have been to some extent changed into black oxide, occur intercalated between sandstones, grits and conglomerates of the Cambrian formation, and have been mined to some extent; the beds vary from one to two feet in thickness, and yield ore, averaging about twenty-seven per cent. of metal, which is used in spiegel making. These deposits are evidently symphytic and belong to group b of that class."

Phillips and Louis believe that these deposits were formed syngenetically but from precipitates in aqueous solutions. This deposit suggests very striking similarities to the Manuels occurrence not only mineralogically and genetically but also from the standpoint of stratigraphy.

ARKANSAS.—The Cason tract of the Batesville region, Arkansas, presents certain petrological analogies to the Newfoundland occurrences. Dr. Penrose (20: 219) describes the ore as occurring

"in lenticular layers, varying from an eighth of an inch to three inches in thickness, and interstratified with an indurated red clay of a slaty structure. Generally, however, the ore occurs in the shape of flat, lenticular concretions, from a quarter of an inch to one inch in diameter, locally known as 'button ore.' They have a concentric structure, are dull black on the outside and bright on the inside and are imbedded in a red or brown, fine-grained and more or less calcareous sandstone."

Analyses of the ore run as follows:

Mn	34.64	50.41
Fe	4.88	7.56
SiO_2	25.65	12.67
P_2O_5	0.58	0.06
Al_2O_3	3.79	1.37
CaO	5.13	2.09

Similar conditions to those postulated by Penrose for the accumulation of the manganese in the Arkansas region seem to me to be applicable to the Newfoundland deposits.

In writing of the circulation of the manganiferous solutions and the conditions under which they might be precipitated in the coastal shoals or lagoons, Penrose says (20: 590, 591):

"This gradual local accumulation of land and marine sediments would eventually cause shoals and possibly coastal lagoons and swamps, into which the waters from Archæan rocks of the Missouri Archipelago would drain."

"Here the solutions exposed in a stationary condition to the oxidizing and evaporating action of the atmosphere, would deposit their metalliferous contents as carbonate or possibly oxide of manganese. In some places considerable bodies of ore might be formed in one spot, in others the manganese would be dessiminated through the mechanical sediments being laid down at the same time. A secondary chemical action might cause the segregation of the disseminated manganese and the formation of concretions of carbonate of manganese, which would be later oxidized in forms such as are characteristically shown at the Cason mine, near Batesville, and elsewhere in the region. In other places the manganese might remain in a finely disseminated state, causing the common occurrence now seen throughout the region of an earthy manganiferous limestone containing from 3 to 15 per cent. of manganese."

SAXONY.—The writer was led to analyze certain of the manganese minerals from Schebenholz near Elbingerode in the Harz, which were purchased from Krantz, because of certain physical resemblances to the Newfoundland specimens. One specimen labelled "Allagite with Dialogite, etc." consists of three different materials; the first is greenish and gave the following analysis:

SiO_2	39.10	$MnSiO_3$	33.98
Fe_2O_3	1.87	$MnCO_3$	15.05
Al_2O_3	10.79	$MgCO_3$	12.64
MnO	27.69	$CaCO_3$	1.70
CaO	1.00	SiO_2	10.97
MgO	6.08	Fe_2O_3	1.75
H_2O	1.13	$2H_2O \cdot Al_2O_3 \cdot 2SiO_2$	23.76
CO_2	13.13		99.85
	100.79		

Unfortunately the early descriptions of this substance were not quoted very fully by later writers, but one of the imported specimens which was similar to the one analyzed had the following original label pasted on the back of it:

Grüner Allagite in Tomosite
eingewachsen.
73.71 Manganoxydulat.
16.00 Kieselerite.
7.50 Kohlensäure.
97.21
Schebenholz bei Elbingerode.

This analysis was published in 1817 and 1819 (Jashe 13: 1–12) and to all appearances is the same mineral analyzed by the writer, which is also labelled allagite.

Another part of the same specimen is a greenish jaspery mineral similar physically to the green band of the Newfoundland specimen and has the following composition:

		Recalculation.	
SiO_2	76.40	SiO_2	69.84
Fe_2O_3	.007	$MnCO_3$	10.00
Al_2O_3	2.46	$MnSiO_3$	8.00
MnO	10.53	$MgCO_3$	3.79
CaO	1.62	$CaCO_3$	2.80
MgO	1.81	$2H_2O \cdot Al_2O_3 \cdot 2SiO_2$	6.11
H_2O	.80		100.54
CO_2	7.32		
	100.947		

According to the recalculation this material is a manganiferous argillaceous chert, and is in all probability the silicious schist or shale of the Culm referred to later on.

The third portion of the specimen analyzed is a pinkish sparry mineral occurring as small veins with the following composition:

		Recalculation.	
SiO_2	7.10	$MnCO_3$	76.13
Fe_2O_3	.62	$MnSiO_3$	8.92
Al_2O_3	.76	$CaCO_3$	4.00
MnO	52.01	$MgCO_3$	2.44
CaO	2.26	SiO_2	2.16
MgO	1.17	Fe_2O_3	.37
H_2O	1.70	$2H_2O \cdot Al_2O_3 \cdot 2SiO_2$	5.98
CO_2	32.20		100.00
	97.82		

This mineral, because of its similarity to another specimen with

a label which reads "Spathiger Diallogit" pasted on it, is probably diallogit. It is however a very impure rhodochrosite.

According to W. Holzberger (11: 383) and C. Zerrenner (25: ?—) the ores from the Kaiser Franz mine near Elbingerode, in the Harz, occur as pocket-shaped intercalations a meter or so thick in the silicious shales of the Culm. The ore consists of psilomelane in dense and botryoidal masses, some pyrolusite and coatings of wad, with rhodonite, rhodochrosite and quartz present as accessories. The ore formerly worked contained on an average 60 to 63 per cent. of manganese peroxide, sometimes rising to 67 per cent. (23: 250). Zerrenner considers these manganese ores as later material separated out of the silicious shales, a theory which needs further investigation. Though the above described deposit is not the same as that from which the specimens analyzed above came from, it is no doubt similar.

The Elbingerode occurrence is similar to the deposits of SE. Newfoundland in that they are both primary manganiferous sediments. They differ in that the manganiferous zone of the former occurrence is considerably regionally metamorphosed while the Newfoundland sediments show very little change in this way. According to the above analyses, assuming that the imported specimens are representative of the region concerned, the deposits are very different in as much as they consist mostly of rhodonite and manganiferous cherts while those of Newfoundland are carbonate-oxides and oxide-carbonates of manganese.

VI. CHEMISTRY OF THE MANGANESE DEPOSITS.

The most striking feature of the accompanying analyses is the high content of MnO which ranges from 19.42 per cent. in **Analysis J**, to 49.25 per cent. in **Analysis D**, with an average content of 30.02 per cent and an average metallic manganese content of 24.64 per cent.

The manganese is present for the most part as the carbonate, $MnCO_3$ or rhodochrosite, which varies from 10.23 per cent. in the red band (**Anal. E**) to 44.39 per cent. in the green band (**Anal. A**) of the Manuels deposit. Rhodochrosite is not recognizable as such because of the impalpable fineness of grain of the deposit.

Solubility tests made of the red band (**Anal. E**) which has 27.61 per cent. of SiO_2, in which HCl was used as the solvent, show that the manganese must be present in some other combination than in that of the silicate, as the residue was about sufficient to cover the total silica, SiO_2, Al_2O_3 and P_2O_5. In **Anal. D,** it is evident that the two most important constituents are $MnCO_3$ and MnO_2 with percentages of 32.89 and 28.93 respectively and that the excess manganese calculated as the oxide is more than sufficient to form an important manganese silicate as the mineral percentage of SiO_2 is only 5.40, which fact lends support to the result of the solubility test made with the red band, **Anal. E.** A similar interpretation might be made with the Topsail ore (**Anal. I**) which is primarily an oxide ore with MnO_2— 34.25 and $MnCO_3$— 11.27. SiO_2, of which there is 10.32 per cent., probably is present in an uncombined state. The comparative instability of $MnCO_3$ would, however, lead one to suspect that the excess MnO_2, where not of primary origin, was a derivative of the carbonate and not combined with SiO_2 to form the silicate, $MnSiO_3$.

ANALYSES OF MANGANESE DEPOSITS OF NEWFOUNDLAND AND ELBINGERODE.

	SiO_2.	Fe_2O_3.	FeO.	Al_2O_3.	MnO.	CaO.	MgO.	$BaSO_4$.	P_2O_5.	H_2O.	CO_2.	Total.
Manuels:												
A, Green band	7.24	3.36	3.21	6.11	35.53	11.30	2.30	....	.....	2.98	28.06	100.09
B, Pink nod. . .	5.14	1.40	....	1.64	20.49	32.92	.01	....	.....	1.65	36.77	100 02
C, Green nod. .	10.31	7.35	....	3.68	31.76	10.47	1.80	6.43	.....	2.85	25.31	99.96
D, Brown band	10.23	1.32	.89	4.14	49.25	8.11	3.02	....	.....	1.31	21.83	100.10
E, Red band . .	27.61	4.25	1.69	6.96	26.05	9.94	3.49	....	4.71	4.73	10.57	100.00
F, 219 A 11 . . .	18.42	6.33	....	7.95	21.44	14.46	5.01	....	3.46	2.58	21.20	100.85
G,* 219 A 3 . . .	58.62	3.12	3.66	22.42	.43	1.25	.26	....	.....	.54	3.99	94.29
H, 219 A 13 . . .	25.20	10.13	....	7.67	.....	23.50	4.78	....	17.68	2.71	2.23	93.90
Topsail:												
I, 219 E 4.	18.04	4.82	....	6.58	41.26	2.24	2.39	5.40	.....	7.98	8.34	97.05
J, 219 E 6.	18.24	10.01	....	14.52	19.42	13.74	4.94	....	1.71	2.07	24.01	99.66
K, Smith Pt. . .	15.14	9.22	....	12.04	25.63	10.04	3.72	....	1.26	2.73	21.05	100.83
Elbingerode:												
L, Elbin. (−) .	39.10	1.87	....	10.79	27.69	1.00	6.08	....	.....	1.13	13.13	100.79
M, Elbin. (+)	76.40	.007	....	2.46	10.53	1.62	1.81	....	.....	.80	7.32	100.94
N, Elbin. (=).	7.10	.62	....	.76	52.01	2.26	1.17	....	.....	1.70	32.20	97.82

* Analyst, Mr. A. F. Buddington.

The evolution of Cl during the digestion of the samples with HCl is evidence that the excess Mn occurs as some peroxide. As

there is considerable water in the Topsail ore (**Anal. I**), the excess manganese probably is present as a hydrated peroxide such as psilomelane but probably in a very fine state of dissemination. The remarkable feature of the samples studied is the conspicuous absence of the dark oxides of manganese so far as macroscopic and microscopic observations are concerned but the reason for this may be, in the case of the lighter samples, anyway, that where there are abundant hematitic spherules there may be some masking. With the darker specimens studied, such as the red and brown bands at Manuels and the baritic manganese ore of Topsails (Figs. 22 and 32), the conspicuous manganiferous and ferruginous staining might easily mask finely disseminated particles of the peroxide of manganese.

Recalculated Analyses.

	$MnCO_3$.	$MnSiO_3$.	MnO_2.	$CaCO_3$.	$MgCO_3$.	$Ca_3(PO_4)_2$.	SiO_2.	Fe_2O_3.	$BaSO_4$.	H_2O.	Clay.	Total.
RA......	44.39	.....	8.08	20.11	4.21	.....	.....	3.36	....	.86	18.24	99.2
RB.....	29.32	.....	2.34	58.05	29.32	.....	3.78	1.40	....	1.06	4.07	100.02
RC.....	39.56	.....	7.30	18.61	3.79	.....	5.94	7.35	6.29	1.51	9.17	99.52
RD.....	32.89	.....	28.93	14.01	5.90	.....	5.40	1.27	....	.72	11.08	100.20
RE.....	10.23	.....	19.71	7.50	7.25	10.31	19.44	4.25	....	1.87	19.01	99.57
RF......	19.22	.....	9.36	18.61	10.54	7.50	9.18	6.22	....	.....	19.61	100.24
RG.....												
RH.....	.....	.....	.....	4.70	9.19	38.77	16.20	10.06	....	.....	19.11	98.03
RI}......	11.27	.....	34.25	4.00	4.97	.....	10.32	4.82	5.40	5.41	16.30	96.74
RJ......	16.79	.....	.....	20.91	10.37	3.75	1.20	10.01	....	.....	36.38	99.41
RK.....	26.91	.....	9.00	15.21	7.75	2.50	.84	9.22	....	.....	30.06	101.49
						Elbingerode						
RL......	15.05	33.98	.....	1.70	12.64	.....	10.97	1.75	....	.....	23.76	99.85
RM.....	10.00	8.00	.....	2.80	3.79	.....	69.84	.....	....	.....	6.11	100.54
RN.....	76.13	8.92	.....	4.00	2.44	.....	2.16	.37	....	.....	5.98	100.

The two most conspicuous mineral associations of the manganese deposits of southeastern Newfoundland are the tricalcium phosphate, $Ca_3(PO_4)_2$, and barite, $BaSO_4$. Only a few of the beds were analyzed for the former of these constituents where percentages of $Ca_3(PO_4)_2$ ranged from 2.50 at Smith Point (**Anal. K**) to 10.31 (**Anal. E**) at Manuels. **Anal. H** shows 38.77 per cent. of $Ca_3(PO_4)_2$, references to which are made on pages 409 and 453. It

is quite probable that others of the manganiferous beds analysed are phosphatic.

Barite ($BaSO_4$) is probably more common than the analyses indicate and is probably included with the SiO_2 and CaO. It is a conspicuous associate of these deposits, as has been found to be the case with manganese deposits in other parts of the world. The chemical reason for this association of two very different chemically-acting elements, as well as the genesis of barite are discussed on pages 451–453.

Al_2O_3, though not as abundant in the important manganiferous beds as in a typical shale, which that of **Anal. G** approximates, is of sufficient abundance to connect these deposits with the argillaceous sediments. CaO and MgO are in greater amounts than in ordinary shales, giving the deposits a calcareous or dolomitic character.

From a study of the mineral percentage composition of the samples analysed, the manganese rocks are found to be essentially calcareous or dolomitic argillaceous carbonates and oxides or carbonate-oxides of manganese, with hematite, barite, and tri-calcium phosphate as the chief accessories.

The following iron determinations of the green and red shales of the manganese zone at Manuels, Conception Bay, show some interesting results.

	FeO.	Fe_2O_3.
Red shale, 210 A 4	4.58	3.86
Green shale, 219 A 3	3.66	3.12
Red band, 219 A 7	1.69	4.25
Green band, 219 A 7	3.21	3.36

It is quite evident from the above analyses that the color in the green shale A 3 and in the green band A 7 is not due entirely to the ferrous iron as we find considerable Fe_2O_3 in both. In the green shale, A 3, there is an excess of .54 per cent. of FeO over the Fe_2O_3, while in the green band, which is manganiferous, there is an excess of .15 per cent. of the ferric oxide (hematite) over the ferrous oxide. In the green band we should expect a masking of the green by hematite inasmuch as there is such an excess of the ferric over the ferrous. Thin sections of this band and the green shale reveal some hematite but in very inconsiderable amounts; not enough, at

all events, to explain the percentages as brought out in the analyses. It would seem then that the ferric iron does not exist essentially as hematite but as a silicate or some other allied mineral, and that the green color so predominant in the manganese bands and shales may be due to the ferrous and ferric silicate.

The presence of hematite in the red band has undoubtedly caused the red coloration and the same may be said in reference to the red shale, 210 A 4, where there is an excess of .72 of FeO over the Fe_2O_3, but in these there undoubtedly has been sufficient masking of the ferrous and ferric silicates of iron by the hematite.

The production of the hematite was probably brought about by the conversion of the silicate into Fe_2O_3 through oxidation.

VII. GENESIS OF THE MANGANESE DEPOSITS AND ASSOCIATED MINERALS.

So many of the sedimentary manganese deposits described in the literature are in such a highly altered condition because of oxidation and deeper seated metamorphic influences whereby the original or primary manganese minerals have been so altered as to be of little genetic significance, that the carbonate-oxide manganese ores of southeast Newfoundland, which are surely primary ores, give promise of yielding evidence of considerable value on the question of genesis. In considering the genesis of any marine sedimentary manganese deposits, we are, however, confronted with many grave difficulties because we are dealing with submarine chemical conditions of which little is known and with diagenetic processes of which still less is known. It is also very difficult to advance any suitable chemical hypothesis founded upon some reaction that successfully works out in the laboratory which will not be of doubtful application in nature. With these difficulties in mind the following subjects relating to the genesis of the manganese deposits of southeast Newfoundland will be considered: Early Cambrian physiography; Nature of deposited sediments; Conditions under which the manganese deposits were formed; Summary of genesis of manganese; Diagenetic structures, as banded, nodular and oölitic; Genesis of barite; Genesis of tricalcium phosphate; Association and separation of iron.

EARLY CAMBRIAN PHYSIOGRAPHY.—In all probability the area occupied by Trinity, Conception, Placentia, and St. Marys Bays, the included land and the western and eastern margins including the present known Cambrian outcrops, was a continuous body of water shortly after the beginning of the Cambrian transgression. West and east of this Cambrian sea were high and extensive pre-Cambrian land areas. The great crustal movements which threw the pre-Cambrian into mountain ranges probably converted the portion now occupied by the four bays and adjacent land into a narrow basin. The main topographic features of the southeastern part of Newfoundland during the beginning of the Cambrian were two land areas of great relief separated by a comparatively narrow trough which had a general north-south direction.

Whether this trough was a closed one or not, it would be difficult to prove, but from the requirements of the problem it is necessary to postulate a more or less closed basin or coastal shoals or lagoons, Concentration of manganiferous soluble salts could go on satisfactorily only in a more or less restricted shallow sea where the water was comparatively quiet. The facts that ripple marks occur occasionally in the deposits such as at Manuels and that a shallow water fauna abounds such as trilobites are sufficient indication that there was a shallow sea at this time.

NATURE OF DEPOSITED SEDIMENTS.—Into this trough during early Cambrian times great quantities of mud were brought by rivers draining the pre-Cambrian land masses and to a lesser extent by the action of the waves on the shore line. As has already been stated the greater thickness of shales in the western portion of the basin is due to the fact that sedimentation had been going on for a longer time in that part of the basin which was in all probability the deeper part. It is also quite possible that the western parts of this trough were receiving more sediments than the eastern. The shales are characterized by their predominant red color in the western parts of the basin interbedded with shales of green color and throughout the entire area by a highly manganiferous zone.

GENESIS OF THE MANGANESE ORE.—The distinctly bedded character of the manganese deposits and their occurrence in definite horizons of limited thickness and considerable horizontal range seem

to point clearly to the conclusion that the deposits are essentially of sedimentary origin, rather than products of a later ground water or weathering concentration. But beyond this conclusion, there is room for great diversity of opinion.

Two questions present themselves at the outset of the inquiry: Was the manganese deposited contemporaneously with the clastic sediments in its present degree of concentration? Or, was it somewhat disseminated through the muds and subsequently concentrated by diagenetic agents? While the first of these alternatives is held by the writer to be highly probable, no positive and final answer can be given to these and to many other questions raised by a study of the problem of genesis, although various suggestions are presented in the following pages.

Manganese exists in sea-water and has been noted by Forchhammer and by Dieulafait (6: 718) but not in sufficiently concentrated form to produce deposits similar to those under consideration. Murray and Irvine (19: 735) found that the red muds of the mid-Pacific and Indian Oceans, which were made up in large parts of basic vitreous volcanic minerals, were responsible for the large amounts of pulverulent and nodular ferromanganese. These nodules consist on the average of 29 per cent. of MnO_2 and 21 per cent. of Fe_2O_3 with the remainder largely clayey material. The basic glasses contain the only important primary manganese-bearing minerals in the ocean and the manganese is reported by Murray and Irvine to have undergone conversion into the soluble bicarbonate which upon reaching oxygenated surface waters, is decomposed with precipitation of the dioxide. The particles of MnO_2 falling to the bottom gather upon various objects which serve as nuclei for concretions, or the nuclei themselves may have been the cause for the precipitation. Murray and Hjort (17: 192) in this connection say:

> "It should be noted that these oxides need by no means necessarily assume a concretionary form. They are very commonly found as thin incrustations on granular and fragmentary objects. Furthermore many, if not most, of the pelagic clays contain intimate admixtures of finely divided brown manganese and occasionally of limonitic iron. Here the supersaturation would seem to have been so high as to transgress the metastable limit, whereupon the oxides have precipitated themselves without the intervention of nuclei; they certainly must have been precipitated from solution."

According to Leigh Fermor (8: 403) the origin of the deep-sea nodules is summed up as follows:

"1. The manganese, although probably partly derived from cosmic dust and volcanic débris, has been mostly precipitated from solution in the sea water, the manganese salts having been originally brought into the sea by rivers.

"2. The manganese oxide, although possibly partly precipitated as a result of the action of the vital processes of organisms, both vegetable and animal, has been mainly precipitated by calcium carbonate aided by the obscure process of segregation from solution round a nucleus.

"3. Where the sea-bottom consists largely of calcareous sediments, the precipitation may have been mainly brought about by the solution of some of this calcium carbonate with the deposition of an equivalent amount of manganese oxide owing to the presence of free oxygen.

"4. Where the sea-bottom consists of red clay, it does so because the depths are there so great that the tests of thin-shelled organisms are completely dissolved by the sea-water before they reach the bottom. The calcareous matter in being dissolved deposits an equivalent amount of manganese oxide, which descends to the bottom, and there acts as a nucleus for the segregative extraction of manganese from the waters at the sea-bottom. The deposition of manganese oxide by means of calcium carbonate associated with the red clays probably also occurs to a subordinate extent, for the shells of thick-shelled organisms may reach the bottom before being entirely dissolved."

This summary of Fermor's is quoted in full here because of the marked divergence of his views from those of Murray and Irvine, and because of the greater stress laid upon Penrose's idea of the precipitation of manganese oxide by calcium carbonate.

It is the belief of the writer that the early Cambrian Sea of southeastern Newfoundland must have had so restricted and shallow a character as to allow of a concentration of the manganese salts sufficient to form deposits of such dimensions and character as we now find. Whether the manganese was brought down entirely in solution or only partially so, or entirely or partly in mineral combination as fine muds from which the manganese was subsequently dissolved, one cannot say at present. Both muds and solutions probably have contributed the manganese which forms in great part the deposits as we now find them.

The conditions which brought about the formation of the carbonate and oxide of manganese are problematical. It is generally

supposed that manganese exists in solution as a bicarbonate or a sulphate. In their work on the Blue Muds of the Clyde Sea area, Murray and Irvine (19: 728) found that the bicarbonate of manganese was derived "first from the direct decomposition of the rock fragments in the mud by the alkaline carbonates in the sea water or, second, from the reduction of the higher oxides of manganese by the organic matter in the muds." In many respects the Clyde Sea area of England is similar to what the lower Cambrian sea of Newfoundland must have been. It receives detritus and waters draining lands which are in large part of an igneous and sedimentary character (19: 780).

> "What is known as the Clyde Sea Area consists of a series of submarine basins, separated from each other by submarine barriers. The depth of the basins ranges from 30 to 106 fathoms, and the depth of water over the intervening ridges varies from 3 to 15 fathoms. In all the deeper parts of the basins there is a bluish mud, in which, as a rule, no manganese nodules are found, but on the immediate surface of the deposit of Blue Mud there is a surface layer with a reddish or light gray color, in which deposits of manganese dioxide occur. When stones are dredged from these muds many of them are surrounded by a dark ring of manganese dioxide, marking the depth to which they have been embedded in the mud. The whole upper surface of the stones has likewise a slight coating of manganese, while a portion imbedded in the mud is free from these manganese deposits."

He goes on to say that

> "The formation of manganese nodules on the immediate surface of the deposit, on the tops of the barriers, and in the pit-like depressions, is most probably to be accounted for by the more abundant supply of oxygen, or the diminished amount of decomposing organic matter in these positions."

A somewhat similar set of conditions probably was present in the muds and superjacent sea water of the Cambrian basin of Newfoundland with the exception that instead of all the bicarbonate being converted into the dioxide the greater proportion of it was precipitated as the carbonate of manganese ($MnCO_3$). The liberation of CO_2 from the bicarbonate of calcium in solution has been experimentally effected by evaporation, increasing the temperature, or through agitation of the solution. It would seem to the writer that the liberation of the CO_2 from the manganese, calcium and magnesium bicarbonates might have taken place through evapora-

tion resulting in a contemporaneous formation of manganese, calcium and magnesium carbonate. As the analyses show from 1.25 to 32.92 per cent. of $CaCO_3$ and from .01 to 5.01 per cent. of $MgCO_3$ this would seem to support such an action.

There is a possibility that the decomposing organic matter present in the muds might have caused a deoxidation of the sulphates of the sea-water and of MnO_2 with the subsequent formation of FeS_2 and MnS_2. The latter, being very unstable, would pass immediately into the bicarbonate to be subsequently freed of its CO_2 to form the carbonate and if oxidized would pass into the dioxide. Such a process might account for the carbonates and oxides of manganese and the little pyrite that occurs. Though there is evidence of life in the manganese deposits of Newfoundland as furnished by the fossil trilobites, pteropods and phosphatic accumulations, we have no evidence that there was any great abundance. However these deposits resemble the Blue Muds studied by Dittmar (6: 43) which are a variety of terriginous deposit which

> "covers about 15,000,000 square miles of the sea bed, and is chiefly found in estuaries, harbours, enclosed seas, and along continental coasts where rivers pour their detrital matter into the ocean."

According to the "Challenger researches" there is an abundant fauna on these muds, which feeds chiefly on the organic remains that fall from surface waters. If any analogy can be made between the ancient terriginous deposits and the more modern ones such a chemical action as described above might very well have taken place.

If the muds on the bottom of the basin contained considerable quantities of decomposing organic matter, conditions would favor a reduction of the higher oxides of manganese, the evolution of much CO_2 and the consequent formation of the bicarbonate of manganese. The subsequent liberation of the excess CO_2 from the bicarbonate to form the carbonate and, where oxidizing influences are active, the oxidation of this carbonate would complete a series of reactions capable of forming the manganese deposits with which we are dealing. It is very probable that these muds contained considerable quantities of decomposing organic matter and were evolv-

ing considerable CO_2. According to the "Challenger researches," when a large quantity of carbonic acid was found in oceanic waters it was "at the bottom over Blue Muds." The great difficulty in this series of reactions is to find in nature the conditions which will bring about the liberation of the excess CO_2 from the bicarbonate to form the carbonate, such as evaporation, increase of temperature, or agitation. If quiet waters are postulated for the formation of manganese carbonate it is quite conceivable that either of the conditions such as evaporation or an increase of temperature might easily be obtained particularly in shoal waters. It is very doubtful, however, in the case of agitated waters whether laboratory conditions can be simulated in nature, because of oxidizing influences whereby some oxide of manganese would form more readily than a carbonate. After the carbonate had formed there would be no particular difficulty in conditions being present which would bring about the oxidation of the carbonate because of the presence of oxygen. The excess oxide of manganese found in the Newfoundland deposit may in part have originated in this way.

Penrose (20: 563) suggested that "carbonate of lime on the sea floor may have acted as a precipitating agent" or as it passes through the sea-waters in the form of organic remains or mineral particles a substitution takes place whereby a solution of the calcium carbonate with a corresponding precipitation of manganese occurs. Fermor develops this suggestion in his explanation of the origin of the deep sea nodules as quoted on page 444. Such an explanation might apply to the origin of the primary oxides of the Newfoundland deposits.

It is possible that manganese may have been present in the sea-water as a chloride. L. De Launay (5: 533) says that "manganese chloride with sodium bicarbonate produces manganese carbonate."

When we stop to consider that manganese only averages .07 per cent. of the lithosphere (Clark, 2: 32) and is 70 times less abundant than iron which averages 4.43 per cent. and compare with these figures the percentage of manganese in the deposits under consideration which is 24.64 we can obtain some idea of the enormous concentration there has been in the production of these deposits.

We have discussed the nature of the sediments and learned that these terriginous deposits must have been derived from the pre-Cambrian land masses which existed in far greater extent on the east and west of the Cambrian sea than the present areas outlined on page 373. The interbedded character of the manganiferous and argillaceous layers signify alternating conditions of chemical precipitation and mechanical deposition, there being, during the formation of the deposits, times when the Cambrian sea was more manganiferous with conditions such that precipitation of manganese carbonate and the oxide was the relatively important feature while, at other times, mechanical deposition of fine muds was the rule. It is more than likely that the greatest portion of the manganese was contributed to the sea in the form of the dissolved bicarbonate by the streams which transported the clastic sediments and that these sediments were not themselves responsible for the major contribution, though undoubtedly the manganese minerals in the muds underwent some solution both during their transit to the sea bottom and during diagenesis. The streams which were responsible for the transportation of the sediments of the manganese deposits and also held, as chief contributors of the manganese, drained the pre-Cambrian land areas above referred to. A modern river like the Ottawa which drains a pre-Cambrian area consisting in great part of Laurentian and Huronian rocks and in all probability not very different from the pre-Cambrian rivers of ancient Newfoundland, has .86 parts per million of manganese in its waters according to an analysis made in 1907 (Shutt, 22: 175).

Manganese in river water results from the solution of manganiferous silicates such as pyroxene, olivine, micas, amphiboles, epidotes and chlorites, some of which are the common and essential basic rock-forming minerals of any igneous and metamorphic pre-Cambrian area. On the decomposition of these elements the manganese is converted into carbonate or oxide and enters into solution, when conditions are favorable, as the bicarbonate, in which form it is carried to the sea, unless oxidized in transit, there to await the further changes into the oxides, MnO_2 and Mn_2O_3, or the carbonate, $MnCO_3$, depending upon the conditions suggested in the preceding pages. Analyses of some of the pre-Cambrian rocks in the vicinity

of Conception and Trinity Bays may be of interest at this point as illustrating the manganese content of some of the rocks which are most like those existing during the formation of the deposits:

	MnO.
Monzonite, Woodford	.19
Quartz porphyry, Manuels	.11
Conception sl., Random Is.	.12
Granite, Manuels	.13
Aporhyolite, Manuels	.12
Basalt, Blue Hills	.48

Analyst, A. F. Buddington.

Similar analyses have been made from the rocks of the Clyde pre-Cambrian drainage area and show from .1 to .7 of a per cent. of MnO (Murray and Irvine, 19: 722). In all probability then the pre-Cambrian rocks on the east and west of the Cambrian basin were the ultimate source of the manganese.

Summary of Genesis of Manganese.

Ultimate Source of the manganese was the manganese-bearing silicates of pre-Cambrian igneous and metamorphic rocks east and west of the Cambrian Sea.

Solution of manganese-bearing silicates and conversion of the manganese into the soluble bicarbonate; under favorable conditions oxides of manganese resulted from the oxidation of the bicarbonate of manganese.

Transportation of the manganese chiefly as the bicarbonate and to a less extent as suspended particles of oxides by pre-Cambrian drainage systems to Cambrian basins.

Concentration of the salts of manganese chiefly as the bicarbonate in the sea-water immediately overlying the deposited muds.

Precipitation of manganese carbonate from solution through liberation of CO_2 from the bicarbonate, or of the oxide.

Clastic Origin of Some Manganese.—While the main contribution of the manganese came from the pre-Cambrian drainage area in solution undoubtedly the deposited muds supplied a minor portion.

Diagenetic Structures

Banded Structures.—By referring to the description of layer 219 A 7 we see that it is a red manganiferous shale with green and brown jaspery bands which may be rather uniform in thickness and may alternate with each other. The green band predominates over the brown so that the greater alternations occur with the green and red bands. Throughout the red shale are numerous nodules of the green and brown jaspery carbonate-oxides of manganese and within the bands themselves are nodular and concretionary forms. The alternating banded and concretionary forms within this bed would indicate alternating conditions of precipitation followed by diagenetic segregational processes which resulted in the formation of nodules and lenticles. Very thin and interrupted laminæ of the red band are found with the green bands. The green and brown bands often occur intergrown with each other. From these observations it would seem that these banded structures were evidence of alternate periods of precipitation and that they have assumed their present indurated and concretionary nature by segregational processes which were active throughout the diagenesis of the bed.

Nodules.—One of the most characteristic features of the shales of the Lower Cambrian is the great prevalence of the nodules (Figs. 14 and 15). The following suggestion is offered as to the origin of the form of these nodules with the hope that this line of investigation may be taken up in greater detail at some future time. Though various theories have been suggested for the origin of oölitic spherules and nodules, in general, along organic and inorganic lines, nothing of a very definite nature has been brought out as to the origin of their form. The suggestion that surface tension may be the cause of this form is here made. This peculiar and prevalent nodular character of certain beds was brought about in all probability by the tendency of surface tension to decrease the surface during the diagenetic stage. Solutions carrying manganese filtering through muds or nearly consolidated muds or shales would quite naturally under certain chemical and physical conditions have the tendency to decrease the surface tension at the contact of the three physical phases; liquid, colloid, and solid. Starting with a

mineral particle such as rhodochrosite or calcite as a nucleus, with the formation of the nodule, there will be a decrease in the concentration of the solution at the contact with the nodule which will be accompanied by a reduction of surface tension. If we are dealing with a liquid-liquid phase we would have a spherical nodule in which case both liquids would be easily deformable and the surface would tend to become a minimum. Our twofold phase, liquid-solid, or threefold phase including the colloidal phase which probably plays a part, only allows of deformability on the part of the liquid and partial deformability on the part of the nodule. Under the bedded conditions of this two or three fold solution, colloid and solid phase the tendency of the surface tension to reduce the surface to a minimum is well exemplified in the discoidal nodule.

SPHERULES.—One of the characteristic features of this deposit is the occurrence of hematite in spherule-like forms and larger, roughly spherical aggregates. Fig. 26 illustrates the occurrence. They differ decidedly from the spherules of the Wabana, Clinton, and other typical oölitic iron ores in that they are less symmetrical and are without any visible nuclei. These spherules are here described as incipient in as much as they seem to lack full development or to have been impeded in their growth. Such a retardation of development might have arisen from their growth in clayey sediments which were still unconsolidated.

MINERAL ASSOCIATIONS.—The three important mineral associaciations of the manganese deposits of S. E. Newfoundland are barite, tri-calcium phosphate and hematite which will now be considered with reference to their occurrence, association and genesis.

BARITE.—Barite is one of the most characteristic mineral associations of the deposits under consideration as is often the case with manganese deposits elsewhere in the world. It is particularly characteristic of the Manuels, Topsail and Smith Point localities and occurs in various ways.

Barite is found in small veins crossing a cryptozoan nodule showing quite clearly its epigenetic character so far as that particular portion of the bed is concerned. Fig. 23 (Slide 276) shows a solitary crystal fragment of barite in a carbonate-oxide of man-

ganese groundmass showing possibly a diagenetic replacement. Barite also occurs as disseminated anhedral crystal grains or blades in the cores and outer zones of nodules at Manuels, which is very suggestive of diagenetic processes (Fig. 16, Slide 288). At Topsail (Fig. 31, Slide 269) barite occurs as bundles of blades or sheath-like aggregates in a manganese oxide groundmass strongly suggesting replacement.

In other parts of the world barium is often found replacing manganese in psilomelane and sometimes enters largely into the composition of wad, specimens from Romanèche containing as much as 16.2 per cent. of BaO (Dana, 3: 258). A very striking phenomenon shown by the barite is its replacement by chlorite (Fig. 12, Slide 296, and Fig. 34, Slide 272).

Just why there is this common association of two very unlike elements we have no definite information. De Launay (4: 52) gives the following explanation for epigenetic deposits:

> "The association between barite and manganese though very frequently exhibited in surface formations, in many cases these two substances are being concentrated by circulating waters in pockets or fissures of terranes."

Various conditions may produce barite with barium salts in solution but only one seems to apply to the occurrences under consideration. As there are evidences of diagenetically and epigenetically formed barite in the deposits, it is quite possible that there has been an intermingling of solutions carrying barium carbonate and some sulphate resulting in the formation of barite. According to De Launay (4: 52)

> "Barite being remarkably insoluble is one of those barium compounds which not only has the propensity to segregate and all at once to be transformed into the carbonate but also the tendency under the influence of H_2SO_4 produced by the superficial oxidation of the metallic sulphides to pass into the state of barite."

The replacement of the colorless barite by the pale green chlorite begins about the edges and along cleavage cracks of the former. The chlorite gradually spreads while the intervening portions of barite decrease until wholly eliminated, resulting in a pseudomorph of chlorite after barite. In general appearance of its various stages,

the process is quite like the serpentinization of olivine but differs essentially from the latter alteration in the fact that the secondary mineral, chlorite, derives none of its material from the original mineral, barite, its change involving a complete replacement by wholly new material. It is a marked example of the comparative ease with which substances which, like barium sulphate are regarded in the laboratory as very stable, yield to the attack of natural reagents.

This replacement seems to have accompanied a more or less general chloritization of the whole formation, at a period long subsequent to the concentration of the manganese ore and under totally different conditions.

Phosphate.—Tri-calcium phosphate, $Ca_3(PO_4)_2$ also is a very conspicuous accessory of the manganese deposits of Newfoundland, averaging, for those beds of which analyses were made, about 6.0 per cent. and for the phosphatic nodules of the nodular bed overlying the manganese zone at Manuels, 38.77 per cent. When we stop to consider the amount of phosphorus in the lithosphere as .11 per cent. (Clark, 2: 32) the amount of concentration in these deposits, particularly in the nodules, becomes very noticeable and something of great interest. The similarity in chemical composition of the phosphatic nodules of Manuels brook and those of Hanford brook, N. B., has been referred to on page 409. As the writer has been unable to make as thorough a study of these nodules as he would have liked, it is hoped that at some future time the investigation may be continued. At this time then a very brief resumé of the modes of concentration of phosphorus may be of interest because of apparent application to the deposit under consideration.

According to De Launay (5: 646) there are three stages in the concentration of phosphatic deposits, namely solution of calcium phosphate, in which he considers that in surface conditions

"the constant presence of carbonic acid and sodium chloride or chlorhydrate of ammonia in the waters determines the solution of phosphate."

The second stage is that in which organisms play an important rôle.

"The faculty which live organisms have of throwing into very dilute solutions those substances which to them are necessary and of making them undergo a primary stage of concentration has played a great rôle for the phosphates." De Launay (5: 646).

The third stage called by De Launay, "Remises en mouvement" consists in a dissolution of the phosphate contained in preceding deposits which is followed by a reprecipitation of the same upon anything which has served as a center of attraction. The tendency in this mode of concentration is for the phosphate to become more and more like the original apatite in composition, the ultimate source of the phosphorus. It involves both a chemical and a mechanical action, the former in dissolution and reprecipitation and the latter in the formation of nodules which, according to the suggestion of the writer in connection with the manganese nodules of Manuels, may be of physical nature, namely the result of surface tension.

IRON.—An interesting, and yet problematical, point arises here in connection with the association and separation of iron and manganese as related to the manganese deposit under consideration. We should expect, in as much as both elements are taken into solution, that they both might be precipitated together as is sometimes the case with bog ores or, if separated, at no great stratigraphic distance. Because of their different rates of oxidation and different degrees of solubility, however, a separation is effected. Assuming both elements entering into solution contemporaneously, the iron would oxidize first, precipitating as Fe_2O_3, while the manganese, remaining in solution longer, is precipitated either as MnO_2, Mn_2O_3 or $MnCO_3$. Though the Newfoundland manganese deposits contain iron, it is much less in proportion to what it would be if both were precipitated together (see Analyses, p. 438) considering the relative abundance of the two elements in the lithosphere referred to on page 447.

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Princeton University,
June, 1914.

MINUTES

MINUTES.

Stated Meeting January 1, 1915.

William W. Keen, M.D., LL.D., President, in the Chair.

The decease was announced of Charles Martin Hall, A.M., LL.D., of Niagara Falls, at Daytona, Florida, on December 27, 1914; æt. 51.

Prof. Edwin G. Conklin read an obituary notice of Prof. August Weismann.

Prof. William B. Scott read a paper on "The Isthmus of Panama in its Relation to the Animals of North and South America."

The Judges of the Annual Election of Officers and Councillors, held on this day between the hours of two and five in the afternoon, reported that the following named members were elected to be the Officers for the ensuring year, according to the Laws, Regulations and Ordinances of the Society.

President.

William W. Keen.

Vice-Presidents.

William B. Scott,
Albert A. Michelson,
Edward C. Pickering.

Secretaries.

I. Minis Hays,
Arthur W. Goodspeed,
Amos P. Brown,
Harry F. Keller.

Curators.

Charles L. Doolittle,
William P. Wilson,
Leslie W. Miller.

Treasurer.

Henry La Barre Jayne.

Councillors.

(To serve for three years.)
Henry H. Donaldson,
Theodore W. Richards,
Robert A. Harper,
Edwin G. Conklin.

Special Meeting January 14, 1915.

WILLIAM W. KEEN, M.D., LL.D., President, in the Chair.

Dr. J. C. Bose, of Calcutta, read a paper on "The Control of Nervous Impulse in Plant and Animal."

Stated Meeting February 5, 1915.

WILLIAM W. KEEN, M.D., LL.D., President, in the Chair.

An invitation was received from the University of North Carolina to be represented at the Inauguration of Edward Kidder Graham, as President, at Chapel Hill, on the twenty-first of April.

A letter was received from Prof. Allen C. Thomas presenting his resignation of membership.

The decease was announced of the following members:

Benjamin Sharp, M.D., at Moorehead, N. C., on January 23, 1915; æt. 57.

Cyrus Fogg Brackett, A.B., LL.D., at Princeton on January 29, 1915; æt. 82.

The following papers were read:

"The Surgery of the Civil War as Contrasted with the Surgery of the Present European War," by W. W. Keen, M.D.

"The Antediluvian Patriarchs on a Tablet from Nippur," by George A. Barton, Ph.D., which was discussed by Professor Learned and Mrs. Stevenson.

Stated Meeting March 5, 1915.

William W. Keen, M.D., LL.D., President, in the Chair.

Dr. David Jayne Hill, elected to membership in 1910, subscribed the Laws and was admitted into the Society.

Letters were received:

From the President appointing Prof. W. LeConte Stevens to represent the Society at the inauguration of Edward Kidder Graham, as President of the University of North Carolina.

From Prof. W. LeConte Stevens, accepting the appointment.

The decease of the following members was announced:

Arthur vonAuwers, at Berlin on January 24, 1915.

James Geikie, LL.D., D.C.L., at Edinburgh, on March 2, 1915; æt. 75.

The following papers were read:

"The Swedes, Governor Printz, and the Beginning of Pennsylvania," by Thomas Willing Balch.

"A Missing Chapter in International History," by Hon. David Jayne Hill, which was discussed by Mr. Carson, Prof. Learned, and Mr. Rosengarten.

Stated Meeting April 9, 1915.

William W. Keen, M.D., LL.D., President, in the Chair.

An invitation was received from the Trustees and Faculty of Allegheny College, to be represented at the celebration of the One Hundredth Anniversary of the founding of the College, to be held at Meadville, Pa., in the week beginning the twentieth of June, 1915.

The decease of the following members was announced:

Charles Francis Adams, LL.D., at Washington on March 20, 1915; æt. 80.

Frederick Winslow Taylor, M.E., at Philadelphia on March 21, 1915; æt. 59.

David K. Tuttle, Ph.D., at Philadelphia on April 7, 1915; æt. 79.

Prof. Charles C. Bass read a paper on "Some Important Factors which Influence Asexual Reproduction of Malaria Plasmodia in Man," which was discussed by Dr. Tyson, Dr. Henry Skinner, Dr. McFarland, and Dr. Keen.

Stated General Meeting April 22, 23, and 24, 1915.

Thursday, April 22, 1915.

ALBERT A. MICHELSON, Ph.D., Sc.D., LL.D., F.R.S.,
Vice-President, in the Chair.

Prof. Eliakim H. Moore, elected to membership in 1905, and Prof. Robert Andrews Millikan, elected to membership in 1914, having subscribed the Laws, were admitted into the Society.

The following papers were read:

"Devices for Facilitating the Analysis of Observations—More Particularly those of the Tides," by Ernest W. Brown, Sc.D., Professor of Mathematics, Yale University.

"On Linear Integral Equations in General Analysis," by Eliakim H. Moore, Ph.D., Sc.D., LL.D., Head of Department of Mathematics, University of Chicago.

"A Direct Solution of Fredholm's Equation with Analytic Kernel," by Preston A. Lambert, Professor of Mathematics, Lehigh University, Bethlehem, Pa.

"The Existence of a Sub-Electron?" by Robert A. Millikan, Ph.D., Professor of Physics, University of Chicago, which was discussed by Prof. Michelson.

"Local Disturbances in a Magnetic Field," by Francis E. Nipher, A.M., LL.D., Professor of Physics, Washington University, St. Louis.

"Explorations over the Surface of Telephonic Diaphragms Vibrating under Simple Impressed Sounds," by A. E. Kennelly, S.D., A.M., Professor of Electrical Engineering, Harvard University and H. O. Taylor, of Cambridge.

"The Hall and Corbino Effects," by Edwin Plimpton Adams, Professor of Physics, Princeton University. (Introduced by Prof. Magie.)

"Spontaneous Generation of Heat in Recently Hardened Steel," by Charles Francis Brush, Ph.D., Sc.D., LL.D., of Cleveland.

"Ruling and Performance of a Ten Inch Diffraction Grating," by A. A. Michelson, Ph.D., Sc.D., LL.D., Head of Department of Physics, University of Chicago.

Friday, April 23.

Morning Session—9.35 o'clock.

William W. Keen, M.D., LL.D., President, in the Chair.

The following papers were read:

"Heredity in Protozoa," by M. H. Jacobs, Ph.D., Assistant Professor of Zoölogy, University of Pennsylvania. (Introduced by Prof. McClung.)

"The Constitution of the Hereditary Material," by T. H. Morgan, Ph.D., Professor of Experimental Zoölogy, Columbia University, New York. (Introduced by Prof. E. G. Conklin.) Discussed by Prof. Conklin.

"The Problem of Adaptation as Illustrated by the Fur Seals of the Pribilof Islands" (illustrated by lantern slides), by George H. Parker, Sc.D., Professor of Zoölogy, Harvard University. Discussed by Professors Conklin and Cattell.

"An Interpretation of Sterility in Hybrids," by Edward M. East, Ph.D., Professor of Experimental Plant Morphology, Harvard University. (Introduced by Prof. Bradley Moore Davis.)

"Heterosis and the Effects of Inbreeding," by George H. Shull, Ph.D., Botanical Investigator, Station for Experimental Evolution, Carnegie Institution. (Introduced by Prof. Bradley M. Davis.)

"The Significance of Sterility in Œnothera," by Bradley M. Davis, Ph.D., Professor of Botany, University of Pennsylvania.

The preceding three papers were discussed by Prof. Parker.

"Morphology and Development of Agaricus rodmani," by George F. Atkinson, Ph.D., Head of Department of Botany, Cornell University.

"The Large-fruited American Oaks," by William Trelease, Sc.D., LL.D., Professor of Botany, University of Illinois, Urbana.

"Relationships of the White Oaks of Eastern North America," by M. V. Cobb. (Introduced by Prof. Trelease.)

"The Present Need in Systematic Botany," by L. H. Bailey, LL.D., late Director of the College of Agriculture, Cornell University.

Afternoon Session—2 o'clock.

William W. Keen, M.D., LL.D., President, in the Chair.

The following papers were read:

"A Convenient Form of Receiver for Fractional Distillations under Diminished Pressure,'" by Marston T. Bogert, LL.D., Professor of Chemistry, Columbia University, New York.

"The Cymene Carboxylic Acids," by J. R. Tuttle and Marston T. Bogert, of Columbia University, New York.

"Syringic Acid and its Derivatives," by E. Plaut and Marston T. Bogert, of Columbia University, New York.

The three preceding papers were discussed by Profs. Keller and Scott.

"The Relation of Ductless Glands to Dentition and Ossification," by William J. Gies, Ph.D., Professor of Biological Chemistry, Columbia University. (Introduced by Prof. Harry F. Keller.)

"Gastro-Intestinal Studies," by Philip B. Hawk, Ph.D., Professor of Physiological Chemistry and Toxicology, Jefferson Medical College, Philadelphia. (Introduced by Dr. W. W. Keen.) Discussed by Prof. Scott.

"On the Rate of Evaporation of Ether from Oils and its Application in Oil-Ether Colonic Anæsthesia," by Charles Baskerville, Ph.D., Professor of Chemistry, College of the

City of New York. (Introduced by Prof. J. P. Remington.) Discussed by Profs. Scott and Remington.

"On Oral Endamebiosis," by Allen J. Smith, M.D., Sc.D., LL.D., Professor of Pathology, University of Pennsylvania. Discussed by Prof. Bogert.

"Certain Factors Conditioning Nervous Responses," by Stewart Paton, M.D., Lecturer in Biology, Princeton University. Discussed by Profs. Donaldson and Scott.

"The Rights and Obligations as to Neutralized Territory," by Charlemagne Tower, LL.D., of Philadelphia.

"Physiographic Features as a Factor in the European War," by Douglas W. Johnson, Ph.D., Associate Professor of Physiography, Columbia University. (Introduced by Mr. Henry G. Bryant.)

"The Pronouns and Verbs in Sumerian," by J. Dyneley Prince, Ph.D., Professor of Semitic Languages, Columbia University, New York. (Read by title.)

"A New Form of Nephelometer," by J. T. W. Marshall, Harriman Research Laboratory, Roosevelt Hospital, New York.

Dr. Stewart Paton a newly-elected member and the Hon. Simeon E. Baldwin, elected to membership in 1910, subscribed the Laws and were admitted into the Society.

Friday Evening—8.15 o'clock.

William Morris Davis, Sc.D., Ph.D., Professor Emeritus of Geology, Harvard University, gave an illustrated lecture, "On New Evidence for Darwin's Theory of Coral Reefs." A statement of the chief results of a Shaler Memorial Voyage across the Pacific in 1914, with Studies of the Fiji Group, New Caledonia, the Loyalty Islands, the New Hebrides, the Great Barrier Reef of Australia and the Society Islands.

Saturday, April 24.

Executive Session—9.30 A. M.

William W. Keen, M.D., LL.D., President, in the Chair.

Pending nominations for membership were read and spoken to.

Dr. L. A. Bauer and Secretary Brown were appointed tellers of election and the Society proceeded to ballot for members.

The tellers reported that the following nominees had been elected to membership:

Residents of the United States.

John J. Abel, M.D., Baltimore, Md.
Edwin Plimpton Adams, Ph.D., Princeton, N. J.
Walter Sydney Adams, Pasadena, Cal.
John Merle Coulter, Ph.D., Chicago, Ill.
Whitman Cross, Ph.D., Washington, D. C.
William J. Gies, M.D., New York City.
Philip Bovier Hawk, Ph.D., Philadelphia.
John Fillmore Hayford, Evanston, Ill.
Emory Richard Johnson, Sc.D., Philadelphia.
John Anthony Miller, Ph.D., Swarthmore, Pa.
Thomas Hunt Morgan, Ph.D., New York.
William Fogg Osgood, Ph.D., Cambridge, Mass.
Raymond Pearl, Ph.D., Orono, Me.
Theobald Smith, M.D., Boston, Mass.
John Zeleny, Ph.D., Minneapolis, Minn.

Morning Session—10 o'clock.

WILLIAM B. SCOTT, Sc.D., LL.D., Vice-President, in the Chair.

The following papers were read:

"Opium in the Bible," by Paul Haupt, Ph.D., Professor of Semitic Languages, Johns Hopkins University, Baltimore.

"Divisions of the Pleistocene of Europe and the Periods of the Entrance of Human Races," by Henry Fairfield Osborn, Sc.D., LL.D., Research Professor of Zoölogy, Columbia University, N. Y.

"The Occurrence of Algæ in Carbonaceous Deposits," by Charles A. Davis, Ph.D., of U. S. Bureau of Mines. (Introduced by Prof. Marston T. Bogert.) Discussed by Profs. Scott and B. M. Davis.

"Additions to the Fauna of the Lower Pliocene Snake Creek Beds, Nebraska," by W. J. Sinclair, Ph.D., Curator of Vertebrate Paleontology, Princeton University. (Introduced by Prof. W. B. Scott.) Discussed by Prof. Scott.

"Tertiary Vertebrate Faunas of the North Coalinga Region of California," by John C. Merriam, Ph.D., Professor of Paleontology and Historical Geology, University of California.

"The Rôle of the Glacial Anticyclone in the Air Circulation of the Globe," by William H. Hobbs, Ph.D., Professor of Geology, University of Michigan.

"Note on the Sun's Temperature," by Henry Norris Russell, Ph.D., Professor of Astronomy, Princeton University.

"Radial Velocities in the Orion Nebula," by Edwin B. Frost, D.Sc., Director of Yerkes Observatory, Williams Bay, Wis. Discussed by Prof. Russell.

"Some Results from the Observation of Eclipsing Variables," by Raymond S. Dugan, Assistant Professor of Astronomy, Princeton University. (Introduced by Prof. H. N. Russell.)

"The Variable Stars TV, TW, and TX Cassiopeiæ," by R. J. McDiarmid, Fellow, Princeton University. (Introduced by Prof. H. N. Russell.)

"The Euler-Laplace Theorem on the Rounding Up of the Orbits of the Heavenly Bodies under the Secular Action of a Resisting Medium," by T. J. J. See, Ph.D., U. S. Naval Observatory, Mare Island, Cal.

"The Work in Atmospheric Electricity aboard the 'Carnegie,'" by L. A. Bauer, Ph.D., D.Sc., Director of the Department of Terrestrial Magnetism of the Carnegie Institution of Washington, and W. F. G. Swann, D.Sc.

"Tammuz and Osiris," by George A. Barton, Ph.D., Prof. of Biblical Literature and Semitic Languages, Bryn Mawr College, which was discussed by Dr. Jastrow.

Afternoon Session— 2 o'clock.

William W. Keen, M.D., LL.D., President, in the Chair.

Dr. John Fillmore Hayford, a newly elected member, subscribed the Laws and was admitted into the Society.

A portrait of Dr. Edgar F. Smith, of Philadelphia, was presented to the Society by Dr. J. H. Penniman on behalf of the donor.

The following papers were read:

"One Dimensional Gases and the Reflection of Molecules from Solid Walls."

"Recent Progress in the Study of the Iodine Resonance Spectra, with a description of a Long Focus Spectroscope of High Power," by Robert Williams Wood, A.B., LL.D., Professor of Experimental Physics, Johns Hopkins University. Discussed by Dr. Brush, Prof. Noyes and Dr. Webster.

Symposium on the Earth: Its Figure, Dimensions and the Constitution of Its Interior

"From the Astronomical Standpoint," by Frank Schlesinger, Ph.D., Director of Allegheny Observatory, Pittsburgh.

"From the Geological Standpoint," by T. C. Chamberlin, Ph.D., LL.D., Head of Department of Geology, University of Chicago.

"From the Seismological Standpoint," by Harry Fielding Reid, Ph.D., Professor of Dynamical Geology and Geography, Johns Hopkins University, Baltimore.

"From the Geophysical Standpoint," by John F. Hayford, Director of College of Engineering, Northwestern University, Evanston, Ill. (Introduced by Prof. Schlesinger.)

These papers were discussed by Professors H. F. Reid, Arthur G. Webster, E. W. Brown, C. L. Doolittle, Frank Schlesinger and W. H. Hobbs.

Stated Meeting May 7, 1915.

William W. Keen, M.D., LL.D., President, in the Chair.

Letters accepting membership were received from:

John J. Abel, M.D., Baltimore, Md.
Edwin Plimpton Adams, Ph.D., Princeton, N. J.
John Merle Coulter, Ph.D., Chicago, Ill.
Whitman Cross, Ph.D., Washington, D. C.
William J. Gies, M.D., New York City.
Philip Bovier Hawk, Ph.D., Philadelphia.
Emory Richard Johnson, Sc.D., Philadelphia.
John Anthony Miller, Ph.D., Swarthmore, Pa.

Thomas Hunt Morgan, Ph.D., New York.

Raymond Pearl, Ph.D., Orono, Me.

Theobald Smith, M.D., Boston, Mass.

An invitation was received from the Johns Hopkins University to be represented at the Inauguration of Frank Johnson Goodnow, as President, on May 20, 1915.

The following papers were read:

"Oil Concentration of Ores," by Howard W. DuBois, M.E. (Introduced by Dr. Harry F. Keller.) Discussed by Mr. Lehman.

"Concretions in Streams Formed by the Agency of the Blue-Green Algæ and Related Plants," by H. Justin Roddy, M.S. (Introduced by the Secertaries.) Discussed by Dr. Harshberger, Mr. Sanders, and Prof. Keller.

"The Conditions of Black Shale Deposition as illustrated by the Kupferschiefer and Lias of Germany," by Charles Schuchert.

Stated Meeting October 1, 1915.

William W. Keen, M.D., LL.D., President, in the Chair.

Professors Philip B. Hawk, Emory R. Johnson, and John Anthony Miller, newly-elected members, subscribed the Laws and were admitted into the Society.

Letters accepting election to membership were received from

Prof. Walter S. Adams.

Prof. John F. Hayford.

Prof. William F. Osgood.

Prof. John Zeleny.

Invitations were received:

From the Secretary of State to participate in the Second Pan-American Scientific Congress to be held under the auspices of the Government of the United States, at the city of Washington from December 27, 1915, to January 8, 1916.

From the Trustees and Faculty of Vassar College, to be represented at the celebration of the Fiftieth Anniversary of the opening of Vassar College during the week beginning October 10, 1915.

The decease of the following members was announced:

Mr. Samuel Dickson, at Philadelphia, on May 28, 1915, æt. 78.

Hon. James T. Mitchell, at Philadelphia, on July 4, 1915, æt. 81.

Mr. Frederick Prime, at Atlantic City, on July 14, 1915, æt. 69.

Sir James A. H. Murray, at Oxford, England, on July 26, 1915, æt. 78.

Prof. Frederick W. Putnam, at Cambridge, Mass., on August 14, 1915, æt. 76.

Mr. John T. Morris, at Bretton Woods, N. H., on August 15, 1915, æt. 67.

Dr. Austin Flint, at New York, on September 22, 1915, æt. 79.

The following papers were read:

"Timber Studies in the Mississippi Bottom Lands," by Henry C. Cowles, Ph.D., of Chicago, which was discussed by Professors Harshberger and Kraemer.

"A Practical Rational Alphabet," by Benjamin Smith Lyman, A.B.

Stated Meeting November 5, 1915.

William W. Keen, M.D., LL.D., President, in the Chair.

Prof. Ulric Dahlgren read a paper on "The Production of Heat by Animals."

Stated Meeting December 3, 1915.

William W. Keen, M.D., LL.D., President, in the Chair.

An invitation was received from the American Association for the Advancement of Science to send one or more delegates to its meeting to be held at Columbus, Ohio, December 27, 1915, to January 1, 1916.

Mr. H. H. Harjes, in accordance with the expressed direction of his father, the late John H. Harjes, of Paris, transmitted to the Society Dr. Franklin's bamboo cane with a horn handle, in the upper hollow chamber of which he was accustomed to carry oil and from it in his walks he dropped oil upon the water to watch its effect upon wind-beaten pools.

The decease was announced of William Brooke Rawle, A.B., at Philadelphia, on November 30, 1915, æt. 72.

The following papers were read:

"Some of the Neuro-retinal Interpretations of Increased Vascular and Increased Intracranial Pressure, being a Clinical Communication," by Dr. George E. deSchweinitz.

"The Geology of Parahyba and Rio Grande do Norte, Brazil," by Ralph H. Soper, communicated by Prof. John C. Branner.

"The Geology of Ceará and Piauhy, Brazil," by H. L. Small, communicated by Prof. John C. Branner.

Dr. W. W. Keen presented the Annual Address of the President.

INDEX.

OBITUARY NOTICES
OF MEMBERS DECEASED.

AUGUST WEISMANN.

BORN JANUARY 17, 1834. DIED NOVEMBER 6, 1914.

AUGUST WEISMANN.

PLATE A.

(Read January 1, 1915.)

August Weismann, a foreign member of this Society, was born at Frankfort on the Main, January 17, 1834, and died at Freiburg in Breisgau, November 6, 1914. He early showed the traits of a naturalist and in one of his books speaks of the excitement he felt as a boy in catching butterflies. He attended the University of Göttingen, where he studied chemistry and medicine, coming especially under the instruction of the distinguished anatomist Henle, and receiving the degree of M.D. in 1856. After spending three years at Rostock as an assistant he began the practice of medicine at Frankfort and during this time he visited Vienna in 1858, Italy in 1859 and Paris in 1860. From 1861 to 1862 he was private physician to Archduke Stephan of Austria at Schamburg Palace. He then studied zoölogy at Giessen under the renowned zoölogist Leuckart and became *privat-docent* in zoölogy at the University of Freiburg in 1863, where he spent the remainder of his life. In 1866 he was appointed *professor extraordinarius* and a few years later became *professor ordinarius,* which position he continued to hold until a few years before his death, when he was made *professor emeritus.*

In person he was a man of striking appearance, being about six feet tall and well proportioned and having a fine head and face and an earnest but kind expression of the eyes. From 1864 to 1874 and again from 1884 on he suffered from an eye trouble which interfered greatly with his microscopical work and turned his attention to theoretical questions. One of his former students and assistants, Professor Alexander Petrunkewitch,[1] to whom I am indebted for much valuable information concerning his personality,

[1] I am also indebted to Prof. H. H. Wilder, of Smith College, and to Prof. J. S. Kingsley, of the University of Illinois, for information regarding the family life and personality of Weismann.

says that although he was usually quiet in manner he invariably became nervous and unhappy in the presence of moving objects, which painfully affected his eyes.

A short autobiography published in *Lamp* in 1903 gives a glimpse of his family life:

> "During the ten years (1864–1874) of enforced inactivity and rest occurred my marriage to Fraulein Marie Gruber, who became the mother of my children and was my true companion for twenty years until her death. Of her now I think only with love and gratitude. She was the one who more than any one else helped me through the gloom of this period. She read much to me at this time, for she read aloud excellently, and she not only took an interest in my theoretical and experimental work but she also gave practical assistance in it."[2]

His great work on the "Natural History of the Daphnoidea" (1876–79) is dedicated to "My father-in-law, Adolph Gruber, in thankful memory of the beautiful hours of leisure spent on the shores of Bodensee." His colleague, the anatomist Wiedersheim, married another daughter of Gruber who was a Genoese banker. After the death of his first wife Weismann married again when about sixty years old, but not happily. One of his daughters married the zoölogist W. N. Parker, who translated into English his best known work "The Germ Plasm." A son was trained as a professional violinist.

Weismann, like so many other naturalists, was of an artistic disposition. He loved nature, art and music and he was an accomplished pianist. During the periods when he suffered much from his eye trouble he says that he "found solace in playing a good deal of music." He was an enthusiastic admirer of Beethoven but could not appreciate Wagner. His artistic temperament is further shown in many of his essays which for beauty of expression are rarely surpassed in scientific literature.

He was an excellent speaker, being simple and earnest in manner and never indulging in jokes. His lectures on evolution, which were delivered regularly for almost forty years, were famous and always attracted great audiences. As a teacher of advanced students he was stimulating and helpful, a kind critic and an attentive listener.

He took no active part in politics, but like many German pro-

[2] Quoted from Locy's "Biology and its Makers," p. 401.

fessors was a member of the "National Liberal" party. In philosophy he held tenaciously to a mechanistic conception of nature, but he believed that extreme mechanism was consistent with extreme teleology, indeed he held that "The most complete mechanism conceivable is likewise the most complete teleology conceivable. With this conception vanish all apprehensions that the new views of evolution would cause man to lose the best that he possesses—morality and purely human culture." In his philosophy as in his scientific controversies he was extremely tolerant. He was interested in the promotion of knowledge but was not aggressive nor offensive in manner.

Inasmuch as his life was so largely given to the extension and support of the Darwinian theory it is interesting to hear from himself how that theory first came to his attention. After remarking, "I never heard evolution referred to in my student days," he describes the influence on himself of Darwin's book in these words:

> "I myself was at the time in the stage of metamorphosis from a physician to a zoölogist, and as far as philosophical views of nature were concerned I was a blank sheet of paper, a *tabula rasa*. I read the book ["Origin of Species"] first in 1861 at a single sitting (*sic*) and with ever growing enthusiasm. When I had finished I stood firm on the basis of the evolution theory, and I have never seen reason to forsake it."

With just pride he mentions the fact that he was one of the first scientific men in Germany to defend publicly Darwin's theory; Fritz Müller was the first to publish a work in favor of that theory ("Für Darwin," 1864), Haeckel was the second ("Generelle Morphologie," 1866) and Weismann was the third, his Inaugural Address at Freiburg on the "Justification of the Darwinian Theory" ("Über die Berechtigung der Darwin'schen Theorie") being published in 1868.

Thereafter his contributions to the Darwinian theory were numerous and important. They appeared from 1872 to 1902 as a series of books and contributions. Five of these earlier contributions were translated into English by R. Meldola and were published as two large volumes in 1882 with an introduction by Charles Darwin. Subsequent studies on evolution were so intimately associated with his theories of heredity that they can best be considered under that topic.

Weismann's contributions to biological theory were so extensive and important that they overshadow to a great extent his observational and experimental work, and yet the latter was by no means small or unimportant. Among these observational and experimental studies must be mentioned especially his extensive works on "The Development of Diptera (1865)," "Natural History of the Daphnoidea" (1876–79), "Origin of the Sex Cells of the Hydromedusæ (1883)," "Seasonal Dimorphism of Butterflies" (1875), "Origin of Markings of Caterpillars" (1876) and "Transformation of the Mexican Axolotl into Amblystoma."

Some of his earlier work was done without assistance, but in all of his later observational and experimental studies he had the assistance of his wife or other helpers. Much of his work was done in collaboration with some of his students or assistants. His method of work was to a large extent forced upon him by his eye affliction. After 1864 all reading had to be done for him, at first by his wife and after her death by a secretary. Experimental work was done under his supervision by his assistant and janitor. All microscopic work was done by his pupils, to whom he suggested topics and whose work he supervised daily. These theses were always in direct relation to his theories and to that phase of them which interested him most at the moment.

But valuable as much of his observational and experimental work was, there is no doubt that he will be remembered chiefly for his theories of heredity. His earliest writings on this subject date from the year 1883 and his latest were published but a few years before his death. His "Essays upon Heredity and Kindred Biological Topics" were translated into English and published in two volumes in 1889 and 1892. Probably his most important work on this subject is his book entitled "The Germ-Plasm, A Theory of Heredity" which was published in English in 1893. Subsequent works on heredity are "On Germinal Selection" (1896) and "Vorträge über Descendenztheorie" (1902). This last-named work, which was published in English under the title "The Evolution Theory" (1904), consists of a summary and an expansion of many of his previous writings on the subjects of evolution and heredity;

indeed as he says in the preface of this book, it is "a mirror of the course of my own intellectual evolution."

Without attempting to analyze these different books, which would require more time and space than is here available, we may proceed at once to a summary of his more important contributions to the theories of evolution and heredity.

All his theories, both of heredity and evolution, center in what he called the "germ-plasm," that particular part of the germ cells which serves to carry over from generation to generation the inheritance factors. This germ-plasm was held by Weismann to be absolutely *continuous* from the present generation back to the earliest generations of living things; it was absolutely *distinct* from the somatoplasm of the body and the latter could never become germ-plasm; it was almost perfectly *stable* undergoing practically no changes except such as came from the mixing of different kinds of germ-plasm (amphimixis) in sexual reproduction.

These views as to the nature of the germ-plasm underwent some modification as the result of criticism. Weismann was forced to admit that the distinctness and stability of the germ-plasm were not absolute, but in spite of all criticism he was able to maintain that the germ-plasm was relatively very distinct from other plasms and very stable in organization and this is now admitted by all persons acquainted with the subject.

His views as to the separateness of somatoplasm and germ-plasm, of body cells and germ cells, and the mortality of the former and potential immortality of the latter, led him to regard organisms in which this distinction does not exist (many protozoa and protophyta) as potentially immortal. With a keenness of insight which was not appreciated at the time but which has been confirmed by recent work he reasoned that "conjugation like food and oxygen may be conditions of life but immortality does not rest on the magic of conjugation any more than on food or oxygen." Again he anticipated the most recent opinions when he held that death is not a necessary correlative of life, but rather the result of higher differentiation. In short, as Minot said, "Death is the price we pay for our differentiation." On the other hand, his attempt to

explain the origin of death as an adaptation due to selection was probably a mistaken one.

As to the location of the germ-plasm in the sex cells Weismann maintained that it was to be found in the chromatic substance of the nucleus. He held that the chromosomes ("idants") were composed of smaller units, the chromomeres ("ids"), and that the latter were composed of "determinants" or inheritance units, while the most elementary units of life he called "biophores." Both chromosomes and chromomeres are visible structures of the cell. Determinants and biophores are ultra-microscopic in size but recent work on heredity and development has shown that there is good evidence of the existence of such units. All recent work in genetics is based upon the hypothesis that there are units or factors or determiners in germ cells which condition the development of adult characters, and though there may be minor differences between these *determiners* of modern genetics and the *determinants* of Weismann no one can fail to note the genetic connection and the family resemblance between the two.

His prediction on purely *a priori* grounds that one of the maturation divisions in the formation of the egg and sperm should be a "reduction division" whereby the chromosomes of the sex cells should be reduced to half the number present in the somatic cells, whereas all other cell divisions should be "equation divisions" in which the chromosomes should divide equally, was almost as brilliant an example of scientific prophecy as was the prediction of the existence of the planet Neptune.

Similarly Weismann's assumption that the determinants are arranged in a linear series in the chromosomes finds strong support in the newest and most striking discoveries in this field, in which Morgan is able to locate at different points along the length of a chromosome the determiners of many developed characters.

Finally there is at present universal agreement to the declaration of Weismann that no purely epigenetic theory of heredity is possible, though for many years even this was hotly contested. When one recalls the storm of opposition which was called forth by his book on "The Germ-Plasm" the present acceptance, at least

in principle, of his major propositions cannot be viewed in any other light than as a triumph for his theory and a tribute to the insight, foresight and constructive ability of Weismann.

As a result of his theory of heredity Weismann was led to investigate the generally accepted doctrine of the inheritance of acquired characters. He carried on extensive experiments in order to learn whether mutilations of parents through many generations were ever inherited by offspring; he investigated many supposed cases of the inheritance of such characters, and as a result of this work he was led to deny altogether the possibility of the inheritance of acquired characters, and he challenged the world to furnish any satisfactory proof of such inheritance. This work of Weismann's called forth a tremendous amount of discussion and a relatively small amount of direct observation and experiment, and for several years it appeared as if no progress whatever was being made toward the solution of this great question, so full of importance, not merely for the biologist but also for the practical breeder and indeed for the human race. But gradually there has grown up a clearer understanding of the problem and of what is meant by "inherited" and "acquired" characters, and gradually this dead-lock of opinions is breaking up. Now we recognize that inherited characters are those whose distinctive or differential causes are in the germ cells, while acquired characters are those whose differential causes are environmental. No one today believes that the developed or somatic characters of an organism are transmitted to the next generation. Today the problem of the inheritance of acquired characters is merely this: Can changes in the environment change the constitution of the germ-plasm so as to produce changes in subsequent generations? No one now asks whether changes in developed characters may be transmitted to descendants, as was generally done before Weismann's work, for it is generally recognized that somatic characters whether inherited or acquired are not transmitted from generation to generation, the only thing which is transmitted being the germ-plasm. Weismann admitted in his later writings that the germ-plasm might be modified to a limited extent by certain environmental conditions, but he held that such

changes of the germ-plasm led to general and unpredictable changes in future generations which might be wholly different from those somatic changes in the parents which were directly produced by such environment. This view is now widely accepted.

Thus while Weismann's views on this subject underwent certain changes in the course of his long life, the opinions of his opponents have undergone so much greater and more important changes that it may be truly said that in the matter of the inheritance or non-inheritance of acquired characters the greater portion of the scientific world has come to Weismann's position.

Finally mention must be made of Weismann's theory of evolution which was a direct outgrowth of his theory of heredity. He maintained that evolution must depend upon an evolution of the germ-plasm and that this was brought about chiefly, if not entirely, by the mixture of different kinds of germ-plasms (amphimixis) in the union of the sex cells. There is no doubt that many variations are produced by amphimixis but in general these combinations of germ-plasms are not actual fusions; new combinations of inheritance units are produced but not new units, and usually these new combinations split up in subsequent generations according to Mendelian rules, so that such temporary combinations of different germ-plasms do not usually lead to permanent modification, or to evolution, of the germ-plasm. On the other hand it is probable that Weismann underestimated the possible influence of environment in producing changes in the germ-plasm and hence its influence on evolution; at least it does not seem possible at present to explain the origin of many inherited mutations except by the influence of changed environment upon the developing germ cells.

In his belief in Natural Selection Weismann out-Darwined Darwin or any of the Darwinians. Darwin dealt only with the survival of individuals or races in the struggle for existence and was always inclined to assign a good deal of weight to the influence of environment in producing new races. Weismann would not admit the existence of any other factor of evolution than selection and he extended this principle from individuals or persons ("personal selection") to organs and tissues ("histonal selection") and

even to germinal units such as determinants and biophores ("germinal selection"). By means of an assumed struggle for nutriment between different determinants he believed that the weaker ones would tend to grow still weaker and to disappear while the stronger ones would increase in strength until they reached such importance that they were checked, or increased, by personal selection. And by a similar struggle between different biophores he showed that the *quality* of a determinant would be changed. By means of this highly ingenious but purely formal and hypothetical system he was able to explain the degeneration and disappearance of useless parts of an organism and the concordant modification of many different parts in the course of evolution.

Of all his theories those which grew out of his belief in the "Omnipotence of Selection" have found least confirmation in subsequent work. The Mutation Theory of deVries has come in to modify in certain important respects the theory of Darwin, and the work of Johannsen, Jennings, Pearl and others has shown that even "personal selection" has little or no influence in *creating* new types. And yet we have not seen the end of the selection doctrine. The elimination of the unfit is still the only natural means of accounting for fitness in organisms and we may well ponder these words of Weismann in the preface of his last book:

> "Although I may have erred in many single questions which the future will have to determine, in the foundation of my ideas I have certainly not erred. The selection principle controls in fact all categories of life units. It does not create the primary variations but it does determine the paths of development which these follow from beginning to end, and therewith all differentiations, all advances of organization and finally the general course of development of organisms on our earth, for everything in the living world rests on adaptation."

Clear thinking is necessary in the advance of science as well as fine technique and Weismann has demonstrated to a more or less scornful world the importance of brains as well as of hands and eyes in the discovery of truth. It does not fall to the lot of any man to make no mistakes, and in this respect Weismann was only human. But it has fallen to the lot of few men to do so much work of lasting value and to have so profound an influence on his

day and generation as was true of August Weismann. The spirit of his life and work may be summed up in the beautiful words with which he closes his essay on "Life and Death": "After all it is the quest after perfected truth, not its possession, that falls to our lot, that gladdens us, fills up the measure of our life, nay! hallows it."

EDWIN G. CONKLIN.

PRINCETON UNIVERSITY,
January, 1915.

MAGELLANIC PREMIUM

FOUNDED IN 1786 BY JOHN HYACINTH DE MAGELLAN, OF LONDON

1916

THE AMERICAN PHILOSOPHICAL SOCIETY

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ANNOUNCES THAT IN

DECEMBER, 1916

IT WILL AWARD ITS

MAGELLANIC GOLD MEDAL

TO THE AUTHOR OF THE BEST DISCOVERY, OR MOST USEFUL INVENTION, RELATING TO NAVIGATION, ASTRONOMY, OR NATURAL PHILOSOPHY (MERE NATURAL HISTORY ONLY EXCEPTED) UNDER THE FOLLOWING CONDITIONS :

1. The candidate shall, on or before November 1, 1916, deliver free of postage or other charges, his discovery, invention or improvement, addressed to the President of the American Philosophical Society, No. 104 South Fifth Street, Philadelphia, U. S. A., and shall distinguish his performance by some motto, device, or other signature. With his discovery, invention, or improvement, he shall also send a sealed letter containing the same motto, device, or other signature, and subscribed with the real name and place of residence of the author.

2. Persons of any nation, sect or denomination whatever, shall be admitted as candidates for this premium.

3. No discovery, invention or improvement shall be entitled to this premium which hath been already published, or for which the author hath been publicly rewarded elsewhere.

4. The candidate shall communicate his discovery, invention or improvement, either in the English, French, German, or Latin language.

5. A full account of the crowned subject shall be published by the Society, as soon as may be after the adjudication, either in a separate publication, or in the next succeeding volume of their Transactions, or in both.

6. The premium shall consist of an oval plate of solid standard gold of the value of ten guineas, suitably inscribed, with the seal of the Society annexed to the medal by a ribbon.

All correspondence in relation hereto should be addressed

TO THE SECRETARIES OF THE

AMERICAN PHILOSOPHICAL SOCIETY

No. 104 SOUTH FIFTH STREET

PHILADELPHIA, U. S. A.